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## A SURVEY OF SOUTHWESTERN ARCHAEOLOGY<sup>1</sup>

By FRANK H. H. ROBERTS, JR.

SOUTHWESTERN archaeology has long occupied a prominent place in North American anthropological researches, but at no time since investigations were started has there been as widespread an interest or so marked a diversity of effort in the area as that of today. Intensive studies and numerous conferences have produced so much material that it is difficult for those not directly concerned with the field to keep abreast of its developments. Several articles, reports, and books on the Southwest appearing in recent months review the archaeology in an effort to explain the present status of the subject. There is still some misconception, however, about various phases of the problem and a number of features, particularly earlier contributions, have been so consistently overlooked that an additional résumé may not be out of place.

Early Spanish explorers observed and recorded ruins which lay along their routes of travel, but it was not until reports by members of military and survey parties of the westward expanding United States became available that a real interest was aroused in the remains which dot the region. Universities and museums were attracted to the field and private individuals organized expeditions to hunt for "relics." From that time onward there has been an ever increasing zeal on the part of diggers. When the eastern tourists "discovered" the Pueblo country a decade ago the Southwest became archaeologically conscious and began to capitalize its antiquities. Local schools and colleges introduced courses on the subject in their curricula and small societies and roadside museums sprang up all over the area. Most of the investigations in former years were conducted by large institutions located outside the region, but the local organizations are now doing their full share. Last winter and early spring the landscape literally swarmed with "archaeologists" sponsored by the CWA and during the summer the activity continued under the FERA. The results of most of this work are still to be determined, although the general consensus of opinion

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<sup>1</sup> Submitted by permission of the Secretary of the Smithsonian Institution.



is that, with a few exceptions, it was unfortunate that "relief employment" was directed into that particular channel.

Hundreds of articles and reports have been written on Southwestern archaeology. Many of the papers are excellent, others indifferent, and some should never have attained to the dignity of print. On the other hand much work has been done which was never reported and, unfortunately, some of the most important excavations ever carried on fall in this category. From a broad point of view the published data fall into three main classes: graphic accounts of the superficial features of greater and lesser antiquities; detailed studies of buildings and objects found, with considerable emphasis on the function and symbolism of the latter; and comprehensive treatises on specifically planned investigations in an attempt to fit the data into their proper position in the historical pattern and to show what part they played in the course of cultural development in the area. The style of report correlates roughly with the series of years in which the work was done. The first belongs to the era of exploration, 1850 to 1880; the second to the interval of promiscuous digging with specimens as the chief incentive, 1880 to 1910; and the last to the period of excavations carefully planned with a view to solving recognized problems, 1910 to the present. This grouping may be criticized on the grounds that a few of the earlier men did endeavor to see the picture as a whole, while some now engaged in researches seemingly do not realize that there is more to the problem than their own little project. But, taken by and large, the three-phase classification does indicate what the trends have been.

That all of the ruins were not contemporaneous was suggested by various factors. Yet, although there was a broad classification of modern and pre-Spanish remains, little attempt was made to determine sequential distinctions between sites until about 1910. Prevailing opinion was that no such differences could be ascertained for the pre-Spanish group. This belief was strengthened by the unsatisfactory results which most of the workers obtained when they endeavored to develop a sequence on the basis of legendary evidence, by comparisons between artifacts, and by the state of preservation of the ruins. For some reason stratigraphy was largely disregarded despite the fact that it had long proved extremely useful in Old World archaeology. Not a few investigators held, and students were even taught, that there could be no stratigraphy in the Southwest because the remains were only those of a single people, the Indian.

Stratigraphy was recognized in a few sporadic cases as indicating relative dates for material, but it was not until the present phase of Southwestern researches that it received due consideration as an important

source of evidence. N. C. Nelson of the American Museum of Natural History demonstrated the validity of the method when, beginning in 1912, he used it in New Mexico. He, as well as other field men, had recognized variations in the kinds and styles of pottery associated with ruins and village sites and believed that these differences had definite significance beyond that of being characteristic of the places where they were found. Accordingly he chose a number of ruins known to be inhabited pueblos during the early Spanish occupation. By working downwards from top to bottom in the adjacent refuse heaps he determined the sequence of the principal pottery types of the Rio Grande region and in consequence the main chronological periods for the district.<sup>2</sup> At about the same time Kidder and Guernsey were using stratigraphy to establish the relative ages of several types of remains in the Kayenta district of northeastern Arizona. Subsequent projects, Morris at Aztec, Hodge at Hawikuh, Kidder at Pecos, Judd in the Chaco Canyon, were conducted with full consideration of the importance of this kind of evidence. Since that time stratigraphy has become one of the accepted routines in the technique of excavation.

To aid him in the study of his material Mr Nelson developed a system of tabulations and percentages which not only showed fluctuations in the pottery from a single site, but which proved of value in making comparisons between the types found at various ruins. Kidder employed an adaptation of the method at Pecos,<sup>3</sup> Kroeber used it successfully in the Zuñi region,<sup>4</sup> and Spier obtained excellent results in a survey of the Zuñi and Little Colorado districts by following an elaborated form of it.<sup>5</sup> Briefly stated, the technique makes possible a relative dating of sites on the basis of the percentages of the different kinds of pottery found at each, provided the ceramic sequence has previously been determined by stratigraphy. The method was used for some time with good success by workers in the Southwest. Recently, however, it has fallen into the discard. Just why this should be the case is not apparent. It is true that under certain conditions it is not an infallible source of evidence, particularly in chronological studies based solely on surface material. Nevertheless it is helpful in outlining the main characteristics of a district and in indicating where intensive work should be undertaken. In a study of the ceramics of a single site it has more than enough merit to warrant its retention in archaeological procedure. It graphically demonstrates the true nature of the pottery com-

<sup>2</sup> Nelson, 1916.

<sup>3</sup> Kidder, 1931. (See also *American Anthropologist*, Vol. 19, pp. 325-60.)

<sup>4</sup> Kroeber, 1916.

<sup>5</sup> Spier, 1917, 1918.

plex. One explanation for the failure to make use of the system is, perhaps, that the workers have become so absorbed in a detailed study of pottery *per se* that they have forgotten the important factor of giving percentages. It is only from such data that the real significance of each group in the series can be judged.

Accumulated data had demonstrated that there were regional variations, that characteristic cultural elements tended to conform to distinct patterns or styles according to the district in which they were found. It was also observed that the stylistic complexes seemed to radiate from particular centers and that they mingled or overlapped along the hazy boundary lines separating the numerous spheres of influence. Moreover, it was definitely established that there were a number of different stages in the unfolding of the culture. Although writers described these features, little attempt was made to combine the knowledge into a coherent whole until Nelson undertook a chronological study of the entire area. He had drawn up a diagrammatic chart to illustrate his conception of the relations between the various groups, as well as their origins, but had not completed his work when his efforts were diverted to other fields.<sup>6</sup> Nelson's outline broadened the viewpoint to a considerable degree, but it was not until Kidder published his "Introduction to Southwestern Archaeology" in 1924 that the possibilities for revealing a vivid and fascinating narrative of culture growth were fully appreciated. Kidder not only assembled, digested, condensed, and made available the salient facts of existing data, he went further and correlated the mass of information into an historical reconstruction presenting for the first time a comprehensive postulation of developments in the area. The book had an even greater value, however. It pointed out blank spots in the record, indicated districts where investigations were needed, and centered attention on a number of general problems.

Within the last decade a new method making possible absolute rather than relative dating became available. This contribution came from an astronomer. Dr A. E. Douglass of the University of Arizona, in studying sun spots and their effects on climatic conditions, turned to the growth rings of trees in an effort to obtain evidence on the occurrence of drought periods and the intervals of moisture. In doing this he discovered that definite ring patterns recorded specific year groups and as a consequence developed a system whereby he can tell the year when a log was cut from a living tree. Beginning with trees whose actual cutting date was known he

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<sup>6</sup> Nelson, 1919, 119.

has been able to devise a type ring chart going back to about 700 A.D.<sup>7</sup> In securing evidence to substantiate his own theories he was forced to resort to timbers from ruins for material antedating living trees and thus furnished the archaeologists with a valuable time scale. When beams are found in ruins it is possible to check their rings against the type chart and, provided the outer surfaces have not been damaged or removed, tell the year of their cutting. The timber may not have been placed in the structure immediately after it was cut and an occasional log was reused, but such factors can be checked by the archaeological aspects of the site and a date is assured which approximates closely the year or years when the dwellings were erected.

Continued work with the tree-ring dating system, or dendrochronology as it is now called, demonstrated that the type chart would not function in all cases. Material from some sections did not correlate properly because of local variations in characteristic ring patterns. For this reason it has been necessary to develop supplementary charts. The work is being carried on by a number of Doctor Douglass' students and newly dated ruins are constantly being added to an already sizeable list. Rivalry between the workers to find earlier and earlier dates has in one or two instances caused misunderstandings. Proper use of the system requires that the announced date should be that of the outermost ring in the timber. Occasionally the date of the earliest discernible ring has been proclaimed in such an ambiguous way that the implication was that that was the year when the log was cut and the building erected. One such case led to numerous articles attributing some 200 years greater antiquity to one major ruin than it actually possesses. This not only created an erroneous idea about the age of the site, but caused considerable confusion since nearby structures believed to be contemporaneous, on the basis of archaeological evidence, had yielded much later dates. Peculiar inconsistencies in some recent identifications and interpretations of material have led a number of those specializing in dendrochronology to the formulation of an agreement that each date and the specimen on which it is based be examined and approved by Doctor Douglass before it is released for publication. Great care should unquestionably be taken because the results so definitely fix the chronological position of a ruin that an inaccuracy might disrupt the sequence for an entire district. A gratifying feature about dendrochronology is that its results have checked with the findings from other sources of evidence. The relative ages of numerous large ruins and village sites, even of remains in

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<sup>7</sup> Douglass, 1929, 1932

different districts, had been worked out by archaeological methods and when tree-ring dates became available it was noted that the previous conclusions were in agreement, although the estimated time lapses had been much too great.

As a result of the stimulation of interest produced by Kidder's book, the entrance of new workers into the field, an increase in published material, and growing confusion in the correlation of information, it became apparent that something should be done to improve the situation. Accordingly, Doctor Kidder invited workers in Southwestern archaeology and related fields to an informal conference at the excavation camp at Pecos Ruins, New Mexico, August 29-31, 1927. The three days of discussions led to an agreement on a series of sequent stages in the culture growth and a set of names designating the several phases, the Pecos Classification, were adopted. This conference was so satisfactory to the workers that most of them met at Pecos again in the summer of 1929. The sessions of the second gathering were devoted mainly to a review of the original classification and to reports on excavations conducted subsequent to the first conference.

Most of those attending the second conference expressed the belief that the classification had been of help to them in their studies. Some stated that they had difficulty in applying the criteria. This was especially true of one definite region. Consideration of this perplexity served to emphasize a fact which had been becoming more and more apparent. Namely, the remains in the southern and western portion of the area, the desert domain, are not Puebloan in type. Cosmos Mindeleff commented on this in 1896 and suggested that the differences were too marked to be attributed wholly to a question of environment.<sup>8</sup> Kidder in 1915 separated Southwestern culture into two major divisions on the strength of the dissimilarities<sup>9</sup> and he again pointed them out in 1924. In the latter publication, however, with pottery as the criterion, he concluded that in some respects these aberrant sites were allied to the Pueblo ruins.<sup>10</sup> Nelson had recognized the distinction and in 1919 indicated it on his diagrammatic chart, although he did not give a detailed discussion of the problem. The situation was not accorded the attention which it merited, actually was overlooked at the first conference, until Gladwin and others, beginning in 1927 and continuing through subsequent years, secured definite evidence that the types are different. The

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<sup>8</sup> Mindeleff, 1896, 186-87.

<sup>9</sup> Kidder, 1917.

<sup>10</sup> Kidder, 1924, 105-6, 107.

full import of this did not crystallize at Pecos but at Gila Pueblo, Globe, Arizona, in April 1931, when a classification was drawn up for that division. The results of the Gila Pueblo conference were presented to a larger group of Southwestern workers at the Laboratory of Anthropology, Santa Fé, New Mexico, in September of that year. The Santa Fé session, which took the place of the biennial Pecos conference in 1931, discussed and adopted the Globe recommendations. There have been no general meetings of that nature since.

From the knowledge secured during the many years of investigations and on the basis of understandings reached in various conferences, most Southwestern archaeologists today synthesize the data broadly and briefly as follows. Scattered over the area are the remains of a basic sedentary, agricultural, pottery-making culture which has two major provinces comprising the plateau and desert patterns. The plateau division, which falls under the Pecos Classification, includes the regions of the San Juan, the Rio Grande, the Upper Gila and Salt, the Little Colorado, most of Utah and a portion of eastern Nevada. The desert domain, summed up by the Globe Classification, occupies the territory extending from the Colorado River on the west to the Rio Grande on the east; from Flagstaff, Arizona, on the north to northern Sonora on the south; with its center lying in the middle Gila basin. The northern boundary follows roughly the 35th parallel from the Colorado, swinging slightly north to include the Flagstaff section, thence southeastward across Arizona conforming approximately to the great diagonal ridge sometimes called the "Mogollon Rim" or the "Verde Breaks," continues along the Gila Mountains and swings eastward into New Mexico. There is, of course, an overlapping of the patterns in the border precincts, but only in late phases any indication of fusion. In only one district, the Verde Valley, do the two appear to have coalesced to form a sub-pattern.

The plateau group is designated by the long familiar names, Basket Maker-Pueblo, while the desert dwellers have been termed the Hohokam, a word used by the Pima when they make reference to the ancient ones. Russell employed it in an archaeological sense when he referred to the antiquities of the region in his monograph on the Pima. It was not adopted or generally used, however, until after the meeting at Gila Pueblo. The Hohokam for a number of years went under the working designation of the Red-on-Buff culture because of the color characteristics of its pottery. In considering the two major divisions the Basket Maker-Pueblo with its so-called Pecos Classification will be discussed first.

## BASKET MAKER-PUEBLO

The uplands pattern is recognized as representing a cultural unit with several horizons in its development. The general view is that agriculture, introduced from the south, was taken up by a nomadic people whose newly acquired art led to a more settled life. At a later date pottery making was either introduced or invented and houses of the pit type were perfected. This was accompanied by changes in existing elements in the material culture and the appearance of other features. A new racial strain then invaded the region, dwellings were built above ground and evolved into many-roomed structures. With the infusion of new blood there was an acceleration in the unfolding of the cultural pattern. Small villages were scattered over a greater part of the province. Then there was a contraction in the extent of occupied territory and a concentration of population into definite centers. This phenomenon was accompanied by improvements in architecture and the ceramic arts, together with marked local specialization. Following this there was an even greater shrinkage in occupied territory, a shift to new localities, and a decline from the preceding cultural peak. This trend was terminated by the arrival of the Spaniards and subsequent colonization by other white men.

The first Pecos Conference grouped the various horizons under two main headings, the Basket Maker and the Pueblo, which were further divided. Hence the Basket Maker I, or Early Basket Maker; Basket Maker II, or Basket Maker; Basket Maker III, Late Basket Maker or Post-Basket Maker; Pueblo I, or proto-Pueblo; Pueblo II; Pueblo III, or Great Period; Pueblo IV, or proto-Historic; Pueblo V, or Historic. These eight stages in the general pattern were based on several diagnostic traits. For the two major groups skeletal material is significant. In the material culture the following elements were believed to be indicative of the various stages: village types, architecture, sandals, pictographs, textiles, stone and bone implements, kinds and styles of ornaments, and pottery. Pottery, it was agreed, furnished the most abundant, convenient and reliable criterion, and the culinary vessels the simplest ware for chronological determinations. Primarily the classification rests on ceramics. One explanation for this is that pottery (potsherds) is readily obtainable without the expenditure of much effort or money. Also, it is characterized by easily noted differences in style and form and it was an exceedingly sensitive element from the standpoint of variations both in time and place. When the first conference was held only a little was known about the houses and still less about other factors for some of the early stages. The original summarized classification

has been so widely published that it need not be repeated in detail.<sup>11</sup>

For the benefit of those not thoroughly familiar with the subject a brief consideration of certain elements in each horizon may help to an understanding of the sequence. This summary includes not only the material available when the nomenclature was adopted but data obtained since 1927 as well. Discussion of all of the components of the complex for each subdivision is beyond the scope of the present article so only a few traits will be described. Since Basket Maker I is postulated there is little to be said concerning it. Theoretically it was a non-agricultural stage possessing in cruder, less developed form some of the elements present in later levels but actually no traces of it have ever been found. When the first tabulation was made recent finds suggested that such had been discovered. Subsequent study of the material, however, indicated that it could not be considered in that light. A number of the discoveries in recent years which indicate human occupancy of the area at a comparatively remote date can not be considered, on present evidence, as representing the initial stage of the classification. These finds, Folsom,<sup>12</sup> Gypsum Cave,<sup>13</sup> etc., thus far have not been shown to bear any relationship to the Basket Maker. The most significant factor in this connection was the discovery by E. B. Howard of a Folsom type point in a level underlying a Basket Maker horizon.<sup>14</sup>

#### CRANIA

Basket Maker II, undeformed, long scaphoid. Basket Maker III, undeformed, long scaphoid; undeformed round (occasional in late sites). Pueblo I, deformed, both long and round. Pueblo II, deformed, round in the majority; an occasional long. Pueblo III, deformed, preponderantly round; sporadic long. Pueblo IV, deformed, mostly round; few long. Pueblo V, deformed, round and long. Undeformed round; occasional undeformed long.

#### SANDALS<sup>15</sup>

Basket Maker II, square toe with fringe, twined-woven of fine cord. Basket Maker III, scalloped toe, woven of fine string, design in color on upper side, woven pattern on under. Pueblo I, round toe, woven of fine string, coarse pattern on under side. Pueblo II, round toe (?). Pueblo III, notched toe, woven of fine string and yucca leaf; square toe, yucca leaf, twilled weave. Pueblo IV, notched toe, string and yucca leaf. Pueblo V, moccasins.

<sup>11</sup> Kidder, 1927; 1931, 5-6, Roberts, 1929, 3-7.

<sup>12</sup> Cook, 1927, 1928.

<sup>13</sup> Harrington, 1933.

<sup>14</sup> Howard, 1932.

<sup>15</sup> Guernsey and Kidder, 1921; Guernsey, 1931.



BASKETRY<sup>16</sup>

Basket Maker II, loose-weave, coiled, rod and bundle type; decorated in black or red. Basket Maker III, coiled rod and bundle type, no difference either in technique or appearance from Basket Maker II; occasional irregular splitting of stitches. Pueblo I, coiled rod and bundle, elaborate designs; twilled-ring baskets. Pueblo II, twilled-ring baskets; general lack of information, however. Pueblo III, occasional coiled, rod and bundle with fine tight weave but no design; twilled-ring baskets numerous. Pueblo IV, same as for III, wickerwork appears. Pueblo V, baskets of plaited yucca leaves attached to wooden rims; coiled rod and bundle baskets and trays; wickerwork baskets.

TEXTILES<sup>17</sup>

Basket Maker II, twined-woven bags with designs in color, finely woven from apocynum-fiber string; coiled-netted weave of human-hair string. Basket Maker III, twined-woven bags of coarse weave with no design; coiled-netted weave of coarse-fiber string. Pueblo I, cotton cloth. Pueblo II, cotton cloth. Pueblo III, cotton cloth of plain loom weave; elaborately decorated loom weave; netted weave. Pueblo IV, same as for III. Pueblo V, cotton, wool, commercial items purchased from the traders.

WEAPONS<sup>18</sup>

Basket Maker II, atlatl, grooved clubs. Basket Maker III, atlatl, grooved clubs, bow and arrow towards end of horizon. Pueblo I, bow and arrow. Pueblo II, bow and arrow. Pueblo III, bow and arrow, throwing club. Pueblo IV, bow and arrow, throwing club. Pueblo V, bow and arrow, throwing clubs, European weapons.

## HOUSES

Basket Maker II, no information; possibly erected temporary shelters in the open. Dug into floors of caves are circular or oval pits, in many cases lined with slabs of stone, which constituted lower portions of granaries. Now and then examples are found with pole, brush and plaster superstructures still in position over pit. Occasionally these cists were lined with bark and grass and seem to have functioned as sleeping places.<sup>19</sup>

Basket Maker III, dwellings of the circular, oval or rectangular pit variety. Excavations lined with upright stone slabs or heavy coating of mud plaster or both, sometimes a wainscoting of poles was used in place of stone. Roofed over with a conical or truncated superstructure of poles covered with mats or brush, plaster

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<sup>16</sup> Guernsey and Kidder, 1921; Guernsey, 1931; Weltfish, 1932.

<sup>17</sup> Guernsey and Kidder, 1921; Kidder and Guernsey, 1919; Guernsey, 1931.

<sup>18</sup> Guernsey and Kidder, 1921; Guernsey, 1931.

<sup>19</sup> Guernsey and Kidder, 1921; Nusbaum, 1922.

and earth. Central smoke hole, side entrance passage, occasional ante-chamber. Granaries of Basket Maker II form clustered about houses. Number of such dwellings irregularly grouped together to form a village.<sup>20</sup>

Pueblo I, characterized by transitions in house types, variety of structures. In the north central section of the province, the portion traversed by the San Juan and its tributaries, the crude, single-roomed semisubterranean dwellings gave way to structures which had only slightly depressed floors instead of pits. Major portion of house was above ground. Had several contiguous rooms. Pole and plaster form of construction, jacal walls, prevailed at first but was replaced in time by masonry. Pit domiciles continued in use in peripheral districts, especially in the south and west. Pits were dug deeper, however, and entrance to chambers was by means of a ladder through the smoke hole. In a few precincts the side entrance survived. Where entrance was through the roof the former side passage was retained in reduced size as a ventilator. Above ground villages retained a subterranean structure as a ceremonial house, the *kiva*.<sup>21</sup>

Pueblo II, unit-type structures or one-clan houses. These dwellings of stone or adobe, built entirely above ground, contained from six to fourteen rooms. They were a single story in height with rooms grouped in one long row, a double tier, an L-shape, or in the form of a rectangular U. Usually at the south or southeast side, detached from the building, was a subterranean ceremonial chamber.<sup>22</sup> In peripheral parts of the area this type of dwelling did not reach as high a degree of excellence. Pole and mud houses and irregular agglomerations of rooms whose walls were formed from large quantities of adobe mud and unworked bowlders prevailed in the south and west. In the Flagstaff district rectangular pit dwellings survived through this horizon.<sup>23</sup>

Pueblo III, the great terraced communal houses of many rooms; mostly of stone construction although occasionally made of adobe. Erected either in the open or in large natural caverns in the cliffs. Also, one-clan houses scattered about in the vicinity of the large centers. In some sections cavate dwellings, rooms cut into the soft tufa of cliff faces, were not uncommon.<sup>24</sup>

Pueblo IV, communal houses, scattered dwellings, cavate lodges.<sup>25</sup>

Pueblo V, villages of terraced houses, of one-storied single-family houses, scattered single-family dwellings. Numerous examples of this stage are known to the general public, Taos, Zuñi, Acoma, and the Hopi towns especially.

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<sup>20</sup> Guernsey, 1931, 25-27; Roberts, 1929, 10-105.

<sup>21</sup> Kidder, 1924, 74-75; Morris, 1919, 182-95; Roberts, 1930, 19-73; 1931, 15-90.

<sup>22</sup> Prudden, 1903. These structures illustrate the form but are Pueblo III in horizon. A number of examples have been excavated in the Chaco range but data are unpublished.

<sup>23</sup> Colton and Hargrave, 1933.

<sup>24</sup> The works of Fewkes, Pepper, Mindeleff, Hough, and numerous others illustrate this horizon. See bibliography in Kidder, 1924; also citations in Roberts, 1932, 17-19.

<sup>25</sup> Roberts, 1932, 20-21, for examples.

## POTTERY

Basket Maker II, no true pottery but large containers of unfired clay tempered with cedar bast, the chaff of corn tassels or grass heads. Moulded in baskets or built up of clay, the beginning of the coiled technique.<sup>26</sup>

Basket Maker III, fired vessels. Light gray to a fairly good white in color; red containers; bowls with an unpolished black interior and gray exterior. Surfaces irregularly stippled in appearance due to protruding particles of tempering material. Sand or crushed rock temper, paste granular in cross section. Red ware due to an intentional overfiring, rarely to colored slip. Bowls usually decorated on interior, other vessels unornamented. Designs generally ribbon-like panels embellished with dots, zigzag and stepped-line elements, occasional life-form figures. Decorations carried over from basketry to pottery. Most vessels treated, after firing, with a wash of red pigment. This is impermanent and has been called "fugitive red." Culinary vessels smooth on the exterior.<sup>27</sup>

Pueblo I, plain gray, black on white, lustrous black on red, slightly polished black interior bowls with brownish exterior. Introduction of slip. Tempering of white sand, ground rock, or pulverized potsherds. Decorations on all types of vessels. Main design elements consist of zigzag, parallel, parallel-stepped lines, and wavy lines; filled triangles and dotted triangles; volutes and ticked volutes; interlocking frets, checkerboard; concentric rectilinear and curvilinear figures. Patterns taken from textiles in addition to baskets. Culinary vessels with corrugated necks, flat neck bands, and smooth bottoms. Period marked by great diversity in form. In the black on white ware there are two main groups, the eastern and the western. Of course there are many local minor variations but for a general consideration the two main forms are sufficient. The eastern centered about the Chaco Canyon area and the western around the Kayenta district in northeastern Arizona. The eastern extends from the northeastern San Juan basin in southern Colorado to the Upper Gila region in southern New Mexico, from the Rio Grande on the east to approximately the Arizona-New Mexico boundary line on the west. In the west its southern fringes penetrated somewhat into Arizona. The western Pueblo I ranged from northeastern Arizona to the Little Colorado in the eastern part of the state, swung a bit south of that stream farther west, and continued across to southeastern Nevada. The eastern borders are not sharply defined and there is a strip extending down the Arizona-New Mexico line where the two phases overlap. The western was the first to be recognized and for a long time was thought to be the characteristic form. Later investigations in the Chaco Canyon and the northeastern San Juan region established the second and what appears to be the most widespread division. The basic difference is twofold, pigment used in the decoration and surface treatment. The eastern phase had an iron-carbon paint, the western carbon. The eastern group applied the pigment after the surface of the vessel was polished. The western

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<sup>26</sup> Morris, 1927, 138-60

<sup>27</sup> Morris, 1927, 161-98; Roberts, 1929, 107-26.

painted the design before the polishing process was completed, hence it seems to fade into the surface of the vessel. In the eastern form it stands out from the slip. There is no difficulty in telling one from the other or in recognizing either as Pueblo I because the general style of decoration is the same for both.<sup>28</sup> The culinary vessels, red and blackened interior bowls, are the same in the two divisions.

Pueblo II, gray ware, black on white, lustrous black on red, polished black interior bowls with reddish exterior. Ground rock tempering, some sand, powdered potsherds. Decorations on all kinds of vessels. Painted designs characterized by broad, heavy elements; some survival of Pueblo I features but without series of bordering parallel lines. Culinary vessels with indented corrugations on necks, smooth bottoms, or plain corrugation over entire surface. Indented corrugation large and coarse, frequently called "exuberant." Simple form of design pinched into corrugation or incised with finger nail or implement. The beginning of spiral coil. In previous stages each loop of clay made only a single circuit while in Pueblo II longer fillets were employed and each made several turns around the wall of the vessel.<sup>29</sup>

Pueblo III, gray ware, black on white, polychrome, black interior and red exterior. Late in period the beginning of black on yellow, black on orange. Fine texture, potsherd tempering as a rule. Designs characterized by elaborate detail and fineness of execution. The era of marked specialization. Pottery of the various districts so typical that its place of origin may be recognized immediately, whether Mimbres, Chaco Canyon, Kayenta, etc. Culinary vessels covered over entire exterior surface with finely indented corrugation. Continuous spiral coil in manufacture.<sup>30</sup>

Pueblo IV, plain gray, plain yellowish, black on white, black on red, black on yellow, black on orange, polychrome, and glazed wares. Sand and potsherd tempering. Elaborate designs, solid heavy elements. Breakdown in corrugation on culinary vessels, beginning of return to smooth surfaced cooking pots.<sup>31</sup>

Pueblo V, modern painted wares of the Pueblos. Smooth surfaced culinary vessels.<sup>32</sup>

#### OTHER TRAITS

There are a number of traits which are more or less distinctive of one period or occur in several, but which are not continuous throughout the pattern. Basket

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<sup>28</sup> Kidder, 1924, 74-76, Western (called pre-Pueblo); Guernsey, 1931, pls. 59, 60, 61, Western; Roberts, 1930, 74-139, 1931, 114-49, Eastern.

<sup>29</sup> Guernsey, 1931, pls. 42, 43, 66; Hargrave, 1932, 12, Coconino gray; 14, Deadman's corrugated; 15, Deadman's black on white.

<sup>30</sup> Kidder, 1924, 51-74, Cosgrove, 1932; Hargrave, 1932; Roberts, 1932, 18-19 for additional references.

<sup>31</sup> Kidder, 1924, 86-87, 1931; Hargrave, 1932. Roberts, 1932, 20-21 for additional references.

<sup>32</sup> Bunzel, 1929; Kidder, 1931, 131-50.

Maker II has tree-shell trowels or characteristic wooden scoops, peculiar lozenge-shaped beads, button hole stitch on selvage of plain-weave cloth. Basket Maker III a cross-stick spindle and a unique type of small globular pot with lateral spout. Basket Maker II-III have small funnel or nipple-shaped unfired clay objects either plain or decorated with a punctate design, also clay figurines usually representing human females.<sup>32a</sup> Basket Maker III and Pueblo I-II have the open end trough metate placed on the floor, Pueblo III-V flat metates set in bins. Pueblo I-V have the domesticated turkey and the polished grooved ax. Pueblo IV has pottery with the designs in glaze. There is always the possibility that something from an earlier stage will appear in one of the later horizons. This may be a continuance, a revival of an older form, or an actual survival of one or more objects. Even among the Indians there are and were devotees of the "antique" and the archaeologist occasionally stumbles upon a choice lot of objects which belonged to such a person. It should be evident that allowances must be made for occurrences of this kind but, as is so often the case, the obvious is so frequently overlooked that attention needs be called constantly to the fact that archaeologically "once a thing has been it will be again and again."

As an illustration of the time element involved and in response to oft repeated queries concerning the age of ruins, a number of writers have supplied dates for the various stages in the sequence. These were not an integral part of the Pecos Classification, with the exception of Pueblo V, and were not given with the idea of isolating each stage between definite sets of years, because there is no sharp break between periods. Insofar as possible these dates were based on information furnished by dendrochronology. For earlier stages, however, data from this source were not available and the figures were speculative. Most reports stressed this factor and pointed out that there could be no hard and fast application of the numerical chronology. A tendency has developed in certain quarters to make these dates the horizon determinant and to ignore the elements in the complex. A bare numerical tabulation is not sufficient to make clear all of the ramifications of peripheral lags and stage survivals.

There are two peripheral precincts where the Basket Maker-Pueblo pattern is not clear cut. In these outlying reaches many features of the nuclear districts are missing. On the other hand local developments have contributed elements foreign to the central portions. These marginal regions are generally designated as the Northern and Eastern peripheries. The Northern comprises the territory north and west of the Colorado River, ranging along the western slopes of the Rocky Mountains into southern Idaho and extending westward into eastern Nevada. The Eastern includes

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<sup>32a</sup> Morris, 1927, 154-58.

the territory lying to the east of the Rio Grande drainage and extends from the Oklahoma panhandle on the north through western Texas to the Big Bend district on the south. The western and eastern boundaries of the two peripheries, respectively, have not been determined.

The Northern Periphery is characterized by a progressive fading of the basic pattern in proportion to the distance from the central portions of the province. The general nature of the remains indicates a Basket Maker III-Pueblo I origin for a complex which has distinctive qualities resulting from a combination of factors. Among these may be noted the survival of early elements, varying rates of diffusion for important features in the main pattern, the synchronous appearance of components which were chronologically distinct in the nuclear districts, the adaptation of borrowed features to local needs, and inventions. Except for a narrow strip along the Colorado River in the southern part of the periphery where the pattern was closely allied to that of northern Arizona, the grooved ax, the grooved maul, sandals, the domesticated turkey, cotton, and various pottery forms are missing from the complex. Local features rare or absent in the central phases are a peculiar type of moccasin called the Fremont, unbaked-clay figurines in later horizons, the Utah type metate, katchina-like petroglyphs, and pottery ornamented with certain kinds of appliqué decorations. In the outer fringes of the periphery the Basket Maker-Pueblo pattern came to an end, due in large part to pressure from nomadic peoples, at approximately the termination of Pueblo II in the main part of the province. Along the Colorado River it continued well into the Pueblo III horizon.<sup>33</sup>

The general features of the Eastern Periphery, except for the Pecos district, are not as well known as those of the Northern Periphery. The Pecos ruins, located on the headwaters of the Pecos River, represent the largest eastern outpost of the Pueblo country and, while topographically not of the Rio Grande group, are so considered because of their obvious relationship to the remains of the Santa Fé region. The Pecos ruins proper, as well as the smaller sites in the vicinity, have been extensively studied by Kidder and his associates, and considerable data are available on them. The presence of ruins farther east from the Pueblo country has been known since the days of Bandelier in the late 'eighties, yet little attention was paid to them until the last few years. Eastward from the Rio Grande drainage small sites with black on white pottery occur almost to the Texas border. Along the Cimarron in Oklahoma are caves from which material

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<sup>33</sup> Steward, 1933.

suggestive of the Basket Makers has come.<sup>34</sup> Basket Maker finds have been made in the Guadalupe Mountains in southeastern New Mexico,<sup>35</sup> and caves in the Big Bend district of western Texas have yielded elements comparable in some respects to Basket Maker.<sup>36</sup> In the Canadian River district of eastern New Mexico and western Texas are the remains of villages which until recently were considered the eastern frontier of the Pueblos. The houses were of stone construction and varied in size from single-roomed circular or rectangular structures to large buildings with numerous chambers of varying sizes and shapes. Because of the crude nature of potsherds found at the small sites they have been identified frequently as Basket Maker III or Pueblo I. As a matter of fact the pottery is of the Plains type and the occasional Pueblo fragment found is intrusive. The larger ruins have yielded Pueblo potsherds indicating a Pueblo IV horizon. General consensus of opinion is that these sites represent the western fringes of an eastern cultural pattern which borrowed Pueblo architecture.<sup>37</sup> On the whole the Pueblo remains of the Eastern Periphery probably do not antedate Pueblo III of the nuclear districts nor postdate the first part of Pueblo IV.

#### THE HOHOKAM

The Hohokam or desert province is not as well known as that of the Basket Maker-Pueblo because intensive work in the remains of that division is only just beginning. Efforts of investigators have produced good results in the last five years and considerable information is now available, but as yet there is nothing comparable to the mass of data on the uplands province. From what has been learned it is apparent that the desert pattern represents a cultural unit with several developmental stages. Contrary to the Basket Maker-Pueblo, which is considered largely indigenous in its growth, the Hohokam is thought to have entered the Southwest as an already established pattern although it continued to evolve in its new locale. The earliest stage is characterized by a widespread distribution of small villages situated in the broad, semi-arid valleys of the province. This was followed by a horizon in which there was a greater concentration and a withdrawal from the more outlying precincts. Then there was an invasion of peoples from the uplands and typical pueblos were built in Hohokam communities. The two peoples lived together, it seems, yet kept their cultural patterns distinct. The association was apparently of insufficient

<sup>34</sup> Renaud, 1930.

<sup>35</sup> Howard, 1932

<sup>36</sup> Setzler, 1933.

<sup>37</sup> Holden, 1932.

duration for a borrowing or hybridization of characteristics. The northern people then withdrew from the area while the Hohokam continued to occupy their long established hearths. Comparative studies between dated sites attributable to the group which penetrated the desert domain and then withdrew, and materials which they left in the Hohokam province place the movements between 1300-1350 and 1400-1450 A.D.<sup>38</sup> It is postulated that the Hohokam eventually evolved into the Pima and Papago, although this is still a moot question.

General characteristics of the Hohokam are: dwellings of the single unit type, rectangular in form; agriculture dependent upon extensive irrigation systems; paddle and anvil pottery; cremation of the dead; head form believed to be long and undeformed (this point doubtful because of cremations). The refinement of the pattern has been grouped under five horizons roughly synchronous with the five Pueblo stages. As a matter of fact six levels might be listed because the students of the Hohokam postulate a migratory or pioneer period preceding the first of their defined stages, although no traces of it have been found. As in the case of the Pecos sequence, the Globe Classification rests primarily on pottery. The nomenclature lists the stages as the Colonial, the Sedentary, the Classic, the Recent (given as Degenerate in some reports, but no longer so called), and the Modern. In making a brief summary of the various stages only a few elements in the complex will be considered.

#### DISPOSAL OF THE DEAD

Colonial, pit cremation. Sedentary, urn cremation. Classic, urn cremation, inhumation (Pueblo). Recent, cremation. Modern, inhumation.

#### HOUSES

Colonial, rectangular, shallow pit, vestibule entrance on side. In some cases floor raised above bottom of pit on stone posts. Walls of poles, brush, and mud plaster. Each dwelling a unit in the village.<sup>39</sup> Sedentary: rectangular pit houses, rectangular surface houses with a framework of poles, covered with grass and daubed with mud. Villages enclosed in a compound wall.<sup>40</sup> Classic: pit houses, one story surface houses of poles, grass and mud; multi-storied communal buildings often referred to as temples, fortresses, or clan castles (Casa Grande) but which were essentially pueblos. The compound wall continued in use. Recent: pole, brush and clay houses and in some sections a combination of compound and pueblos. Modern: pole, brush and clay dwellings.

<sup>38</sup> Information supplied by Gladwin.

<sup>39</sup> Haury, 1932.

<sup>40</sup> Woodward, 1931.



## POTTERY

Colonial: red on buff and plain brown wares. The decorated vessels have a buff base color with designs in red. Vessels are bowls, jars, plates, effigies. Distinguishing features for the Colonial period are a typical bowl shape, like an inverted bell with a wide flaring rim, and the nature of the decorations. Most designs were formed by the repeated use of small elements bordered or fringed on one or both sides by sets of short, oblique, parallel lines. Common elements are figures resembling a simple or crude swastika, the letter z, the letter x, number 3; naturalistic symbols such as birds, mammal, reptile, and human forms; solid figures, triangles, rectangles, trapezoids, circles (all of the latter often enclosing a small element); interlocking scrolls applied in narrow bands. The kind of pottery which identifies the Colonial period has been named Santa Cruz red on buff. The brown vessels are called Gila plain ware.<sup>41</sup>

Sedentary: painted pottery a clear buff base color with designs in red, a plain red ware with black interior, a terra cotta red with black interior. No decorated bowls identified with this period, style changed to bowls of terra cotta red with black interior. Painted vessels mainly jars and dippers. Jars large with sharply returned and flattened rim. The area of greatest diameter well below the center of the jar producing a sharp angle, the Gila shoulder, and giving the effect of a flat-bottomed, although actually rounded, vessel. Designs composed of panels, the chief elements of which are herring-bone patterns, stepped lines, hachures, frets bordered with fringes of short, narrow lines. The negative type of design is common and the patterns were tied together by interlocking scrolls. The name of the painted ware which identifies the period is Sacaton red on buff. The red with black interior is called Santan red ware, and the terra cotta red with black interior is Gila red ware. The latter is believed to be a developed form of the Gila plain ware of the Colonial period. Colonial sherds are also found at all Sedentary sites.<sup>42</sup>

Classic: red on buff, terra cotta red, and the introduced polychrome. The painted red on buff has a fainter base color than in preceding stages, often faded to a faint brown. New technique in the decoration of bowls. Interior colored a dull gray-blue, by burning, and ornamented by a band of red decoration, usually a running fret. Outside decoration, bold designs with cross hatching common. The typical feature of the Classic is the jars. Body shape is globular, but the Gila shoulder retained in modified form. Necks distinct from previous stages in that they were vertical. Negative patterns of frets commonly employed in decorations which closely resemble Sedentary designs. Vessel necks ornamented with square fret, panels of parallel or stepped lines, interlocking negative patterns. Main pottery of the period from the standpoint of the Hohokam seems to have been the terra cotta or Gila red ware. Vessels in this group include square and rounded bowls, jars, pitchers, ladles, effigies, canteens, and eccentric forms. Occasionally there was an exterior design in white, a thin zigzag line bordered by rows of dots. Apparently

<sup>41</sup> Gladwin, 1933; Haury, 1932.

<sup>42</sup> Gladwin, 1933.

synchronous with the advent of the polychrome wares there was a further development of the red on buff in which bowls and small wide-mouthed jars were smoke blackened on the interior, and both the interior and exterior surfaces of the vessels were given a high polish. In the polychrome group all visible surfaces have a clear, well polished red slip upon which bands and fields of white slip were applied as a background for the designs which were painted in black, or occasionally black and red.<sup>43</sup> This is the ware which is correlated with the Pueblo peoples, and it was on pottery of this type that earlier conclusions that the Gila remains were a variation of the Pueblo were based. The typical red on buff jar forms which identify the period are called Casa Grande red on buff; the terra cotta red is Gila red ware.<sup>44</sup> The dull gray-blue bowls are known as Tucson red on buff.<sup>45</sup> The polychrome group has a variety of names, Salado, Pinto, Tonto, Gila, depending upon the source and local characteristics.<sup>46</sup>

Recent: large percentage of plain ware. It is generally a red, porous ware of brick-like consistency with heavy firing smudges. It resembles the plain ware of the modern Pima and Papago.<sup>47</sup>

Modern: the pottery is modern Pima, a highly polished red with designs in black, and the Papago, bright red bowls highly polished both inside and out, and jars with a grayish or brownish buff base color and designs in a brownish red.

There are certain general features in the Hohokam which should be noted. In the Gila basin the Colonial progressed from east to west and north to south; the Sedentary was distributed over the entire area; the Classic centered around Casa Grande; the Recent occurs mainly in the north; the Historic only in the south; in some districts some stages are missing. There was no Classic in the west where the Sedentary developed into Modern. In the southern periphery, the Papagueria, there was no Sedentary. The great irrigation systems of the Gila-Salt River valleys attained their maximum development in the Classic. It is thought by the investigators in this province that the Hohokam cultural pattern flowed outward to effect peripheral areas where the people were in a less advanced stage of development, rather than that the Hohokam received its impetus from an exterior source.

As stated previously, the Hohokam periods correlate roughly with the Pueblo stages. This has been indicated by the finding of Pueblo potsherds in Hohokam sites or an association of potsherds in border line districts separating the two provinces. Pueblo I potsherds have been found in

<sup>43</sup> Kidder, 1924, 109-10.

<sup>44</sup> Gladwin, 1933.

<sup>45</sup> Gladwin, undated, p. 119, type 2.

<sup>46</sup> Gladwin, 1930b.

<sup>47</sup> Gladwin, 1930a, 178, type 2.

Colonial sites, Pueblo II-III in Sedentary, Pueblo III-IV in Classic. In southwestern New Mexico a series of sites, designated Mogollon by Gladwin, which differ from both the Hohokam and the Pueblo, yielded a few Basket Maker II sherds as well as some Colonial Hohokam. From this evidence it has been suggested that the Colonial existed through Basket Maker III, Pueblo I and II into early III. Since the site in question furnished a dendrochronological date of 900 and it is known that Pueblo I was in full flower in the north by 800, the cross finds are not as significant as they might be under other circumstances. Furthermore, no Hohokam sherds have thus far been found in any Basket Maker III sites. The Pueblo I material associated with Hohokam Colonial has been mainly of the western type, which present evidence indicates to be later than eastern Pueblo I. Under the circumstances it would seem precipitate to attempt any closer correlation than that of an approximate synchronization.

One of the interesting problems is that of the paddle and anvil pottery. The question naturally arises as to where this method was derived from and what relation it bears to other areas where a similar technique was used.<sup>43</sup> If the modern Pima are descendants of the Hohokam, their pottery making methods may possibly be considered as a heritage from their predecessors. The Pima pottery is paddle and anvil finished but it is built up by coiling, as is also the case in the southern California and Colorado River tribes. If the same was true for the Hohokam there was not as great a difference in Southwestern ceramics as the general statement of coiled versus paddle and anvil would indicate. Basically they are similar, the distinction being in the finishing processes.

#### COMMENTARY

The Pecos Classification has been enthusiastically praised on the one hand and ardently damned on the other. Its proponents have felt that it was the most outstanding advance in years, while those who have not subscribed to its tenets are convinced that it represents the ultimate in asininity. Both the pros and the cons have shown a propensity to fall into the same error, namely that of thinking that the classification was final. Such was not the idea of the conference. Consensus of opinion was that whenever clear-cut proof warranted a change there should be no hesitancy about making the necessary modifications. An example of this is shown in the case of the Hohokam. Criticism has been forthcoming on a number of counts. In some cases the exceptions have been well taken; in others they show merely a lack of understanding, a failure to read carefully or even

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<sup>43</sup> Gifford, 1928.

attempt, it would seem, to discern the intention of the classification. Among the more intelligent criticisms the outstanding are: that the use of a numerical system has too definitely fixed a time element for the whole province; that it implies a cultural homogeneity in all districts; that too great reliance is placed upon pottery; that there is no need for Basket Maker I; that the various terms are not sufficiently defined; that the assumption that all elements in the complex, except agriculture and the idea of pottery making, were independent local developments is open to question.

Not in the sense of a defense of the Pecos Classification, but to make clear how those most concerned in its formation interpret it may help in understanding its ramifications. In the first place the Basket Maker-Pueblo remains are thought to be representative of a single cultural pattern. The various periods are not regarded as distinct cultures, rather as stylistic or developmental sections of that pattern. It is not thought that the growth followed a smooth and orderly progression. On the contrary, that the advances were intermittent with periods of quiescence during which there was little change. It is the material from the intervals when conditions were static which furnishes the picture for each typical horizon. The lines of demarcation between stages are often vague and there is an overlap of characteristics which may tend to be confusing, although such occurrences generally can be explained.

While the progression of stages infers a certain degree of contemporaneity between sites of the same horizon, it does not necessarily mean that they will fall within identical chronological dates. There may have been, no doubt frequently was, a difference in the precise years in which similar objects were in vogue in different districts. For this reason a simple numerical classification, i.e., dates, is not feasible at the present time. Also, it should not be expected that in every district each group passed through all of the stages. In some sections Pueblo I survived until it was supplanted by Pueblo III. In other parts of the province Basket Maker III continued until it was replaced by Pueblo II. This explains statements in some recent publications to the effect that there was no Pueblo I and in others that there was no Pueblo II. Similar conditions were pointed out for the Hohokam where certain stages are missing in some districts.

Many times the difficulties encountered in determining the horizon of a site are to be attributed not so much to shortcomings of the Pecos Classification as to the failure of the investigator to properly identify his material. The latter can partially be blamed on the classification on the grounds that the definitions are not sufficiently detailed. The excavator on the other hand

may be at fault for not familiarizing himself with the literature on the subject. One reason why the summarized tabulation of the sequence and nomenclature was not more specific was due to the fact that those attending the first conference recognized immediately the characteristics of the stage to which each name applied. Realization that newcomers in the field and those with only a general interest in the subject would not have the knowledge necessary to an understanding of the classification led to an amplification in a number of articles and reports written subsequent to the first and second conferences. These publications cited examples of ruins illustrative of the several stages with names of the authors and the monographs which described the artifacts found there. The papers do not as a rule use the Pecos terminology since they antedate its adoption, but in every instance the correlation is indicated in the article supplying the reference.

The complaint that the use of a numerical system too definitely fixes a chronology has justification, but regardless of what form the designations may take the existence of a certain temporal aspect can not be ignored, because there was a sequence in the development of components of the cultural pattern. That the classification implies a cultural homogeneity in all districts is true, and there is no reason why it should not, because in a broad sense there was a general similarity. In any pattern resulting from home industry there unquestionably will be local variations and regional differences. The age of mass production with clothing, implements, household utensils, ready-to-erect dwellings, all fabricated along standardized lines, had not yet been inflicted upon the country. As a consequence allowances must be made for deviations from the general pattern. Regional variations can be emphasized by using the horizon name in conjunction with that of the district, as Basket Maker III-Chaco Canyon. Pronounced complex differences within small restricted districts may be indicated by the addition of the site name in designations, as Pueblo I-La Plata-Red Horse Gulch. In this connection it may be said, that while horizon fluctuations and period differences in a single region are important, it is just as essential to point out the variations in the same level over the whole province.

The most persistent criticism of the Pecos Classification is that concerning its great dependence upon pottery. Actually much of the censure directed toward that aspect of the problem may be attributed to ramifications for which the classification is only partly to blame. The emphasis placed upon pottery in both the Pecos and Globe sequences, as well as in the discussions at the various conferences, focused attention upon that particular element in the cultural pattern. The result has been that many have made

a means the end. An opinion frequently heard of late is that Southwestern archaeology seems to have become the Elysian fields of the ceramists from which any and all who evince the slightest concern about anything but potsherds must be purged. This is unfortunate, but it is not without some foundation. Much valuable effort is being expended in obtaining information which is of import only insofar as it contributes to the story as a whole, and there seems to be an increasing tendency to disregard the latter part of the problem. A minute study of all the shades of variation and fractional degrees of difference in the pottery of a single site or restricted district constitutes a worthwhile contribution in the field of ceramics. Its full archaeological significance is lost, though, unless its relation to other components in the complex is made clear. The data should be presented in such a way that those who see the Southwest as something more than a mass of potsherds can fit the material into its proper niche in the major structure. While such a statement should not be necessary, it must be pointed out that the work in general calls for wholehearted coöperation and the presentation of all the information. The publicly expressed attitude prevailing in some quarters that "we don't give a whoop about outsiders, whether professional anthropologists or laymen" is inexcusable. "Rugged individualism" has no place in Southwestern archaeology.

Inconsistencies in the results obtained by determining cultural levels from the kinds of pottery found at sites are frequently pointed out as an illustration of the shortcomings of both the Pecos and Globe classifications. Such discrepancies are often solely due to the manner in which the material was interpreted. Under the Pecos sequence, where culinary vessel sherds occur, identification should not be attempted unless sizeable rim or neck fragments are present. It is difficult to determine whether a sherd from the bottom of a smooth surfaced pot is Basket Maker III, Pueblo I, or II. Yet if it came from the neck or rim portion of the jar there would be no dispute. In a number of cases Basket Maker III has been reported on the strength of potsherds which, when accompanying elements are taken into account, should be identified as Pueblo II. Some village remains have been classified as Basket Maker III when there was a large showing of the banded neck Pueblo I type of vessel. Similar conditions prevail in the painted ware groups. Each cultural level has a number of characteristics in pottery and these should be well represented if the identification is to be made upon ceramics alone. Too often reliance is placed upon a single pottery type. Considering the likelihood of survivals this is a tenuous thread upon which to hang conclusions.

One factor which has complicated the use of pottery types in determin-

ing horizons is the divergent methods of making classifications now in use. Some are based on paste quality, others upon design; some make color the criterion, others note only the surface. In many cases the form or shape is ignored because the studies are confined to potsherds. One student names a ware on the strength of his paste analysis, another gives it a different title from its style of design, then a third with surface finish as the criterion adds still another designation. When this condition is multiplied many times over it is no wonder that the ensuing hodge-podge brings dismay. In making a classification for pottery, all of its characteristics should be considered, but hard and fast rules of systematization can not be laid down for its description. As manufactured in the Southwest, it was too plastic an element to conform to rigid specifications. Each household was its own producer of ceramics and while the finished product conformed in general to the prevailing style there were distinct dissimilarities in minor details. This was not only true of the output from different families but of the products of each individual potter as well. As a consequence deviations from a specific pattern will be the rule rather than the exception. It is possible, however, to draw up standards of description which will adequately identify a particular vessel or group of vessels. Any delineation should be based upon the fundamental features of paste, surface finish, decoration, and form. From the standpoint of paste analysis, both as to content and to hardness, the only reliable conclusions are those based on laboratory tests such as have been developed by Miss Shephard at the Laboratory of Anthropology at Santa Fé. Classifications made with consideration of the four factors named above avoid many subsequent difficulties and reduce the total number of groups in the tabulation. Some of the schemes recently proposed would, if followed to their logical conclusions, require a separate class for every pot made in the area. One feature which should be improved is that many of the present classifications not only fail to indicate relationships and similarities between forms which unquestionably are minor variations of a general basic group, but they imply that the differences are equal to those existing between totally distinct types.

As one who has made considerable use of both potsherds and pottery in Southwestern studies, the writer does not by any means deny or belittle their value. He is aware, however, that too great reliance can be placed upon the evidence which they furnish unless it is substantiated by that from other elements. There is no question but what pottery is more sensitive to change than anything else in the complex. Yet in itself it is not sufficient to delineate the whole history of Southwestern development any more than an outline of the changes in styles of china and crockery can

depict the entire course of events in Europe from the time of the Battle of Hastings to the fall of the German Empire.

The criticism of the assumption that the whole Basket Maker-Pueblo pattern is indigenous to the Southwest, except for agriculture and the idea of pottery making, focuses attention upon some of the most pressing problems of the entire study. At the time of the first Pecos conference general opinion was that the pattern had originated in the San Juan area and spread to other regions. There is no question but what much that is typically Puebloan did reach its maximum development in the great San Juan centers and diffuse from them. On the other hand it still remains to be proved that the basic forms of those elements were not introduced from the outside. Closely allied with this problem is that of the Hohokam and its apparent appearance as a full-blown pattern in the desert domain. The belief expressed by those working in that province, that many basic Pueblo traits were transmitted to them from the Hohokam and that the former, with "barbaric vigor," improved upon and carried them to perfection, is open to considerable question. This is particularly so since many of the features are basically quite different.

One important factor in the problem of indigenous development for the uplands pattern lies in the results of dendrochronological studies. Whereas thousands of years had been postulated for the unfolding of the complex, actual dates now show it to have been an extremely short and rapid florescence. Pueblo I dates from approximately 800 to 900 A.D.; Pueblo II, 875 to 950, longer in the peripheral districts. It is possible that the true significance is that the tendency to regard cultures as the result of a slow and laborious process of advancement has been erroneous, and thinking should be modified to the extent that it can comprehend a swift culmination not only for individual elements but for a whole complex. Victor Mindeleff asserted over forty years ago that Pueblo architecture was the result of a comparatively rapid development and that once attained it persisted over a long period of time. Tree-ring data certainly prove his contention. Dendrochronological dates are not now available for the Hohokam and the general belief is that the pattern represents a long interval of development. Since the Pueblo province has been demonstrated to be of recent date, those most concerned with the Hohokam judge that it must be older than Basket Maker-Pueblo. Considering the evidence from the latter, it would seem that in so doing they were falling into the same error with respect to speed of culture growth and that their estimates for the age of the Hohokam may need to be revised.

Criticism of the Globe Classification and the general presentation of the



Hohokam pattern has not been as pronounced as that against the Basket Maker-Pueblo. Of course there are some who insist that it is all "hokum," but in view of the available evidence, that is beside the point. Lack of censure may be attributed to a number of factors. When the sequence was formulated the conferees profited by some of the mistakes made at the Pecos sessions and thus avoided subsequent controversies over minor items. A good example of the benefits accruing from the Pecos discussions and subsequent fault finding is to be observed in the Hohokam terminology. It uses names without numbers, although a certain progression is recognized and reports frequently use figures for convenience in referring to the stages. The major complaint against too marked a dependence upon ceramics and too detailed studies of potsherds *per se* apply to the Hohokam in the same degree as they do to the Basket Maker-Pueblo. As a matter of fact the Hohokam structure was largely erected from potsherd surveys and surface material, and depends to an even greater extent on pottery for period determination, than is the case in the northern province. Excavations made at several sites have tended to substantiate the distinctions made from ceramic characteristics, but much more intensive work with the spade is necessary before evidence now available can be considered conclusive.

There have been numerous proposals in recent months for another conference of workers for the purpose of clearing up present difficulties with nomenclature, classifications, potsherd studies, regional variations, and methods of procedure. Many favor a complete repudiation of both the Pecos and Globe sequences and the starting from scratch with an entirely new terminology. Others are not as drastic and suggest that with a few minor changes both would prove reasonably satisfactory to most of those working in the field. There are many items more pressing than the question of the nomenclature of the major provinces and the developmental stages in the two patterns. The writer thinks that the present classifications are satisfactory as far as they go and sees no particular need for change at the present time; amplification and expansion are essential but complete revision is not the paramount issue.

The nomenclature for the elements in the complex—house forms, implements, pottery types, pottery forms, decorations, areas, etc.—is still in a muddled condition. The naming of pottery types has proved satisfactory in a majority of cases. The plan is to employ binomial terms: a geographic name followed by a technically descriptive word, as Chaco Black on White, Pecos Glaze. The geographic name may be that of the site where the type was first encountered or it may be indicative of the locality of highest

development. Sometimes this is one and the same, although they are frequently different. This explains the numerous examples of discrepancy between the type name and the name of the type site. This situation could be alleviated if the workers were not in such a rush to christen every new potsherd which they find. Definite designations could be withheld, a working title would suffice, until the form was established as a type, the limits of distribution determined, the center of development noted, and the name then be given. A few investigators have followed this plan with good results. In this connection it might be advisable for the ceramists to circularize brief descriptions of new forms, requesting from other specialists such information as they might have on similar specimens and asking if they had been named. This would avoid complications such as now exist where the same type bears several names, each man finding it bestowing a different one. Names for various shapes of pottery do not follow any special plan, except in the Hohokam where a glossary adopted at the Globe conference is employed. Hence there is considerable confusion in the use of such designations as are current. Some workers rely on Spanish words, others use the English, but the names appear indiscriminately in the descriptions of a great variety of vessels. The Hohokam glossary would not function without some modification in the case of the uplands pattern, and thus far no consideration has been given to the problem of making adaptations from it. There likewise is need of a glossary of terms for the various design elements, as well as for styles and types of decoration, since these factors play an important part in regional and horizon studies.

When other objects in the complex are considered, it is observed that there are neither classifications nor names for types. There is no agreement on what constitutes a pit house; the term slab house is carelessly employed; what a pueblo is has not been clearly defined; and architectural features in general are indifferently designated and described. There are no accepted names for the different kinds of stone and bone implements. The area designations are satisfactory in a geographical sense but fail from the viewpoint of indicating phases in the cultural pattern. Much confusion is caused and erroneous conceptions are presented by the terms San Juan culture, Little Colorado culture, etc., because typical features of each are not confined to those areas nor does the material in each constitute a unit. The Chaco phase of the San Juan is considered characteristic of the geographical subdivision, when as a matter of fact Chaco type remains are numerous in the upper Little Colorado and extend over into the Rio Grande. The Kayenta ruins are regarded as San Juan, although they are distinct from Chaco, but many typical Kayenta elements are found in the

Little Colorado. The Little Colorado, on the other hand, contains a heterogeneous lot of remains of Chaco, Kayenta, and Upper Gila derivation as well as characteristically local features. For this reason it would seem better to restrict the use of such geographical terms entirely to an indication of the topographical district, while the archaeological features are denoted by the more specific Chaco phase, Pecos phase. In other words make the classification one of phases rather than of drainage systems. Definite locations could be identified by an additional term such as the Bonito aspect of the Chaco phase.

One of the greatest needs at the present time is a series of comparative studies of bone tools, axes, spear and arrowheads, knife blades, scrapers, milling stones, and related objects. These studies should not only include classifications, but also indicate associations between the various types and other elements in the complex. Furthermore, distribution maps showing the ranges of the various types are essential. This group of artifacts should have careful and thorough treatment of the kind hitherto accorded mainly to ceramics and in some degree to basketry. Except for a very few notable examples, practically no attempt has been made to obtain information from these elements in the material culture.

Questions frequently asked which cannot be answered as yet concern the brachycephalic groups which appeared in the uplands province and became the Pueblo peoples. Where they came from and what lines their penetration into the area followed still have to be determined. Gladwin has attributed the "round-head" invasion to Caddoan peoples because of resemblances between their skeletal and archaeological material and similar remains from certain sites in the southern part of the Pueblo province. Because of considerable disagreement as to what Caddoan is archaeologically and the supposedly late arrival of the Caddo in the Southwest, this suggestion will no doubt provoke much discussion. Perhaps the skeletal similarities should more properly be attributed to a development of both Pueblo and southern Plains peoples out of a basic strain of brachycephals which drifted south along the cordillera to diffuse in several directions. Nevertheless their origin is one of the things which needs to be determined.

The question of the two types of pit houses, the Hohokam and the Basket Maker-Pueblo, calls for consideration. Kidder and Guernsey advanced the theory some years ago that the first Basket Maker houses were an outgrowth from the slab-lined storage bins of the Basket Makers. In its earliest known form, however, the house is so specialized and bears such a striking resemblance to some of the structures in northeastern Asia

that the similarities would hardly seem to be the result of mere coincidence. On the other hand if Kidder and Guernsey had postulated the above-ground structures as being derived from the granaries they would have been correct. The writer found this to be the case in southern Colorado<sup>49</sup> and at a site near Allantown, Arizona, and workers from the Flagstaff, Arizona, museum observed such a transition in that district.<sup>50</sup> The affiliations of the Hohokam houses are not clear. Superficially they do not give the impression of being derived from the Basket Maker-Pueblo form, nor does the latter suggest that it was influenced by the Hohokam. Both have certain features in common, namely pole and plaster superstructures, a side entrance passage (present only in Basket Maker III but surviving as a ventilator in later horizons), and the floor level below that of the surrounding terrain. Some of the Basket Maker-Pueblo pits were rectangular with rounded corners, although they more closely approximate a square than do the Hohokam. In addition the interior features of the two are quite different. Both may have been derived from some basic form and have developed along divergent lines before they appeared in the Southwest. Inasmuch as certain elements of the Hohokam are considered an indication that it penetrated into the desert domain from the east, it may be that the house form is in some way related to the rectangular earth lodges of southwestern Arkansas, Kansas, and Nebraska.<sup>51</sup> That the latter, Kansas-Nebraska, are particularly suggestive of something basic in the matter of affinities is indicated by the fact that the pottery associated with them seems to have been made in the paddle and anvil technique, one of the Hohokam traits. Harrington's pottery was of the coiled variety. On the other hand, until the possibilities of showing local developments have been exhausted, it would appear somewhat illogical to go so far afield for evidence.

The outlying districts present a number of problems. Among them is the question as to whether or not the Northern Periphery should be considered as a separate cultural province related to Basket Maker-Pueblo rather than an essential part of it. Also, should the remains in northwestern Arizona and southern Nevada be grouped into a division to be known as the Western Periphery? In the east the limits of the Basket Maker-Pueblo should be traced. Some work is being done on this problem but it deserves more consideration. Then there are the questions of the so-called Basket Maker in the Eastern Periphery and the relationships between the Plains

<sup>49</sup> Roberts, 1930, 63.

<sup>50</sup> Colton and Hargrave, 1933, 73.

<sup>51</sup> Harrington, 1920, pl. 6, pp. 49, 79; Sterns, 1914, 136.

type sites and the pueblo forms. Should the Big Bend cave materials be considered as representing a peripheral off-shoot of the Basket Makers or a separate pattern with some Basket Maker similarities? The writer is inclined to adopt the latter view inasmuch as the main criteria of relationship are the atlatl and curved club. These implements were probably so basic and widespread at one time that *per se* they have no marked significance in the problem of affinities and pattern identification. Unless other characteristic elements are present, the complex should not be considered Basket Maker merely because it includes the club and atlatl. Furthermore, the latter do not necessarily imply any great antiquity when the facts of their late survival in certain districts are recalled.

One thing frequently mentioned in discussions of Southwestern problems, although no one ever seems to do very much about it, is that of carrying the investigations across the international border. The Southwest must be considered as only a portion of a large culture area which likewise includes northern Mexico. The true explanation of many Basket Maker-Pueblo features, unquestionably Hohokam traits as well, will only be obtained after much work has been done to the south of their region. Numerous sites must be excavated in the same careful technique employed in the north in the last few years. Rapid surveys and sporadic sampling have produced some information but, as yet, nothing definite enough to be relied upon. Present evidence in the material culture indicates that after Basket Maker II there was very little, if any, contact between Mexico and the Basket Maker-Pueblo province. In Pueblo III some trade objects appeared which undoubtedly are of Mexican origin, but there was no marked effect on the pattern proper. Whether this was due to the presence of the Hohokam and its serving as a buffer or to some other reason constitutes one aspect of the problem.

The Hohokam presents the question of its origin. There is also the matter of relationship between this group and the so-called Yuman peoples of southern California where certain elements indicate a definite basic tie-up. Furthermore, it has been suggested that the Hohokam, southern California, Sonora, and possibly lower California, should be grouped into one single province comparable to that of the Pueblo area. This consideration includes more than archaeology and calls for close coöperation with the ethnologists. Insofar as the Hohokam itself is concerned, the greatest need at the present time is additional work in the remains of that pattern. When more evidence is amassed various lacunæ may disappear and troublesome features become simple. What intrigues many of the workers is the correlation, both chronologically and culturally, between the Ho-

hokam and the Basket Maker-Pueblo. Suggestions that the Basket Makers received their cultural stimulus from the Hohokam need considerably more support in the form of tangible evidence. It is true that the elements of agriculture and pottery must have been transmitted from the south through some intermediary, but if they came through the Hohokam it would seem that they would more closely parallel that pattern. Certainly the pottery does not and the agricultural methods differed. Until more is known about the Hohokam, however, and the evidence of contacts more convincingly established there should be no rash attempts to compare its stages with those of Basket Maker-Pueblo.

There are many more problems, many ramifications, and numerous views on the Southwest which cannot be included in the present article. Many of these have been pointed out by Doctor Kidder in the Pecos reports, by Doctor Kroeber,<sup>52</sup> by the Medallion Papers, the publications of the Flagstaff museum, and other institutions. An indication of the task confronting the archaeologist is indicated by the examples which have been mentioned and it becomes increasingly clear that the field is by no means worked out. The efforts of the future will of necessity be more detailed than those of the past. When such is the case there is danger that students will lose sight of the significance of their work which, as others have pointed out before, should be that of preparing the basis for a solution of a problem in cultural evolution.

As a result of recent progress there has been a tendency to overlook the work of earlier investigators. This is a mistake, as many important facts are recorded in the pages of their reports. A better knowledge of the literature is unquestionably required by numerous present day workers. One example of a lack of background is shown by occurrences in the last few years. On two different occasions there was a great ballyhoo and splashing of headlines in the press over the discovery of large and vitally important ruins or groups of ruins; a third case was explained in a recent issue of the *AMERICAN ANTHROPOLOGIST*. One of the groups of ruins so heralded was presented with ground plans and a fairly complete description in 1896. The second was reported in 1874, was discussed at a scientific meeting in 1912; an article on investigations in the district was published in 1920; mention of it was made again in 1930; then in 1931 it was "discovered." In each of these cases professional archaeologists were involved. There is need also for better ethnological knowledge on the part of archaeologists working in the area, and early Spanish journals should not be forgotten; there is much of value to be gained from them.

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<sup>52</sup> Kroeber, 1928.

On the whole it may be said that investigations in the Southwest have been producing good results. Despite the criticism directed towards them, both the Pecos and Globe classifications have functioned reasonably well when used with discretion and when proper allowances have been made for local variations. They have been helpful in keeping a broader view of the subject constantly before the investigator. Moreover, they have assisted students in other branches of anthropology and interested laymen in discerning what the archaeologists are trying to do and what their progress has been.

In conclusion the writer may offer one suggestion with respect to what appears to be one of the "burning issues," the Pecos Classification. Since the chronological implications of the sequence seem to be the cause of so much dissatisfaction and difficulty, a slight revision of the terminology may be proposed. Because the terms early and late, as well as numerals, inherently imply chronology, they may be omitted. With these factors in mind the following nomenclature is offered for consideration.

**Basket Maker:** to designate the stage at present indicated by the titles Basket Maker II or Classic Basket Maker. This name was given as an optional term in the original Pecos list. (Since there is no evidence for an antecedent stage, a designation for such a stage is omitted here.)

**Modified Basket Maker:** this would replace Basket Maker III, Late Basket Maker, or Post Basket Maker. The designation would have the merit of indicating that the level was basically Basket Maker, although somewhat changed in form.

**Developmental Pueblo:** this term was used by Morris several years prior to the first Pecos Conference and is now proposed again to supplant both Pueblo I and Pueblo II, incorporating them under the one heading. The complexities caused by the absence of one or the other in some sections and the difficulty of horizon determinations in others would thus be eliminated. It would indicate that the complex was in the evolutionary stages leading up to the maximum development.

**Great Pueblo:** an alternative title for Pueblo III in the original nomenclature, may be retained to designate the era which was truly the classic period of the Pueblos.

**Regressive Pueblo:** replacing the Pueblo IV, this name would denote the period in which there was a general recession from the preceding cultural peak.

**Historic Pueblo:** another choice proffered by the original tabulation, instead of Pueblo V.

It is not thought that these names would solve the nomenclature problem in its entirety, but if they are employed to indicate the cultural level of each site, while the actual chronological position is determined by dendrochronology, much present confusion can be avoided. Also, certain psychological resistance to a more general acceptance of the classification

might be lessened. It should be emphasized that these designations apply to the complex and not to a single element or series of years. The criteria outlined in the Basket Maker-Pueblo discussion would hold for this classification. Even in the case of the original Pecos nomenclature the several horizons should only be considered as indicating the cultural level, the chronology being established by tree-ring dates as suggested above.

Just after the completion of this article a paper proposing a method for the designation of cultures and their variations was published by the Gladwins.<sup>53</sup> It suggests a system of roots, stems, branches, and phases. Characteristic features of the phases are described in uniform terms which avoid such comparatives and prefixes as early, late, pre-, post-, etc. The Pecos and Globe nomenclatures for the main sequences, as well as other familiar terms, are retained. The authors state that the purpose of their plan is to furnish the specialist with a means for making minor distinctions in the building up of sequences and at the same time supply those not concerned with the minutiae of classifications with broader terms. A series of charts illustrating the plan in use are presented. These include names of present linguistic groups in an effort to fit them into the archaeological pattern, which is a hazardous procedure in the present state of our knowledge, but one which furnishes food for thought. The scheme has considerable merit in its wider aspects and is worthy of careful consideration. In some respects it offers solutions to classification problems discussed in preceding pages; in others it adds new ones. The writer does not agree with the interpretation placed on certain features in the Southwest nor some of the groupings in the charts but that is a matter of viewpoint. The plan does provide a systematic method of classification and a means for presenting the archaeological material in a diagrammatic way.

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<sup>53</sup> Gladwin, 1934.



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## TRIBAL INITIATION OF BOYS IN ANGOLA

By WILFRID D. HAMBLY

AN ACCOUNT of initiation rites at Kuchi, Ngongo near Ngalangi, and at Cangamba, has recently been published.<sup>1</sup> At the two first places mentioned a mixture of Ovimbundu and Vachokue tribes occurs. But Cangamba (20°E, 13°S) is a typical center of Vachokue culture, though some of the Babunda and the Valuchazi tribesmen are present.

A comparative study of local rites can now be made with the help of data from the published articles of independent observers: H. Baumann,<sup>2</sup> F. and W. Jaspert,<sup>3</sup> A. Schachtzabel,<sup>4</sup> C. P. Holdredge,<sup>5</sup> and P. A. Delille,<sup>6</sup> whose reports will be considered *inter se* and with my own notes. Personal observations made at Cangamba are particularly useful for comparative study, since the notes were made several hundred miles from initiation centers described by Baumann, Delille, and Holdredge.

### COMPARATIVE STUDY

The rites reported in *The Ovimbundu of Angola* are in harmony with those observed in many Bantu Negro tribes, and they are definitely part of an aggregation of initiation customs described by De Jonghe<sup>7</sup> and L. Frobenius,<sup>8</sup> while all the ceremonies illustrate the principles laid down in A. van Gennep's analysis of puberty rites.<sup>9</sup>

Despite local difference of procedure at Angolan centers, the essentials of the rites are the same everywhere. We have to note the basic distinction between the circumcised and the uncircumcised, seclusion, harsh treatment, instruction in dancing and tribal customs, fabrication of masks and costumes, also rigorous exclusion of women and the uninitiated. Yet to this absolute isolation of novices H. Baumann records some exceptions.<sup>10</sup>

<sup>1</sup> *The Ovimbundu of Angola* (Anthropological Series, Field Museum of Natural History, Vol. 21, No. 2, 1934), pp. 226-33, Plates LXXVII-LXXXIII.

<sup>2</sup> *Die Mannbarkeitsfeiern bei den Tšokwe* (Baessler-Archiv, Vol. 15, No. 1, 1932).

<sup>3</sup> *Die Völkstämme Mittel-Angolas* (Stadischen Völker-Museum, Frankfurt am Main, 1930), p. 140, Plates 9, 10.

<sup>4</sup> *Angola* (Berlin, 1926), pp. 132-34, Plates 8, 13, 24, 48.

<sup>5</sup> *Circumcision Rites Among the BaJok* (American Anthropologist, Vol. 29: 661-69 1927).

<sup>6</sup> *Besnijdenis bij de Aluunda's en Aluena's in de Streek ten Zuiden van Belgisch Kongo* (Anthropos, Vol. 25: 851-58, 1930).

<sup>7</sup> *Les sociétés secrètes au Bas-Congo* (Brussels, 1907).

<sup>8</sup> *Masken und Geheimlunde Afrikas* (Halle, 1898).

<sup>9</sup> *Les rites de passage* (Paris, 1909).

<sup>10</sup> *Op. cit.*, p. 2.

The idea of death and rebirth is emphasized, and there is a change of name, while a ceremonial introduction to village life at the conclusion of the rites is important. Boys circumcised at the same time form an age grade whose nature and functions are inadequately known. The phallic nature of the ceremony is stressed. Barkcloth is still used for masks at places where trade cloth is now in general use.

Such are the general points of comparison of initiation ceremonies, but a more detailed examination of the evidence is instructive.

Information given to Schachtzabel (pp. 132-34) included the important point that novices who reappear in their village at the conclusion of the rites are returned spirits of the dead. This is one of the most constant concepts of all the rites. On his Plate XXIV showing masks at Kasindi, Schachtzabel portrays seven boys in fibre costumes, white fibre collars and skirts; also masks of Roman helmet type. These costumes are of the Kuchi pattern.

Schachtzabel's Plate XLVIII pictures high, conical, spirally-formed headdresses which he saw at Savitenga ( $17^{\circ}$ - $50'$ E,  $14^{\circ}$ - $50'$ S). This mask is of exceptional interest because of its wide distribution in eastern Angola. The boy on the right in my Plate LXXIX, Figure 1 (Cangamba) is wearing such a mask, and the headdress is again shown in Jaspert's Plate IX, Figures 2 and 3; also in Baumann's Figure 24. H. Baumann states that he regards this high conical mask as the oldest form in use, and he notes that the design of the mask is applied to stools and other objects.

A unique mask is pictured by Schachtzabel (plate VIII). This was worn by a novice at Katoko ( $17^{\circ}$  E,  $15^{\circ}$  S). The wings of the mask extend for several feet on each side of the wearer's head, though there is no elevation of the mask above the top of the head, and the general form of this headdress is that of a large boomerang worn transversely. I believe that no other example of this headdress has been reported.

Turning now from mixed central tribes—Vangangella, Vanyemba, and Ovimbundu (with some Vachokue influence)—we consider the Vachokue proper, who have as one of their centres Cangamba in southeast Angola. Another important Vachokue locality is Saurimo in Lunda, and between Cangamba and Saurimo is Dilolo, just over the Belgian border ( $23^{\circ}$  E,  $10^{\circ}$  S), a place for which Delille's description of initiation rites is available.

The photographs given by Delille show that Dilolo and Cangamba, though three hundred miles apart, have the same costumes and rites. The medicine-man wearing a fibre suit, a leather apron, a collar of stiff reeds, and a helmet of barkcloth (Delille, fig. 2), is exactly like the Uluchazi medicine-man who took part in the ceremony at Cangamba (Hambly,

plate LXXXI, fig. 2). Furthermore, Delille's Figure 6 shows boys who are wearing short fibre skirts, while they are confined in small wicker cages closely resembling those photographed at Cangamba (Hambly, plate LXXX, fig. 2). And Delille's Figure 10, depicting seven novices of various statures wearing fibre skirts, is exactly like the novices in my Plate LXXX, Figure 1, showing initiates within the enclosure at Cangamba.

F. and W. Jaspert's Plate IX, Figure i arrests attention since the oval mask ascribed to the Vachokue resembles a disused mask that I obtained from Dr Ennis of Elende, a centre of Ovimbundu culture near the coast. Jaspert's illustration shows the mark of a cross in a circle on the cheek of the mask, and this is a pattern often cicatrized on the cheeks of Ovimbundu women at the present day. I think that the occurrence of this mask among the Vachokue is an instance of the cultural exchange which has been taking place between eastern and western Angola.

The personal observations of C. P. Holdredge were made near Chiombe River in northeast Angola, and later the account was written by Kimball Young. Holdredge appears to be the only reporter who was a witness of the circumcision rite among the Vachokue. The enforced absence of women and the uncircumcised, the building of a stockade for segregation of the novices, the use of fibre skirts, and all other observances are in agreement with data from Cangamba five hundred miles to the south.

The use of green leaves in Vachokue ceremonies is a point deserving further mention. Holdredge saw the novices washed with water and green leaves, and he further states that men chewed leaves which they spat on to the backs and bellies of the initiates. The stilt walkers at Cangamba carried bunches of leaves, and at the same place a medicine-man was seen to use bunches of leaves for washing his patients on several occasions. Evidently this ceremonial use of leaves is frequent and important among the Vachokue.

The statement of Holdredge, that none of the initiation class could be released until all had perfected their lessons, further explains why ethnologists have received such different estimates of the length of time which novices spend in the enclosure.

Holdredge mentions change of name and a prohibition against use of the former name, which was that of the child who by initiation has become a man. My interpreter mentioned that among the Ovimbundu at Elende a boy or a girl makes a voluntary change of name about the time of puberty. He said "they take names they like better," and this present custom may be a survival of a practice followed during initiation rites that are now decadent or obsolete among the Ovimbundu.

When discussing the age of the boys at circumcision, Baumann places the lowest limit at six years and the upward limit at twenty years, and he states that the higher the social standing the earlier the age of circumcision. Baumann further explains that, owing to present economic conditions, boys leave the enclosure intermittently in order to follow their usual occupations, for work must be done and hut taxes have to be paid. These data help to explain the disparity of age among the novices who were photographed and the various statements respecting duration of the rites.

Baumann introduces the subject of the bull-roarer (fig. 13) called *ndumba-mwela*, "the lion's roar," in the Chokue language. Baumann's experience was similar to my own, for he made many inquiries before seeing this instrument in use. Only once was I able to observe a bull-roarer in use, and then as a toy in the hands of an Ocimbundu boy of Elende.

According to Baumann (p. 13) the rite of circumcision is a ceremonial death, therefore mothers of the novices are informed that their boys have died. This information should not be confused with my observation that parents are informed of the death of a boy, whose bored wooden platter is returned from camp as an intimation of the owner's death. This custom at Ngongo is not symbolic of a *ceremonial* death and rebirth as a man; on the contrary the act is an intimation of an *actual* death.

Baumann and Holdredge agree in mentioning a special speech for the novices. Probably this "beschnittenensprache" is an archaic dialect, but no observer has given precise information on the subject. The phallic element mentioned in the rites at Cangamba (Hambly, plate LXXIX, fig. 2) is stressed by Baumann (figs. 10, 30, showing a phallus of wood). Baumann's Figure 25, showing an oval mask fringed with white hair as a beard, is of exactly the same pattern as that worn by a boy at Cangamba.

The foregoing records of different observers, investigating independently at various centres and at different times, give a fairly complete and understandable picture of initiation rites in eastern and east-central Angola. The accounts agree well *inter se*, and they harmonize in ritual and belief with the fundamentals of initiation ceremonies in the west and southwest of the Belgian Congo, though local differences in ceremonial dress and procedure have to be recognized.

#### DECLINE OF CEREMONIES

Initiation rites have undoubtedly declined among the Ovimbundu, but it is improbable that they are defunct. Père R. P. Laugel of the Caconda Catholic Mission stated that he had visited a circumcision camp for boys at Caconda in 1928. He reported that boys who revealed the secrets of the camp were likely to be poisoned, and that harsh treatment prevailed.

Dr M. W. Ennis, who has lived for thirty years at Elende, thinks there has been a reinvasion of circumcision rites in that district within the past fifteen years. Ngonga, my Ocimbundu interpreter, said that the Ovimbundu were taking initiation rites from the Vangangella, a name which the Ovimbundu use to designate a number of tribes of east-central Angola. This is the region in which the rites were observed at Kuchi and Ngongo.

Probably the explanation is that the Ovimbundu, because of their long and intimate contact with Europeans, have almost lost the rites of initiation. But these ceremonies have been better preserved among the more secluded tribes of the east, especially the Vachokue. No doubt exists with regard to the virility of the rites in east and central Angola, and the probability is that initiation ceremonies are gradually penetrating the central region occupied by the Ovimbundu. Of the initiation of girls in Angola scarcely any information is available, and the rite is one of the most important subjects for future research.

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## CENSUS DATA FROM TWO HOPI VILLAGES

By PEARL BEAGLEHOLE

IN THE course of field work at the Second Mesa Hopi villages (Arizona) in the summer of 1934 I took a census of Mishongnovi and Shipaulovi villages.<sup>1</sup> Analysis of the data appears to throw some light on such topics as population and the sex ratio, household composition, variation from the pattern of matrilocal residence, types of marriage and their stability, and clan development, in these two villages. Much of the data is not conclusive since the material was collected at the end of, and incidental to, other inquiries. Though some pertinent facts which I did not get are readily procurable, a return trip is not immediately in sight, and I offer what data I have. The material was obtained from one well informed Shipaulovi woman, Irene, a member of the Sun's-Forehead clan married to a Mishongnovi Pumpkin clansman. Though young, she is remarkably cognizant of, and sympathetic to, Hopi institutions.<sup>2</sup> The data are given in tables supplemented by such comments as seem necessary.

### POPULATION

The population figures are shown in Table 1. I did not obtain the number of males and females by age groups, which would be impossible in any case for the older individuals. I have not used the government census material because this, I think, is of more than doubtful accuracy. The classification of minors and adults roughly distinguishes persons older than the marrying age (18 to 25) from those below it.

TABLE 1

	<i>Male Adults</i>	<i>Female Adults</i>	<i>Male Minors</i>	<i>Female Minors</i>	<i>Total Males</i>	<i>Total Females</i>	<i>Males per 100 Females</i>	<i>Total Popu- lation</i>
Shipaulovi	30	22	17	31	47	53	88	100
Mishongnovi	55	45	75	64	130	109	118	239
Both villages	85	67	92	95	177	162	108.3	339

It is clear from the table that, while there is a preponderance of males in the two villages taken together, in Shipaulovi alone females outnumber males. In the latter village, however, there are nearly twice as many female

<sup>1</sup> I have to thank Dr Ernest Beaglehole for much valuable criticism of the manuscript.

<sup>2</sup> Wherever it was possible to check on her statements with other Hopi or with my own knowledge of the Mishongnovi households, her accuracy was found to be unquestionable.



minors as there are male minors and about three-fourths as many female adults as male. In Mishongnovi there are about four-fifths as many female minors as there are male minors and approximately three-fourths as many female adults as male. There is, then, a large preponderance of adult males over adult females in each village. Although I have no figures on the death rate for each sex, I would guess that the death rate for females is higher than for males, due possibly to a relatively high maternal mortality.

#### HOUSEHOLD COMPOSITION

I have included in this census the few houses located on the ledge below each village. These houses have preserved village identity in terms of the two parent villages. Following Kroeber,<sup>3</sup> I define household as the house unit in which a number of people live around a single hearth. Table 2 shows the number and size of the household in each village.

TABLE 2

	<i>Households</i>	<i>Population</i>	<i>Average per Household</i>
Shipaulovi	16	100	6.25
Mishongnovi	35	239	6.83

The average number of persons per household is considerably above that given by Fewkes and Mindeleff-Stephen for the period just prior to 1900.<sup>4</sup> The figure of these investigators was 4.8 for Shipaulovi and 5.4 for Mishongnovi. The averages computed for 1934 are still well below Kroeber's 1915-16 figure of 7.56 for Zuñi.<sup>5</sup>

The wide variation in the numerical composition of households is shown in the following table.

TABLE 3

Persons per household	2	3	4	5	6	7	8	9	10	11
Number of households, Shipaulovi	1	1	1	3	2	3	3	1	1	—
Number of households, Mishongnovi	2	3	4	4	—	4	9	4	1	4

The range in Shipaulovi is from 2 to 10 persons per household; in Mishongnovi from 2 to 11. But the interesting point is that whereas out of the

<sup>3</sup> Zuñi Kin and Clan (Anthropological Papers, American Museum of Natural History, Vol. 18, Pt. 2, 1917), p. 124

<sup>4</sup> Quoted by Kroeber, *op. cit.*, 123.

<sup>5</sup> *Ibid.*, 124.

total number of fifty-one households there are 12 containing from 2 to 4 persons each, at the other end of the scale there are 23 households containing from 8 to 11 persons each. While I have no specific data on the subject, the household is, in my judgment, a fairly stable unit, excepting only such changes necessitated by marriage and the relatively few occasions on which young people of both sexes and old men change their residence for reasons mainly personal and economic.

Along with the variation in numerical composition is extreme variation in the kinship of the members of the individual households. The data for the two villages are given in Table 4. In describing relationships in this table, I use the term mother for the oldest married woman in the household, or for the senior woman where there is no married couple. Thus mother (*M*) may, or may not, have children, and she may or may not be the senior woman of the household. Father (*F*) is mother's mate, whether or not he is the biological father of her children. Children (*Ch*) covers married or unmarried sons and daughters of the mother. *D* is daughter; *Sis*, sister; *Bro*, brother; *H*, husband. Columns 1 to 17 include all mother's relatives residing with her in accordance with the rule of a matrilocal society. Columns 18 to 24 give father's relatives residing in the household as well as blood and affinal relatives equally of mother and father whose residence is not in accordance with the matrilocal rule. A cross in the appropriate column indicates the presence in the household of one or more persons related to the mother according to the caption at the top of the column. It does not necessarily indicate the number of such persons in the particular household.

The table shows, first, that of the 16 households in Shipaulovi no two are composed of the same kinship group. The thirty-five households in Mishongnovi give 20 different combinations of kin, of which 5 are duplicated in Shipaulovi. Taken together, the fifty-one households give 31 distinct groupings of relatives.

The biological family of mother, father, and children, is the basic unit in forty of the fifty-one households. There are two households in Shipaulovi (3, 12) and four in Mishongnovi (14, 15, 31, 35) in which the married daughter and her family reside with her mother. One household (15) in Shipaulovi includes, among other relatives, two married sisters, their husbands and families, an arrangement apparently infrequent and, in this solitary case, to be terminated soon by the removal of one family to a house now under construction. There are in Mishongnovi two households (28, 29) in which the married son (with his foreign wife) resides with his mother, an arrangement possibly not without precedent in those cases of Hopi-

Navaho intermarriage in the last half century where the couple preferred to reside with the husband's people.

The tendency today is for the married daughter to remove to a separate household as soon as her family is enlarged by two or three children; the new house she occupies is either another house in the village belonging to her clan, or, more desirably a new one on the ledge below. In one case (Mishongnovi, 14) where a couple is residing with the wife's mother and father, the junior husband has been in almost constant conflict with his mother-in-law, mainly over the matter of his children's upbringing. In order to escape from his mother-in-law the husband is anxious to move his family elsewhere, preferably to a new house on the ledge. For economic reasons he is unable to do this. He might move to another house owned by his wife's clan, but either her clan (Butterfly) has no vacant house, or, just as likely, his mother-in-law does not wish her daughter to occupy it. He tried to resolve the difficulty by moving to a vacant house owned by his own clan, to no avail, however, since his mother-in-law promptly padlocked her own house and took up residence in her daughter's new dwelling. I know of no other instance of overt conflict between a woman and her son-in-law resident in one household, though I suspect that in one other case (Mishongnovi, 15), latent hostility would be quickly manifest if the son-in-law did not spend much of his time away from the reservation. Unlike these two sons-in-law, one aged about 45 with an eldest child of 8 years, and the other about 35 with an eldest child of 5 years, the remaining four sons-in-law resident with their wives' mothers are relatively young, still well in their twenties, with children not more than 2 years old. This means that older husbands with older children either solve or avoid actual or potential mother-in-law conflict situations by living in independent households. This is the situation today, and in terms of psychological reality there seems no reason to doubt that this solution would have been adopted in former times, given available houses.

In each village there is one unmated widow at the head of a household. The remaining unmated woman at the head of a household is a divorcée.

The following comments may help further to elucidate the table. Daughter's children, instanced in Column 4, where there is no daughter's husband indicated in the next column (5), are either the children of a deceased mother or of an unmarried, unmated mother. One household (16) in Shipaulovi embraces the children of the deceased daughter as do five households in Mishongnovi (11, 12, 13, 27, 30). In no case has the widowed father remained with the children in his wife's mother's house. But there is one case (Mishongnovi, 27) where, on the death of the mother, the

TABLE 4

[illegible]



father and two of his three children took up residence with the father's mother, the third and eldest child remained with his maternal grandmother.

In Shipaulovi there are two unmarried, unmated mothers both living with their parents (4, 5); in Mishongnovi there are four, of whom two live with their parents, (20, 21); one is a mother's sister's child in the household (24) and the other a daughter's child (30). These are all quite young mothers in their early twenties who will no doubt acquire mates in the usual manner.

In the categories mother's sister's children (Column 8) and mother's sister's daughter's children (Column 9), the mother's sister is deceased in every instance but one (household 15, mentioned above). Similarly, in Column 12, mother's mother's sister is in both cases deceased.

The seven cases in Shipaulovi where mother's brother resides with his sister include nine mother's brothers: one is a widower, one a divorced man, one a bachelor, and six youths whose own mothers are deceased. The five Mishongnovi cases, embracing seven mother's brothers, include four widowers and three youths whose own mothers are deceased. The total of twelve cases of this type (16 individuals) in the two villages is outstandingly large by comparison with most of the other totals outside the biological family. It is a matter of accident that the number of mother's brothers is numerically out of proportion to the number of mother's sisters in that first, only one of the nine youthful mother's brothers has a youthful sister (Shipaulovi, 15), and that second, the number of male adults in the two villages is higher than that of female adults. All other mother's sisters, excepting one, are either married and living in separate households, or deceased. The five mother's brothers who are widowers account for slightly less than one-third of all the widowers now unmated in the two villages. Besides the one who is a mother's brother in household 12, Shipaulovi, there is another divorced, unmated male living with relatives, father's father in household 11, Shipaulovi.

The three mother's mother's brothers (Column 14) residing with their sister's daughters are all widowers. The mother's mother (Column 15) living with her daughter is a widow. Mother's mother's sister's daughter's child (Column 17) is a boy who has until recently lived with his deceased mother's mother; he moved to his mother's sister's daughter's home because her husband needed extra help, a not unusual reason for the infrequent changes of residence.

The total number of persons whose place of residence accords with the strict matrilineal rule (Columns 1 to 17; excepting two children, sons

married to foreign wives living with the husband's mothers: Mishongnovi households 28, 29) is 320, or 94.3% of the total population of the two villages.<sup>6</sup> The percentages for the two villages are practically equal, 94% residing matrilocally in Shipaulovi and 94.5% in Mishongnovi. The remaining individuals, 6 in Shipaulovi and 13 in Mishongnovi, are included in Columns 18 to 24 of the table (again excepting the two married sons in Mishongnovi households 28, 29). By way of explanation of some of these apparently anomalous domiciles the following comments are offered. The two son's children (Mishongnovi, 27) might have resided with their maternal grandmother as does their older brother but for the fact that the paternal grandmother, old, and nearly blind, needed help in the household. For the choice of residence by five of the seven mother's fathers living with their daughters I have no explanation other than personal preference, possibly combined with an economic reason in some cases, since their respective clans (Butterfly, Bear, Water, Sun's-Forehead) are sufficiently strong to provide a home easily for the individuals in question. The same explanation must hold for two additional fathers living with their daughters who belong to clans which could have sheltered them, though not so easily. One of these men stays with his daughter (Shipaulovi, 9) who lives in a house owned by his clan. This household includes also the one man, a widower, resident with his brother's daughter (Column 20). The father's father (Column 21) living with his daughter-in-law in Shipaulovi (11) is a member of a Mishongnovi clan (Pumpkin) which is not so weak that it could not house another person. Two of the father's sister's children (22), whose mother is deceased, are members of one of the strongest Mishongnovi clans (Bluebird); the other (23), whose mother also is deceased, is a member of the Katsina clan which has nearly died out, the only other member in the village being the lad's maternal uncle in whose wife's household he lives. The female child (Column 25) adopted by a clan brother of her mother and his childless wife (Shipaulovi, 8) was one of many children in a household of the strong Mishongnovi Bear clan. In practically every instance, excepting the boy of the Katsina clan and the two foreign wives, totalling 3 out of 19 cases, it would have been possible for these individuals to follow the matrilocal rule of residence. Unfortunately, I cannot give the reason in most of the cases why the rule was not followed. Nor do I know how frequently this variation from the pattern occurred in the past, but on the basis, at least, of what little information there is on the dying

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<sup>6</sup> Of the 36 Hopi residing at the Mission, 29 or 81.5% reside matrilocally, a figure notably lower than that for the two villages.

out and merging of clans, I suspect that there may be precedent for it. Neither my informant on these census data nor any other person ever expressed any condemnation of these "infractions" of the rule. I note that there is one Mishongnovi woman whose daughter lives with her father in another village (Shimopovi); the only reason given is that the daughter prefers to stay with her father. There is no instance of a Hopi wife's residing in the household of her mother-in-law, an arrangement noted by Kroeber as not infrequent at Zuñi.<sup>7</sup>

#### MARRIAGE

The living together of man and woman in one household constitutes a Hopi marriage. The Hopi vocabulary does not distinguish a couple ritually or legally united from one living together without benefit of ceremony. A married couple is described simply as a'mu'm ɣadī'βdī'i', "staying together." I follow in this context the Hopi definition of marriage. Table 5 is an analysis of the fifty-seven marriages in the two villages with reference to the nature of the union and the marital status of the mates. The caption Hopi means married according to the native ritual. Civil designates a couple married according to American civil law. Where neither Hopi nor civil occurs, the couple has not been ceremonially united. The terms widow, widower, divorcée, and divorcé involve in every instance a previous Hopi marriage. The term maid is defined below.

TABLE 5

	<i>Hopi</i>	<i>Hopi</i> <i>and</i> <i>Civil</i>	<i>Hopi</i> <i>and</i> <i>Civil</i> <i>of</i> <i>Wid-</i> <i>ower</i> <i>with</i> <i>Maid</i>	<i>Civil</i>	<i>Civil</i> <i>of</i> <i>Wid-</i> <i>ower</i> <i>with</i> <i>Widow</i>	<i>Civil</i> <i>of</i> <i>Di-</i> <i>vorcé</i> <i>with</i> <i>Maid</i>	<i>Wid-</i> <i>ower</i> <i>with</i> <i>Widow</i>	<i>Wid-</i> <i>ower</i> <i>with</i> <i>Maid</i>	<i>Wid-</i> <i>ower</i> <i>with</i> <i>Di-</i> <i>vorcée</i>	<i>Di-</i> <i>vorcé</i> <i>with</i> <i>Di-</i> <i>vorcée</i>
Ship., No. of couples	6	7	0	1	2	0	0	0	1	1
Mish., No. of couples	17	9	1	3	0	1	6	1	0	1
	<hr/> 23	<hr/> 16	<hr/> 1	<hr/> 4	<hr/> 2	<hr/> 1	<hr/> 6	<hr/> 1	<hr/> 1	<hr/> 2

The native ritual marriage occurs, in almost every case, while the couple is still quite young. From the number and relative ages of their

<sup>7</sup> *Ibid.*, 105.



children, and from my acquaintance with the individuals themselves, I judge that the twenty-three couples who have been married in the Hopi way only are all either approaching middle age or are definitely old. This means that these twenty-three unions have endured for a long period of time. I do not know whether there have been in the past temporary separations, and if so, how many, in any of these cases; there is no reason, however, to believe that estrangements occurred frequently. Of the sixteen couples married both by Hopi rite and by law, six are, in my reckoning, either at or nearing middle age; the remaining younger couples are not without substantial families (from one to four children) and, since I know of no young divorced man or woman, I see no reason to expect that these Hopi unions based on native ritual marriage will not also remain stable.

The civil marriages, of course, have been indulged in mainly by the more sophisticated of the men and women. The civil marriage is not legally binding on the Hopi but I have no data on the effect of the legal tie on the relative stability of the union. The four couples married by law only and without previous marital status include one young couple who have been working off the reservation at intervals and who will probably have a Hopi marriage whenever it is convenient, one Christian couple living within Mishongnovi village, and the two couples of which the wives are foreigners (one Pima, the other Papago). There is a third couple of which the wife is a foreigner (Shasta), but my informant stated, perhaps incorrectly, that this couple was married in the Hopi way, and I have listed it in that category.

The table shows that in two instances widow and widower have been married by civil law. This is not often the case, as is seen from the number of widows and widowers mated without ceremony. There is a good reason for the fact that a person is rarely married twice by Hopi ritual. There is only one person in this category in the two villages. Hopi belief is that if a woman marries in the Hopi way a man himself previously so married, none of the wedding garments is hers to secure the passage of her soul to the adults' afterworld; "she has nothing," everything belongs to the first wife, even though the latter is dead. She remains a maid as far as the after-life is concerned and is regarded as a poor unfortunate by her tribesmen. A man who marries in the Hopi way a woman who has been previously so married, would be punished in the after-life. There is no instance of this in either village today. The instance of Hopi marriage between widower and maid included a civil marriage as well as the native one. In the case of the civil union of divorcé with maid, the wife (a Hopi) was previously married to, and divorced from, a Kiowa off the reservation,

and it is my uncertain impression that this earlier marriage, in some fashion not clear, mitigates her situation in the minds of her fellows. Though I have not heard it deplored, the fact is, nevertheless, that she has never been married in the Hopi way, which normally means that she likewise would have nothing when she died. The case of union between widower and maid without ceremony is most deplorable from the Hopi viewpoint: the wife not only has no wedding robe but the widower in question is her stepfather, and it would seem, on the whole, to be an unusual and ill-favored union. These three irregular marriages are all in the larger village and constitute 7.7% of all Mishongnovi marriages.

Table 6 shows the number of persons now unattached who have been married according to Hopi ritual and the number of bachelors, men well on toward middle age who have never married. There is no really old man in either village who has never been married in the Hopi way.

TABLE 6

	<i>Widowers</i>	<i>Divorcés</i>	<i>Widows</i>	<i>Divorcées</i>	<i>Bachelors</i>	<i>Spinsters</i>
Shipaulovi	7	2	2	0	1	0
Mishongnovi	11	0	1	1	1	0
	—	—	—	—	—	—
	18	2	3	1	2	0

There are now sufficient facts and implications to substantiate certain conclusions. Of the total of 140 persons who either have been married or are bachelors, 128, or 91.4%, have been married according to Hopi ritual. Of the remaining 12 individuals two are the bachelors, two are Christians, two are young mates with a Hopi wedding ahead of them, two are foreign wives, two the husbands of these foreign wives, and the remaining two are the maids, who though mated are not married in the Hopi fashion. This high percentage of persons married at some time in the Hopi way demonstrates the strength even today of the native marriage pattern and the ideology behind it.

There are nine individuals of the 140 under consideration whose "Hopi" marriages were broken by divorce, a percentage of 6.4. I cannot calculate the divorce rate per one hundred marriages since the unions ended by divorce or death involve persons who are now remarried. Data on the frequency of divorce among couples married by neither Hopi nor civil rites were not obtained in this connection: I doubt whether they could be accurate in any case. Dr Parsons has coined the expression "brittle monogamy" to characterize Zuñi and Hopi marriage. On the basis of the data presented above and in the light also of what an investigator in-

evitably learns, not only of the economic coöperation in the household but of the psychological reality of the bond of affection that grows up between marriage partners, the term with all its implications hardly seems justified or apt for the two villages under consideration. Monogamy is the pattern, certainly, for these Hopi marriages. In theory divorce is an easy matter, but if "brittle" refers to that which breaks easily and often, then the term may not be correctly applied to marriage in Shipaulovi and Mishongnovi. There may possibly be some justification for applying the term to the ten marriages last discussed. But insofar as these latter constitute only 17.5% of the total number of marriages, and insofar, again, as it is my feeling that it is not clearly demonstrated by the data that these unions do not possess stability, these marriages can hardly be considered typically "brittle" Hopi marriages.

#### CLAN CENSUS DATA

Though I made no special study of clanship in these villages, the census material contains certain relevant data which have interest when compared with the results of Dr Lowie's thorough study of these two (and other) villages.<sup>8</sup> In the following table giving the clan census for 1934, I adopt the current English translation of the Hopi clan name, though it may not always express the current Hopi usage and even though it is not the most accurate translation. I have indicated the clan names which differ, however slightly, from those given by Lowie. In those instances where Lowie's information distinguished between the membership of two linked clans (e.g., between Chicken-Hawk and Eagle) which are now so completely merged that my informant, at least, did not make any distinction, I in-

TABLE 7

#### *Census of Shipaulovi Clans*

	<i>Resident in Shipaulovi</i>	<i>Married men, Resident Else- where</i>	<i>Christians, Resident at Mission</i>
Sun's-Forehead (Lowie, Qálo[95])	51	9	15
Bear (16)	17	—	1
Reed	5	1	10
Pumpkin	4	—	—
Butterfly	5	—	—

<sup>8</sup> Notes on Hopi Clans (Anthropological Papers, American Museum of Natural History, Vol. 30, Pt. 6, 1929), p. 317 ff.

dicating this as, e.g., Eagle (Lowie, Chicken-Hawk + Eagle). The figure in parenthesis after the clan name is the total of the clan membership in 1916 as given by Lowie.

Since 1916 three new clans have arisen in Shipaulovi. The information obtained on the point, though scanty, may have some bearing on the history of Hopi clans. The manner (though not the cause) in which these new clans have established themselves, is clear in the case of Pumpkin and Butterfly clans: one Mishongnovi woman belonging to each of these clans moved to Shipaulovi. The Pumpkin clanswoman, now old, is married by Hopi ritual to a Shipaulovi Bear clansman. I do not know whether the house she lives in belonged in the first place to the Bear clan or whether it is now considered as her own house. The Butterfly clanswoman whose husband is a Mishongnovi Eagle clansman lives in a house which belonged to the clan (Sun's-Forehead) of her father who stays with her: again, I do not know whether native opinion deems her owner or merely temporary occupant. The reason for her having taken up residence in Shipaulovi may relate to the size of her own clan (Mishongnovi Butterfly) which possibly suffers from a shortage of houses. Since she and her husband are young, there is yet time for them to build a new house on a desirable site below the village: whether they build on the Mishongnovi or on the Shipaulovi side, will finally decide whether Shipaulovi is permanently to have a new clan.

The case of the Reed clan is more puzzling. My informant's explanation is given in full. Not many years since, the Shipaulovi woman now at the head of the single Reed clan household broke with the Sun's-Forehead clan of which she was a member and started calling herself and children Reed clan people. The Sun's-Forehead clan (of which my informant is a member) was apparently glad of the break for, according to Irene, the woman and her family are queer people, to say the least. The Sun's-Forehead and Reed people do not even "go together" (*a-mĩ'mĩm'a'*) now, in the sense that linked clans "go together" (as, e.g., in the naming rite), for the former will have nothing to do with the latter and possibly the reverse is also true. Whatever the whole story may be, my informant was certain that the present Reed clan mother is no recent immigrant to Shipaulovi, and that she was previously a member of the Sun's-Forehead clan. The fact that a Shipaulovi Sun's-Forehead clanswoman is married to an Oraibi Reed clansman suggests that these clans were not formerly considered to be linked clans. Within Shipaulovi there is only this one lineage in the Reed clan. My informant, however, designated two Christian women who reside at the Mission as Reed clanswomen. One is a middle-

aged woman, the other somewhat younger. I did not inquire the nature of their relationship to the Reed clan mother or whether they had moved from the village prior to the establishment of the Reed clan in Shipaulovi.<sup>9</sup>

The Mishongnovi clan census (table 8) shows that the size of the clans has remained relatively stable since 1916. The Badger clan has, however,

TABLE 8  
*Census of Mishongnovi Clans*

	<i>Resident in Mishongnovi</i>	<i>Married, Resident Elsewhere</i>	<i>Christians, Resident at Mission</i>
Butterfly (26)	38	1	0
Bear (42)	31(+1 Christian)	3	1
Corn (26)	26 (+1 Christian)	2 (+1 in town)	0
Bluebird (Lowie, Tcō'cu [22] + Carrying-Strap [10])	26	2	1
Eagle (Lowie, Chicken-Hawk [37] +Eagle [3])	21	4	0
Water (Lowie, Cloud [17])	19	3	0
Coyote (Lowie, Cedarwood-Fire [22])	19	(1 woman, in town)	0
Pumpkin (Lowie, Squash [13])	13	1	2
Parrot (16)	9	2	3
Snake (Lowie, Lizard [11])	9	1	0
Badger (15)	6	0	1
Katsina (5)	2	1	0

suffered serious depletion, and its future depends entirely on one young girl whose only sister is being raised at the Mission. The Parrot and Katsina clans are definitely dying out: there is no longer a woman in the Katsina clan and only two old women without daughters in the Parrot clan. According to my informant, no new linkage of these clans with others more flourishing is yet apparent. Five of the clanless individuals in Mishongnovi are the children of a foreign mother and a Katsina clansman. Should they ever affiliate with any clan it may possibly be with the Katsina clan.

Apart from the mergings (if my informant is correct) where there were formerly but linkages, there is only this to add to Lowie's data: my in-

<sup>9</sup> Dr Lowie has suggested to me that the Shipaulovi Reed clan may have been one lineage within the Sun's-Forehead clan.



formant stated that the Sun's-Forehead and Eagle clans are linked, "go together;" her statement is borne out insofar as neither Lowie, nor I, has record of an intermarriage between these clans. This may be additional evidence to Lowie's that the existence in Shipaulovi in 1916 of only two clans was the result of a "secondary reduction in the number of clans,"<sup>10</sup> the linkage having been extended from a now extinct Shipaulovi Eagle clan to the Mishongnovi Eagle clan. I have found it impossible to collect any new data on the number of maternal lineages within the individual clans: my informant was loath to speak of the dead and I could not get genealogies.

The interclan and intervillage marriages in Mishongnovi and Shipaulovi are given in Table 9. I do not have information on the clan membership of the wives from other villages married to Shipaulovi and Mishongnovi men. The table substantiates the rule of the exogamy of linked clans. The record of intervillage marriages shows the same strong tendencies noted by Lowie: a high percentage of intermarriage between Mishongnovi and Shipaulovi, and for Shipaulovi, a relatively large number of intermarriages with Shimopovi, its parent village on Second Mesa.

HONOLULU  
HAWAII

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<sup>10</sup> *Ibid.*, 325.

## THE SOCIAL DIVISIONS AND ECONOMIC LIFE OF THE WESTERN APACHE

By GRENVILLE GOODWIN

BY WAY of introduction it would be best to explain what peoples are included by the term Western Apache. The term is here used to designate all those Apache peoples who have lived within the present boundaries of the state of Arizona during historic times, with the exception of the Chiricahua, Warm Springs, and allied Apache, and a small band of Apaches known as the Apaches Mansos, who lived in the vicinity of Tucson. The various peoples thus classed together as the Western Apache are apparently enough like each other, and different from other Apache peoples in certain aspects of their culture, to give reason for placing them in a division by themselves.<sup>1</sup>

### SOCIAL DIVISIONS

At the time of the first American occupation of their territory (middle of nineteenth century) the six thousand or so people comprising the Western Apache were divided into five distinct groups,<sup>2</sup> each having its own territory. These groups were: (1) White Mountain Apache, (2) Cibecue Apache, (3) San Carlos Apache, (4) Southern Tonto Apache, (5) Northern Tonto Apache.<sup>3</sup> The five groups felt themselves to be quite distinct from one another, and hostility between certain of them was not unknown. However, among people of the same group there was a fairly close feeling of relationship in custom and speech. It was this that held them together and not any political unity.

Each one of the five groups was in turn broken up into bands or semi-bands. These bands were not equally distinct nor as strongly formed in

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<sup>1</sup> The material upon which this paper is based has been collected during the years 1930-1933 under the auspices of the University of Arizona, in a project which entails the study of the culture of the Western Apache now living on the San Carlos and White Mountain Indian Reservations, and at various settlements off the reservations in the state of Arizona, near Camp Verde, Payson, etc.

<sup>2</sup> The terms "tribe" or "tribal group" may possibly be more suitable than "group," but the latter is here used until some one term becomes established for designation of these Western Apache units.

<sup>3</sup> The reasons for the naming of these five groups in the above manner, as well as for naming the bands as they are on the map, is not here explained. The whole subject is fully discussed in a manuscript now completed on the social organization of the Western Apache, which will be published shortly. The three westernmost bands of the Northern Tonto Apache intermingled with Yavapai people who shared the region with them.





all groups. The formations among the Southern Tonto cannot really be called bands in comparison with those of some of the other groups, and are thus termed semi-bands for convenience. Bands and semi-bands again each had their own territory and refrained from encroaching on that of their neighbors. Though the unity within a band or semi-band was naturally more intense than that within a whole group, still the people were not a political unit, and were mainly held together by common custom and clan and blood relationship.

The next unit below the band was the local group. Every band or semi-band was composed of several local groups, each having its own territory. The local group was the basic unit upon which the social organization and government of the Western Apache was built. Each local group had its own chief who led his people and directed it in matters of importance, such as war or raiding parties, food gathering expeditions, farming projects, and relations with other local groups or foreign tribes. Chiefs did not have supreme power, but instead led their people mainly by prestige and good example: attributes for which they were chosen as leaders.

Beside the chief there was another leader in the local group: the head woman or woman chief, as she was sometimes called. Her function was to counsel those about her in the ways of living and especially to organize wild-food gathering parties among the women.

In a local group there were from nine to perhaps thirty houses, and the majority of people in these generally belonged to the same clan, though some might be blood relatives of other clans, relatives by marriage, or even unrelated. This strong clan and blood relationship within the local groups was what really made them such closely knit units.

Within the local group were several family groups, say from three to six in number. These family groups were in turn made up of from three to eight households. The members of a family group were usually related within the limit of second maternal cousin, though a few relatives by marriage would be included also. Each was controlled by a head man, who directed in almost the same way as a chief, and it was from the various head men within a local group that the chief was chosen. The family group was in reality almost a miniature local group, and naturally acted even more in unison. Often it operated as a separate unit in pursuit of economic ends for short periods of time, but never permanently.

Blood relationship among these people was and is a very strong bond, involving mutual aid and responsibility. Obligations thus entailed were felt to be stronger on the maternal side than on the paternal because of

matrilocal residence and the clan system. Blood relatives were considered close within the limit of second or third cousin, but beyond this strict observance of blood relationship obligations depended more on close association between the relatives concerned.

The clan system of the Western Apache is not so easy to fit into the sequence formed by the already described social divisions. Whereas each of the groups, bands and semi-bands, local groups, and family groups belonged to one area only, the clans formed cross strata of relationships which ran through the several groups, bands, etc., joining all together. Many of the clans were represented in more than one group.

Each clan had a name, usually of the place-name type, designating its legendary place of origin or first settlement. Children were born into the clan of their mother. All members of one clan were considered blood relatives and called each other by kinship terms identical with those of consanguinity. Marriage between members of the same clan was not countenanced, though marriage into the father's clan was permissible if the blood relationship was not too close. Members of the same clan were expected to aid each other in time of need, and if it was necessary the whole clan might be called together to avenge a wrong done to one of its members. However, there was no clan government or law beyond the obligations governing the actions of clan relatives to one another.

There existed a varying interrelation among the clans of all groups, and one clan might be related to another clan or several other clans. Between members of related clans the same rules of exogamy and mutual obligation held as between members of the same clan, though to a slightly less degree.

Within historic times, at least, the Western Apache clan was not primarily a territorial unit like the local group, though among certain of the bands there was a tendency to localization. The real power of the clan lay in its far flung web of interrelational obligations between its members in all the Western Apache groups.

Residence was usually matrilocal though not necessarily so. Generally neither boys nor girls married till they had proved themselves fully able to perform the tasks of men and women. In marriage proceedings between two families, the man's family first made a present to the girl's parents, and after that there might be mutual feasting and present-giving. When these evidences of friendliness and esteem were concluded, the young couple set up housekeeping for themselves. From the time of betrothal and marriage the obligations among relatives by marriage were strong, and there was a definite code for the various classes of affinal relatives with which an individual was expected to comply.

In former times a man might have more than one wife (usually not more than two or three) if he could support them, but in doing this he was expected to marry women who were of the same clan as his first wife (usually her true sisters, or daughters of her mother's sister). The same rule applied to a widower, and a widow was under obligation to marry a clan relative of her deceased husband (usually his true brother, or mother's sister's son).

When a man and woman were married, each had his share of the family work and providing. What this really amounted to was that the men did all the dangerous and very arduous or strenuous tasks (war, hunting, heavy digging or lifting, handling unruly livestock, etc.), while the women did all those things which did not require a man's strength and endurance (cooking and camp work, tanning, harvesting, etc.). Thus, though the women had the commonplace tasks, their lot kept them more steadily at work than did that of the men.

Children did not take a serious part in the culture of their people till they were about six years old. From that time on they were taught by their parents or relatives, so that they gradually became familiar with the things which they would have to know in later life. When they had reached the age of twelve or so, they took an active part in the procuring and preparation of foods, and henceforth their serious activities were increased.

The foregoing is a superficial description of the Western Apache social pattern, and it must be realized that it was merely a pattern, not a stereotyped program that the people followed regardless of all circumstances. Also it applies only to the social organization of these people within historic times, and up to the period when the United States Government first seriously started to interfere with the original balance of their culture (1871-1873), when the centralization of the Western Apache on government reservations was accomplished.

After the life on the reservations commenced, the old distinction between the groups began to break down, due to the people being thrown more closely together. At the present time group distinction plays a minor part, though the existence of a sort of rivalry between the descendants of different groups is still quite evident. The same is true of the bands and semi-bands, though with them the distinctions have lapsed even more.

The local group is no longer the close knit unit that it once was. The chiefs are gone, and their power, in great part, is now in the hands of the white agent and his employees. However, the family group still preserves a great deal of its old form. A head man directs the family affairs, and these head men still exert great influence in their communities. In the past

years it has been mainly through them that the people have dealt with the agency.

The clan system also remains partially intact. Thus, though marriage between related clans is sometimes allowed, marriage between members of the same clan is not.

Marriage and family life is much the same now as formerly, except that the presents and feasting at the time of a wedding are not considered strictly necessary, a marriage license and legal marriage ceremony are required by law, and a man may not have more than one wife. Remarriage obligations are still generally in force, as well as those between relatives-in-law. However, children do not take the part in the economic life which they used to take, because they are in school during several months of the year.

At the present time the real economic unit is the family group; the groups, bands and semi-bands, and local groups having given way to the modern, more sedentary, small farming and ranching communities, which are centralized at the seven or eight main farming locations on the San Carlos and White Mountain Reservations, and at two or three localities off these reservations.

#### ECONOMIC LIFE

To understand the economic life of the Western Apache it is necessary to know something of their natural environment. The country which comprised their historical territory can be roughly divided into two areas. The first lies in the southern and southwestern part of the territory, and is in general lower. It is a country of great open desert valleys, separated from each other by abruptly rising mountain ranges. In the valleys grow creosote bush, mesquite, yucca, chollas, sahuaro, etc., grading into oaks, junipers, and piñons on the lower slopes of the mountains. On the tops of the mountains are pines, some conifers, etc. The climate is hot in summer, mild in winter.

The second area is a more uniform upland country, averaging from five to seven thousand feet in altitude, and covered with growths of oak, juniper, and piñon. It was in this type of country that the people formerly lived during the greater part of the year. The mountains are fewer and less rugged, but higher in altitude than those in the first area. On them are thick stands of pine, conifers, etc. The climate is pleasant in summer, but from November to March and sometimes April the weather is fairly cold, with snowfalls not infrequent.

The climate is generally arid in both lower and higher areas. However,

there are certain rainy seasons: in the last part of July and during August, and again during some of the winter months. The varying altitudes throughout both areas gave rise to differences in the character of the country and plant and animal life, thus affording a variety of foods.

The Western Apache had four sources from which to obtain food: wild animals and birds, wild plants, domesticated plants raised on the small farms, and livestock and agricultural products which could be taken in trade or in raids on neighboring peoples. The last source was the least important, as it was easier to obtain food at home. Its main value lay in the fact that horses, mules, burros, and cattle could be captured from the Mexican settlements in the south, and sheep and goats from the Navajo in the north to be butchered and used as food.

Of meat and plant foods, meat formed roughly about thirty-five to forty percent of the whole, plant foods about sixty to sixty-five percent. This percentage naturally fluctuated throughout the year according to the abundance of game and crops. Of edible game there used to be several kinds: bear, deer, antelope, some mountain sheep and elk as well as smaller game like rabbits, rats, squirrels, and certain birds. The larger quarry was hunted with the bow and arrow; the smaller was snared or shot with arrows, mostly by boys.

Men occupied much of their time in desultory hunting, but there were two principal seasons when hunting was given particular attention. These were late spring and fall. Late spring was a good time to hunt, coming as it did between planting and the first wild food crops of July, when the women would have time to care for hides and meat. Fall was even better, as meat and hides were prime, and a man could leave his family safely at the farms, there being nothing much to do at home. It was in the fall that the big hunting parties set out. They were not highly organized affairs, but composed only of a few men under the leadership of one of the party. Women did not usually accompany them, but instead the men butchered and skinned the carcasses, and packed the meat home to the women on horseback. This was possible because the hunting parties were only gone a few days and never ranged very far from home.

Of the sixty to sixty-five percent of plant food of the total food consumed during the year, about thirty-five to forty percent consisted of wild plant foods and the remaining twenty to twenty-five percent of domesticated plant foods. This proportion applied to those who had farms of average size. Among those who had no farms the domesticated plant foods used were only the few obtained by trade. Thus with the non-farmers the percentage of wild plant foods used was high, often the full sixty to

sixty-five percent. There were many wild food plants, and during the whole growing season (April to November) one or several plants were always available for food. Even in winter there was the mescal which could be roasted and eaten. Certain plants were staples: mescal, sahuaro fruit, acorns, mesquite beans, fruit of Spanish bayonet, sunflower seeds, fruit of prickly pear, piñon nuts, and juniper berries. Of these nine, mescal and acorns were the most important.

In the spring parties set out for the lower country to gather and prepare mescal. In July the sahuaro fruit was ripe there and also certain of the prickly pears. Late July and August was the season for gathering acorns, summer that for mesquite beans. Later, in early September the Spanish bayonet fruit was ripe. When October and November came, the last crops, piñon nuts and juniper berries, were harvested.

As the food was gathered it was either eaten or stored for winter. Storage was usually in caves in sealed olla-shaped baskets, or in the dwellings of the families themselves.

Since most of the principal plant foods grew at different altitudes, it was necessary to keep moving from place to place to harvest them. These journeys lasted from ten days to a month, and as many women and girls were needed to help, the whole family usually went along. However, the men spent the time hunting, as it was not their task to help gather and prepare any of the wild plants except mescal.

Among the Western Apache every family did not have a farm. The farming opportunities of the area varied and even some families who could have did not wish to farm. In the northwestern part of the area many of the people did not farm because of their exposed position to enemy tribes and the resulting danger of living on farming sites. The varying degrees in the amount of agricultural activity between the several groups stands out best when a comparison is made. Among both bands of the White Mountain group and all three bands of the Cibecue group most of the local groups farmed. Among the four bands of the San Carlos group, and the second, third, fifth, and sixth semi-bands of the Southern Tonto group the majority of local groups had farms. In the Mazatzal band and the first and fourth semi-bands of the Southern Tonto group only about half the local groups farmed. Of two bands of the Northern Tonto group the majority of local groups did not farm, and in the other two there was no farming at all.

Farming, as can be seen, was not necessarily carried on by all families in one group, band or semi-band, or clan, but it is true that if some of the families in a local group farmed then the majority of families of that local

group did, usually all at the same site. This does not mean that the farm was a local group institution. It pertained essentially to the family.

Among these people agriculture was not a complex affair. Fields were small: about half an acre or so, often less. On them were raised corn, some beans, squash, and later wheat, but corn was the main crop and formed a staple food. Clearing and tilling fields was done with the digging stick, and in seeding, the planting stick was used. Farming sites located in country high enough to get sufficient rainfall were not irrigated, but those in low country were and the neighbors helped each other in the construction of dams and ditches. Preparing the fields and planting took about a month. All the members of the family group were expected to help if needed. When the corn was about three feet tall, most of the people moved away for the summer to harvest the various wild plant foods. In September they returned to harvest and store the crops, this again taking about one month's time. Much of the corn was stored in large ground caches for future use.

After harvest part of the population remained at the farms, and part moved down to lower country to escape cold weather and to be within close raiding distance of enemy settlements. Though away from the farms for much of the year, yet these were the places that the people considered their real homes.

The old way of life of the Western Apache shows that these people had a mixture of three modes of living. They hunted large game, but did not depend on meat to such a degree that they ever exhausted or drove the game from their territory and had to go long distances for it. They farmed to some extent, but by no means enough so that they could depend on crops for sustenance throughout the year and so remain in one place. They made use of wild plant foods and small game, but this was not sufficient for them to forego the hunting of big game, though it did allow a minority of them to do without agriculture. Thus their existence kept them moving about within a limited territory in which they were able to practice all three ways of living and follow out their seasonal schedule.

One point of importance was the method of travel. In spite of accessibility to the Mexican ranches of Sonora, the horse never became indispensable in travel, and travel by foot still remained general. The horse was used very often as a pack animal. When families had to move on foot they packed their belongings on their backs in burden baskets. The Western Apache, it must be understood, were never a stock-raising people.

Though much of the population had semi-permanent homes at their farms, they moved too much from one place to another to develop



any of the arts that a more sedentary people might have. Their dome-shaped, brush-covered dwellings were easily reconstructed and set up in a new place when necessary. On account of frequent travel their belongings had to be conveniently transportable and thus, though they made some pottery, the art was never carried far. They did no textile weaving in cotton or wool, but made their clothing and blankets from skins. However, there was one real art which they did develop and which fitted perfectly to their mode of life. This was basketry; an art carried to perfection among them.

The old type of subsistence has not been given up altogether: even today a good many of the more common of the wild plant foods are used, mescal and acorns still being staple foods. However, due to lack of game, beef is now the principal meat food. In spite of the many changes of recent years the most popular type of dwelling is still the old style house.

Unfortunately the reliable material already published on the Western Apache is not abundant. What little there is deals mainly with separated aspects of their culture so that it is impossible to get a clear perspective of the people as a whole. Therefore it is interesting to look forward to the time when a thorough understanding of the Western Apache and their position in the Southwest can be made possible not only in relation to the other Apache peoples and the Navajo, but to the whole area.

SANTA FÉ

NEW MEXICO

THE CONCEPT OF SUPERNATURAL POWER  
AMONG THE CHIRICAHUA AND  
MESCALERO APACHES<sup>1</sup>

By M. E. OPLER

A NUMBER of my informants have introduced our discussions concerning Apache ritual with the statement: "The Apaches are a very religious people."

These informants know precisely what they mean by "religious." They mean that at every point of his life, the Apache seeks supernatural aid in meeting his problems and conducting his affairs.

"The Apache has help for everything against which he has to contend," is the way one native expressed it with no little pride. And in the days that followed this man abundantly justified his statement.

These supernatural aids to overcome the difficulties and emergencies of life take the form of ceremonies. Such ceremonies, though widely different in detail, are markedly similar in pattern. Very nearly all of them include preliminary ceremonial smoking, the throwing of pollen to the four directions, prayer, and a set of songs.

No matter what your straits, if you are an Apache, you can be sure that there is some ceremony designed to meet the situation. There are ceremonies to bring children to the childless; to aid the woman in difficult labor; to find lost objects. There is a ceremony conducted when a baby is first put into its cradle, another when the child first walks, still another when the young girl enters womanhood. Every disease has its curative rite. When contact with the white man demonstrated that his antics were wholly capricious and unpredictable, an effort was made to control them by supernatural agency, and so there exists even a ceremony to influence the white man. I understand that many an unsuspecting official and army officer has unwittingly yielded to its influence. While the United States Government was deciding whether or not to execute Geronimo, such a ceremony was conducted by the Indians, and my informants remind me that Geronimo finally died, unmolested and ripe with years, at Fort Still, Oklahoma.

It is not proposed to describe, in this paper, any of these numerous ceremonies. It is the intention, rather, to sketch, in broad outline, the ideological foundations upon which these specific ceremonies rest. The hope is that an appreciation of the idea system, of which the various rites and

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<sup>1</sup> This paper was read at the meeting of the Southwestern Section of the American Anthropological Association which met in May, 1933, at Las Cruces, New Mexico.

religious activities are, after all, only channelized and concrete expressions, will pave the way for a more correct interpretation of the ceremonies themselves.

The Apaches conceive of a supreme deity, whose native name can be roughly translated as Giver-of-Life. Giver-of-Life is an impersonal deity, to whom no sex is attributed. Likewise, it is impossible to get a description of the appearance or place of abode of this god. As the name implies, Giver-of-Life is thought of as the creator, the maker of world and man, and the source of all supernatural power. Since the creation Giver-of-Life has had little direct contact with mankind.

Supernatural power, the supreme creation of Giver-of-Life, does reach mankind, and, as we shall see, exercises much control over worldly affairs. In contrast to supernatural power, Giver-of-Life is remote and nebulous to the Apache mind. One might say that this deity has been invoked to lend conceptual wholeness to the supernatural world of the Apache, if indeed the god's origin is not to be sought in Spanish or more recent missionary influence.

The power of Giver-of-Life becomes translated into specific ceremonies. It is these ceremonies which warmly and intimately impinge against Apache life. It is these ceremonies which dominate Apache religious thinking. Giver-of-Life may be mentioned in the opening prayer of a rite. Thereafter, however, the attention shifts to the specific power whose aid the ceremony is attempting to win.

Again, children who have had, of course, no power offered them, and who therefore have nothing to "live by" and no one to "talk to them," as the native phrases go, are sometimes taught by their parents to pray, at least in their hearts, to Giver-of-Life in gratitude for their lives, their creature comforts, and the natural beauties around them. An adult who has not accepted or who has not been offered a ceremony may likewise direct his prayers and thanks to Giver-of-Life.

As has been pointed out, the power of Giver-of-Life does not immediately spend itself in the government of human affairs, nor does Giver-of-Life act as a directing genius over supernatural power.

Power is thought of as a mighty force that pervades the universe. Some of it filters through to the hands of man. But to become manifest to man, power must approach him through the medium of certain agencies and channels, must "work through" something. The most conspicuous of these agencies are certain natural phenomena such as the lightning or sun, and a number of animals, principally the bear, snake, owl, and coyote. I have mentioned some of the more common media through which power is

obtained, but actually there are scores of possibilities. I have obtained descriptions of rites which were secured to man through the mediation of many plants, animals, and natural objects other than those which have been named here. In fact nothing is barred a priori from being a conductor of supernatural power, though the tendency to expect transmission through traditional and well-known channels is strong.

One point which should be emphasized is that every Apache, man or woman, is a potential recipient of supernatural power. Theoretically, at least, no one knows in advance what power may be offered to him or when it may be offered. Then one day a person may have "something speak to him." It may be in a dream; it may be when he is alone in his camp; it may be when he is with a crowd of his fellows. The words or the vision are for him alone. Others, though present, will not see or hear them.

Now follows a very novel and enlightening episode. Perhaps Bear is appearing to a man with its offer of power to cure "bear sickness," a malady marked by deformity and occurring when an individual is frightened or attacked by a bear, or has unknowingly crossed a bear's tracks, touched bear fur, or invaded a bear's den.

Bear informs the man selected that he has watched him closely and has decided that he is the proper person through whom to work and do much good. The listener is careful not to appear too eager for the power. He reminds Bear that there are many other people nearby and modestly suggests that some of them must be more suitable than he.

To this argument the power returns a soothing reply. No one will do but the man selected. He has been especially singled out from among all his fellows. The power will give him and his family long life, will provide him with the means of doing good so long as he lives.

An Apache may accept or reject the power thus offered. If he accepts he is given directions for conducting a ceremony: he is instructed in the songs, the prayers, the four ceremonial gifts he must ask in return for his services, and the taboos, if any, which he must observe himself and impose on the one for whom he is working.

At a later date this same Apache may be approached by another and different power, and he may accept this second ceremony too. I have recorded five different ceremonies for one man now living, and several for a number of other individuals. Thus it is that an Apache becomes "loaded up with powers," as one informant put it.

It is not necessary for a man who has a power to inform others of it. Very often none but a man's own family will know of his ceremony, and this ceremony will be used in behalf of the family only. "A man's power is

not public property. A man with power is not a public man," I have been told. When, however, an individual has consented to conduct his rite in the interests of those outside his family, and with conspicuous success, his services may thereafter be in great demand.

As the mode of transmission would suggest, there is no one version of bear ceremony, lightning ceremony, etc. There may be several individuals, as is now the case with many of these rites, who have the right to conduct ceremonies which bear the same name and have the same purpose; yet these ceremonies will all differ in songs and ritual detail. There are on the Mescalero Reservation today several men who have the important right of painting masked dancers. The purpose for which they perform their task is the same, but their songs, designs, and procedure differ considerably.

It must not be thought that because traffic with a supernatural power is a personal matter, dependent upon the individual's own account, the native will consciously claim for himself experiences never really attained. One of the most poignant beliefs is that performance of a ceremony to which one has not the supernatural right proves fatal to the simulator and most unfortunate for his family. My informants would not even sing a ceremonial song which was not their own without an elaborate explanation in prayer to the power, pointing out that the song was not being sung ceremonially but just for illustrative purposes to accommodate a friend.

In order to practise a ceremony it is not necessary to obtain the rite at first hand from the power. A ceremony can be transmitted from one person to another, usually from an elder to one of the younger people, in order that it be carried on to the next generation. But the transfer must be in accordance with the wishes of the power. Not long ago a middle-aged man dreamed of the masked dancers of a very old painter of the *ga'hé'*, as the masked dancers are called. He took this as a sign that he was destined to carry on this rite and so approached the old man. The old man at once began instructing him in the prayers and songs. The younger man learned them with ease within four days, and this is the accepted test of whether or not the power is willing to "work through" one. Later the old man prayed and sang to his power, asking whether it was the wish of the power to use the novice as its instrument. He received an affirmative reply, and now the two conduct the rite jointly. More and more the major part of the work is done by the younger man. The older man may discontinue his activities altogether soon and retire, so to speak. If he continues assisting until his death, the younger man will perform the ceremony alone thereafter. The mechanics of this particular transfer are typical of those which occur in the transference of all other ceremonies.

It is held desirable to bequeath ceremonies to someone in the family. Wherever possible the ceremony passes from parent to child or from grandparent to grandchild. When the ceremony is transferred to a member of the family, very often no payment is required beyond the ceremonial presents which must always be offered to the power; but when the ceremony is taught to one outside the family circle, a fee always is exacted by the teacher in addition.

I have purposely refrained from giving the word for supernatural power, because there is no one Apache term for it. Supernatural power is neither necessarily beneficial nor harmful. Its potential nature is dual. It may be one or the other. All that is certain is that power is wonderful, capable of accomplishing extraordinary things. The Apaches account for death, disease, and disaster when power is manipulated by malevolent persons or when power itself seeks to do harm. Such harmful supernatural power and the individuals who direct it or lend themselves to it are called 'èn'tì'; beneficial supernatural power and those who carry on its ceremonies are called dîγj̄.

It would exceed the limits of this brief outline to discuss the many interesting aspects of this duality. I will confine my remarks to one point, the psychological implications of which have rather fascinated me.

It is believed that if you practise a ceremony with unusual success for many years, in the end your power will demand that you sacrifice to it one of your near relatives or you yourself must die. If you consent to the sacrifice, the relative will soon afterwards fall in battle or will become the victim of some inconspicuous accident. The death seems natural; actually you have permitted the man to die in your stead.

So strong is this belief that many Apaches live in decided fear of relatives who practise cures consistently. I know a man who is mortally afraid of his own father's power.

The social and economic system of the Apaches has the family as its nucleus. It is important that there be no open dissention or hatred between members to strain the unity of the familial structure. Yet the Apache, like many others, does not always have equal admiration for everyone in his family. I wonder, then, whether we may not look at the fear of a close relative's power as a mechanism by which Apache society permits an honest dislike, which would be ruinous if expressed in social terms, to be expressed in terms of the supernatural.

It will be noted that the picture here given of Apache religion departs considerably from the traditional norm. It has been the practice of travelers and Indian fighters to talk of Apache ceremonial life in terms of "medi-

cine-men." These were supposed to be a few individuals set off from their fellows by their esoteric ritual practices and wielding a power in tribal affairs out of all proportion to their numbers.

I think that we shall have to abandon the use of the word "medicine-man" or acknowledge that the Apaches are a nation of medicine-men. I have worked among three Apache tribes, and I have found scarcely a person of middle age who is not the custodian of some ceremony and the recipient of some supernatural power. Each individual plays his part in the great economy of ritual. One has the songs which must be sung when the war shield and spear are made. Another can raise a dust storm and save his people from detection by the enemy. Another can cure the loathsome sores that attack the face when "snake sickness" is contracted. Each has his feeling of importance. Each is buoyed up by the knowledge that "he who speaks to him" will guide, advise, and protect him.

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AN ANALYSIS OF THE MATERIAL  
CULTURE OF THE TUPI PEOPLES

By S. KLIMEK AND W. MILKE

I

THE object of this work is a structural analysis of the material culture of the Tupi tribes in South America, and finally a culture history of this family of peoples. The tabular compilations, which Alfred Métraux added to his book *La civilisation matérielle des tribus Tupi-Guarani* (Paris, 1928), have furnished us the material for this research.<sup>1</sup> Métraux is a student of E. Nordenskiöld. His work bears all the characteristic marks of the Swedish school: an immense industry, an extensive reading; on the other hand, a not always fortunate typology, and not infrequently carelessness difficult to understand.<sup>2</sup> In spite of this, it stands almost unique in ethnographical literature because it registers tabularly the presence and the absence of a large number of culture elements for a large group of peoples. Only by reason of its existence was it possible for us to produce an exact method for use in culture historical problems.

2. Ethnology directed to historical problems has set as its aim the recognition of the past through the present and the explanation of the present through the past. In order to satisfy this high aim, it must, first of all, bring order into the endless ethnographical material. First it attempts to comprehend the grouping of peoples through the criteria of form and quantity, and then the grouping of the cultural elements through the criterion of distribution. The criterion of form and that of quantity are peculiar not only to historical but to all inductive sciences; the criterion of distribution was introduced into ethnological work by Frobenius, but above

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<sup>1</sup> Métraux, pp. 295-300; p. 56.

<sup>2</sup> Attention should be drawn to a few cases. The statements in the table repeatedly differ from those of the text: e.g., according to the table (p. 295) fishing with poison is present among the Guarayu; according to the text (p. 92) it is absent. The same holds true concerning fishing by damming the streams. According to the table (p. 299) the Tupinamba lack the bow-net, but according to page 92 it is used by them. The harpoon, according to p. 300, is present among the Yuruna and Chipaya; according to p. 92 it is absent there. The spear-thrower is represented doubly in the large table; according to one statement it appears only among the Omagua, but according to the other also among the Aueto. This latter statement coincides with that in the text (p. 79); therefore we held it as authoritative. Compare further Maloka (pp. 295: 56), pile-work (pp. 295: 56), Engobe (pp. 295: 246), blow-gun (pp. 298: 79), pipe (pp. 298: 305). Since it was not possible for us to decide which is the correct and which the false statement, we have followed the table throughout. The trident is represented twice in the table with identical statements. Obviously, this is a simple mistake. It would be desirable if Métraux were to remedy this defect in the second edition of his valuable book.



all by the works of Graebner. Unfortunately, in Graebner's "*Methode der Ethnologie*" it is not specified in a desirable manner. Above all, it is not separated from the criterion of quantity.

The comparison of peoples, in order to recognize their relationship, has meaning only when it extends to a large number of traits, and possibly to the entire culture-inventory. In this sense the criterion of quantity should be specified: it has almost nothing to do with the assumption of historical relations. If two peoples are to be considered similar when they correspond in a large number of traits positively and negatively, then two culture elements are to be considered as diffused in the same way when they appear among the same peoples and also are lacking among them.

But when somewhat more extensive material is available then the purely technical question arises as to how these criteria are to be applied. It is almost impossible, in the comparison of peoples, to take into account simultaneously some hundreds of traits and, in the comparison of the culture elements, to comprehend their presence and absence among a hundred peoples. For the comparison of culture elements, the so-called cartographic method has been used up to the present. Whether or not two culture elements were similarly diffused was decided by observation of the distribution charts. Moreover, these charts were, for the most part, very defectively made. They showed, above all, only the presence but not the absence of an element. Where nothing was entered, it remained questionable whether the element was missing or only sufficient data. The charts of G. Tessman alone make a laudable exception in this regard. But even the best chart is an insufficient basis for ethnological work, especially because it does not show to us the distribution of elements among the ethnographical units, tribes and peoples, but only among ethnographically indifferent areas. If the cartographic procedure can be thus regarded only as a highly inadequate expedient of trait comparison, then, up to now no technical procedure whatever has been known. At first the starting point was from a small number of traits that could be easily surveyed. Such an inadmissible method naturally must have produced questionable results.

As the only way of getting out of these difficulties, we believe that the statistical method should be considered. This was introduced into ethnology in the year 1911 by J. Czekanowski<sup>3</sup> but up to the present it has not found a wide adoption. Like Czekanowski, we have applied the association coefficient *Q* in this work.

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<sup>3</sup> Czekanowski, *Objective Kriterien in der Ethnologie*

If, for instance, we want to establish the degree of similarity between pile work and mud masonry in regard to their diffusion among Tupi peoples, we must first of all take into account the presence and the absence of these elements.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
Pile work	-	-	-	+	-	-	-	-	-	-	-	-	-	-	-	-	+
Mud masonry	-	+	+	+	+	-	+	-	-	-	-	-	+	-	-	-	-

In the vertical column both of the compared elements appear; in the horizontal column seventeen tribes of the Tupi group. We have now to establish: (a) how frequently pile work and mud masonry appear together, (b) how often mud masonry appears alone, (c) how often pile works appear alone, (d) how often both elements are absent. The four numbers obtained are arranged in the following manner:

	Pile work		
		+	-
Mud masonry	+	a	b
	-	c	d

In our case the following number group results:

	Pile work		
		+	-
Mud masonry	+	1	5
	-	1	10

From these numbers, we can calculate the association coefficient  $Q$ , e.g., by Yule's formula:

$$Q_2 = \frac{(ad - bc)}{(ad + bc)}$$

$$Q_2 = \frac{(10 - 5)}{(10 + 5)} = \frac{5}{15} = +0.33.$$

The computed coefficient fluctuates between +1.0 and -1.0. An absolute identity is indicated by +1.0; -1.0 is an absolute difference. Zero indicates that there exists neither a tendency toward similarity nor one of dissimilarity. The greater the positive coefficient, the greater the similarity.

But we have also the following:

		Bororo spindle	
		+	-
Pipe	+	1	3
	-	-	13

Thus Yule's formula gives:

$$Q_2 = \frac{13-0}{13+0} = +1.0$$

Since both elements appear three times separately, we cannot consider this coefficient as an adequate expression of actual conditions. Therefore it is recommended, in those cases where one of the four numbers is equal to zero, not to apply Yule's formula, but that of Pearson, as follows:

$$Q_s = \sin \frac{\pi}{2} Q', \text{ wherein } Q' = \frac{ad-bc}{\sqrt{(a+b)(a+c)(d+b)(d+c)}}$$

In the case mentioned above, the following values result:

$$Q' = \frac{13}{832} = +0.451$$

$$Q_s = \sin \left( \frac{\pi}{2} \times 0.451 \right) = \sin 40^\circ 40'$$

In the logarithmic tables we find that  $\sin 40^\circ 40' = 0.65$ . Thus the coefficient is  $+0.65$ .

Coefficients  $Q_s$  can then be regarded as a much closer approach to reality, although their calculation is a little more complicated than  $Q_2$ .

Just as the similarity of distribution of two traits can be determined with the help of the stated formulae, so also can that of the whole cultures of two peoples. Here we count (a) the traits that are common to both peoples, (b) those pertaining to only one, (c) those confined to the other; finally (d) those traits that are lacking among both peoples, but which appear within the district investigated. The use of these four numbers follows the method just described.

In order to comprehend the grouping of the traits representing whole cultures, we must compare each people with the other trait by trait. If we have, let us say, four peoples, Tupinamba, Guarani, Caingua, and

Chiriguano, who are characterized by 146 culture elements, we obtain, after calculation of  $Q_6$ , the following tabular arrangement.

	1	2	3	4
1 Tupinamba	+1.0	+0.86	+0.41	-0.18
2 Guarani	+0.86	+1.0	+0.63	+0.15
3 Caingua	+0.41	+0.63	+1.0	+0.50
4 Chiriguano	-0.18	+0.15	+0.50	+1.0

This table shows us that Tupinamba and Guarani are very closely connected. The Caingua join them with a somewhat smaller coefficient. The Chiriguano are, however, isolated. They show greatest relationship with the Caingua. We can make these conditions still more evident by a graphical representation introduced by J. Czekanowski.<sup>4</sup> We take the same coördinate-net as in the table and evaluate the coefficients in such a manner that we represent the values from +1.0 to +0.80 by black squares; those from +0.79 to +0.60 by squares with two strong black lines; those from +0.59 to +0.40 by squares with a thin line. Values from +0.39 to 0.0 and the negative values remain without a graphical equivalent. In our case, we obtain the following diagram (fig. 1). The close relationship of

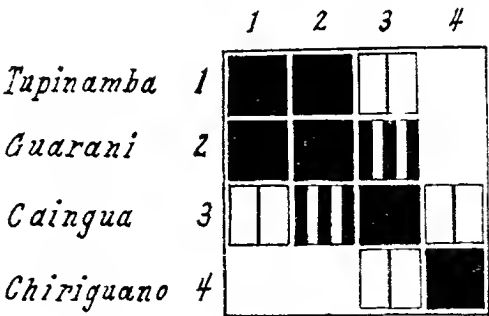


FIG. 1. Graphic representation of the coefficients of association ( $Q_6$ ) between four tribes.

Tupinamba and Guarani and on the other hand the isolation of Chiriguano are expressed in it with exceptional sharpness.

Reference must be made to a noteworthy peculiarity of both Yule's and Pearson's coefficients. When we compare only two peoples with each other, we can establish which elements appear among both and which are peculiar only to one or the other people. It is, however, impossible to procure any elements which are lacking in both without lapsing into absolute arbitrariness.

<sup>4</sup> Czekanowski, Zur Differentialdiagnose der Neandertalgruppe.

ness. The coefficient becomes then constantly negative. This can be considered as the analytical expression of the fact that the comparison of two isolated groups, without the presence of a *tertium comparationis*, does not lead to any result.

If we have distinguished on the one side groups of peoples and on the other groups of traits, we must place both of these groupings in relation to one another.

Only then can we explain our groupings historically. Culturally, very similar peoples will have stood, in general, in a historical relation to each other. On the other hand, one can assume from the uniformly distributed traits that their distribution has been determined by like factors. That means however, in the majority of cases, that the elements of a trait group must have spread together. The next task consists in determining the chronological sequence of various trait groups. According to our view, the various criteria of stratification can give here only indications and probabilities. A certain chronology is, however, possible only with the help of archaeology. We must then place our groups in relation to those of the neighboring territories. As to relations with distant areas, without doubt a prominent rôle falls to the continuity-criterion. It is clear that here again the help of archaeology is indispensable.

## II

1. From the large number of Tupi peoples, Métraux has selected seventeen most representative and best known. These are in the order chosen by him: Tupinamba, Guaraní, Caingua, Chiriguano, Guayay, Yuruna, Chipaya, Ararandeuara, Tembe, Tapirape, Auetó, Apiaca, Munduruku, Mauhe, Parintintin, Omagua, and Oyampi. Of these seventeen tribes, he notes in a large chart the presence and absence of 146 culture elements. Since, among these as mentioned above, spear-thrower and trident are doubly represented, only 144 remain. We have added two more elements—square house and men's house—from the table in the text (p. 59), so that again there are altogether 146 elements.

Our first task consists in establishing the degree of similarity between the seventeen Tupi tribes on the basis of these 146 elements. The calculation gives, on the basis of Pearson's formula, the following Table 1 and, as its graphical presentation, the diagram, Figure 2.

The first conspicuous result is the isolation of Chiriguano, in opposition to whom all other Tupi peoples form a closed group. The Chiriguano have either negative or very slight positive coefficients with other tribes, while elsewhere in the table there appear only positive coefficients, mostly of

TABLE 1. THE COEFFICIENTS OF SIMILARITY ( $Q_6$ ) BETWEEN TUPI TRIBES

	4	3	5	2	1	17	8	9	6	7	14	12	15	10	13	11	16
Chiriguano	+1.0	+ .50	+ .19	+ .15	- .18	+ .05	- .11	+ .04	- .14	- .05	- .01	- .10	- .06	+ .11	- .29	- .04	+ .06
Caingua	+ .50	+1.0	+ .66	+ .63	+ .41	+ .49	+ .52	+ .59	+ .47	+ .45	+ .53	+ .42	+ .44	+ .56	+ .33	+ .36	+ .26
Guayru	+ .19	+ .66	+1.0	+ .69	+ .61	+ .65	+ .65	+ .61	+ .60	+ .61	+ .66	+ .59	+ .72	+ .63	+ .56	+ .51	+ .26
Guarani	+ .15	+ .63	+ .69	+1.0	+ .86	+ .43	+ .55	+ .53	+ .61	+ .64	+ .53	+ .48	+ .45	+ .59	+ .44	+ .36	+ .44
Tupinamba	- .18	+ .41	+ .61	+ .86	+1.0	+ .68	+ .63	+ .54	+ .69	+ .74	+ .70	+ .58	+ .68	+ .53	+ .63	+ .45	+ .40
Oyampi	+ .05	+ .49	+ .65	+ .43	+ .68	+1.0	+ .77	+ .75	+ .71	+ .65	+ .70	+ .64	+ .69	+ .64	+ .49	+ .50	+ .47
Araandeuara	- .11	+ .52	+ .65	+ .55	+ .63	+ .77	+1.0	+ .89	+ .83	+ .78	+ .91	+ .79	+ .73	+ .78	+ .58	+ .54	+ .43
Tembe	+ .04	+ .59	+ .61	+ .53	+ .54	+ .75	+ .89	+1.0	+ .79	+ .84	+ .77	+ .77	+ .72	+ .66	+ .5	+ .50	+ .38
Yuruna	- .14	+ .47	+ .60	+ .61	+ .69	+ .71	+ .83	+ .79	+1.0	+ .98	+ .81	+ .83	+ .80	+ .71	+ .71	+ .72	+ .54
Chipaya	- .05	+ .45	+ .61	+ .64	+ .74	+ .65	+ .78	+ .84	+ .98	+1.0	+ .92	+ .86	+ .83	+ .70	+ .81	+ .65	+ .62
Mahe	- .01	+ .53	+ .66	+ .53	+ .70	+ .70	+ .91	+ .77	+ .81	+ .92	+1.0	+ .89	+ .80	+ .82	+ .79	+ .56	+ .39
Apiaca	- .10	+ .42	+ .59	+ .48	+ .58	+ .64	+ .79	+ .77	+ .83	+ .86	+ .89	+1.0	+ .87	+ .75	+ .86	+ .66	+ .24
Parintintin	- .06	+ .44	+ .72	+ .45	+ .68	+ .69	+ .73	+ .72	+ .80	+ .83	+ .80	+ .87	+1.0	+ .78	+ .69	+ .58	+ .24
Tapirape	+ .11	+ .56	+ .63	+ .59	+ .53	+ .64	+ .78	+ .66	+ .71	+ .70	+ .82	+ .75	+ .78	+1.0	+ .61	+ .58	+ .52
Munduruku	- .29	+ .33	+ .56	+ .44	+ .63	+ .49	+ .58	+ .53	+ .71	+ .81	+ .79	+ .86	+ .69	+ .61	+1.0	+ .54	+ .19
Aueto	- .04	+ .36	+ .51	+ .36	+ .45	+ .50	+ .54	+ .50	+ .72	+ .65	+ .56	+ .66	+ .58	+ .52	+ .54	+1.0	+ .24
Omagua	+ .06	+ .26	+ .26	+ .44	+ .40	+ .47	+ .43	+ .38	+ .54	+ .62	+ .39	+ .24	+ .24	+ .52	+ .19	+ .24	+1.0

considerable size. The gradation of positive coefficients allows us to undertake a further subdivision within the large group. The Caingua and Guarayu are relatively most closely related to the Chiriguano; otherwise both are comparatively isolated, although in a much smaller measure than Chiriguano. Then the Guarani and Tupinamba (group B) form a closed group. A large group (A) follows that includes not less than ten of our seventeen Tupi peoples. The center of this group (sub-group A<sub>2</sub>) comprises Yuruna, Chipaya, Mauhe, Apiaca, Parintintin, and Tapirape. To them are

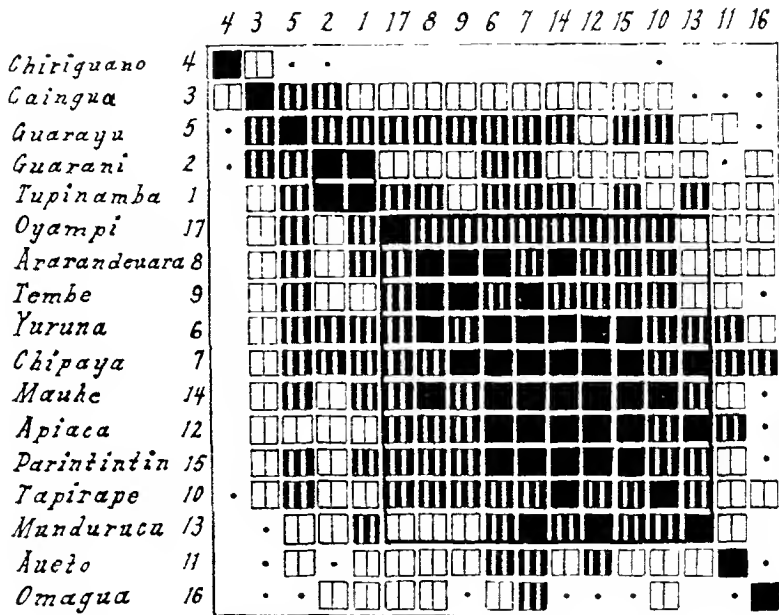


FIG. 2. Graphic representation of the coefficients of association ( $Q_6$ ) between Tupi tribes.

joined on the one side the Tembe, Ararandeuara and Oyampi (sub-group A<sub>1</sub>) and on the other, the Mandurucu (A<sub>3</sub>). Outside of group A, yet not without a certain affinity to it, are the Auetó. Again, the Omagua are strongly isolated; they approach most closely the upper part of group A. As a general result of the diagram we can establish an extensive cultural equation of all Tupi peoples with the exception of Chiriguano alone. This fact was already emphasized by Nordenskiöld and Métraux.

If we want to learn more fully which peoples within the Tupi family occupy a central and which a marginal position, the accompanying dia-

gram yields information (fig. 3). It is arranged in such manner that in the vertical columns are noted the six largest positive coefficients. Thus each vertical column shows us to what other tribes a people has relatively the greatest similarity; the horizontal, on the other hand, shows for what other tribes this people is among the most similar. As we see, all these tribes which have a great number of entries in their horizontal columns belong to the group A.

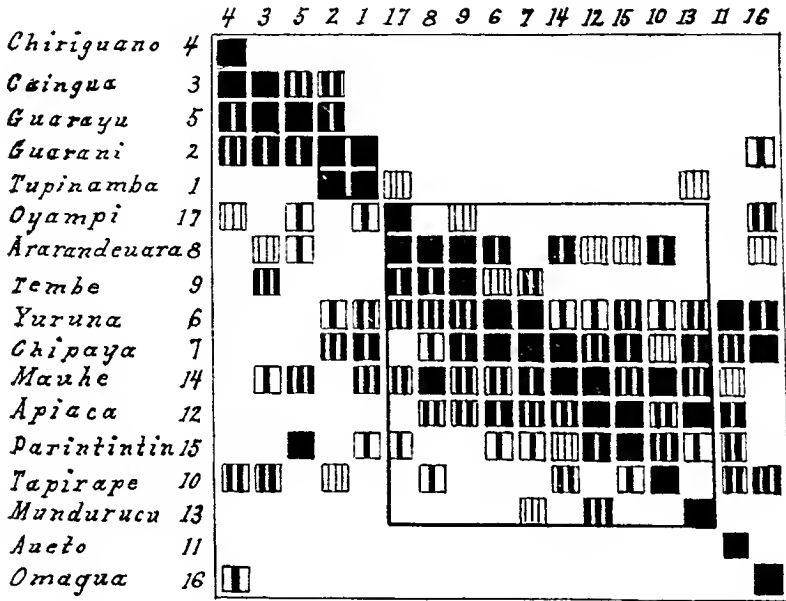


FIG. 3. Graphic representation of the six highest coefficients of association ( $Q_6$ ) between Tupi tribes.

The grouping presented is in full conformity with the geographical distribution of the Tupi peoples, as the accompanying map shows (fig. 4). The southern basin of the Amazon is occupied by the peoples of group  $A_2$ . Scattered among them appear the Mundurucu ( $A_3$ ). On both sides of the mouth of the Amazon we find tribes of the group  $A_1$ . The Tupinamba of the Brazilian coast and the Guaraní of the Parana region are united in group B. The Caingua,<sup>5</sup> Guarayu, and Chiriguano are also mostly isolated like the Aucto and the Omagua.

<sup>5</sup> The Caingua appear on the charts of P. W. Schmidt, W. Krickeberg, and A. Métraux marked in quite different places. It would be welcome if competent authors were to agree on that point some day.



2. We have now to consider the question: what is the reason for just such a grouping of tribes? To answer this question we must investigate the grouping of the elements according to their distribution among Tupi peoples.<sup>6</sup> The result of the calculation of culture elements with the help of Pearson's formula appears in the accompanying diagram (fig. 5).

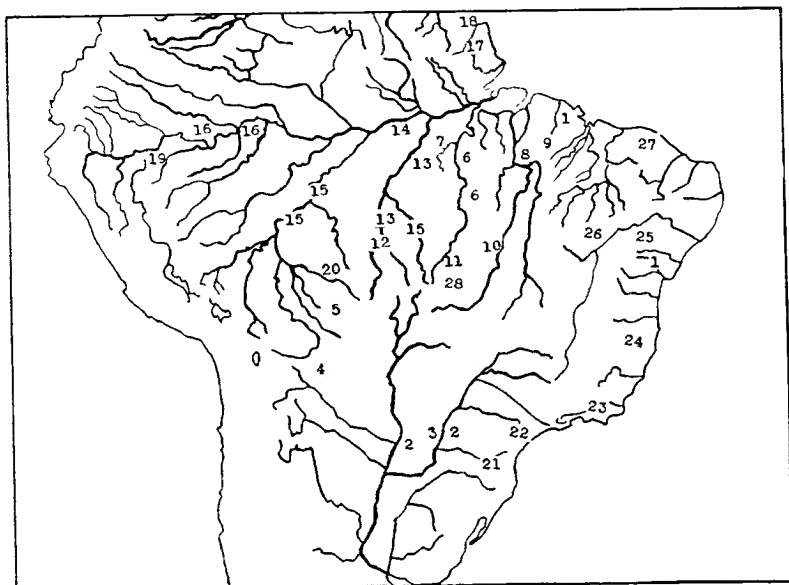


FIG. 4. Distribution of the more important Tupi tribes. (After A. Métraux, W. Krickeberg, and W. Schmidt.)

1. Tupinamba	11. Aueto	20. Pauserna
2. Guarani	12. Apiaca	21. Cario
3. Caingua	13. Munduruku	22. Tupi
4. Chiriguano	14. Mauha	23. Tamoya
5. Guarayu	15. Parintintin	24. Tupiniquin
6. Yuruna	16. Omagua	25. Tupina
7. Chipaya	17. Oyampi	26. Amoipira
8. Ararandeuara	18. Emerillon	27. Potiguara
9. Tembe	19. Cocama	28. Kamayura
10. Tapirape		

<sup>6</sup> We have enumerated the elements on the basis of their order in Métraux's table, to which we add square house (147) and men's house (148). The following elements have an identical distribution (in the diagram sometimes only the first appears): 51 and 27; 109, 102, 66; 64 and 94; 23 and 49; 87, 88, 86, 85, 78, 75, 74, 73, 71, 70, 62, 60; 59, 68, 98; 12, 58; 108, 100, 99; 20; 91, 166, 139; 77, 79.

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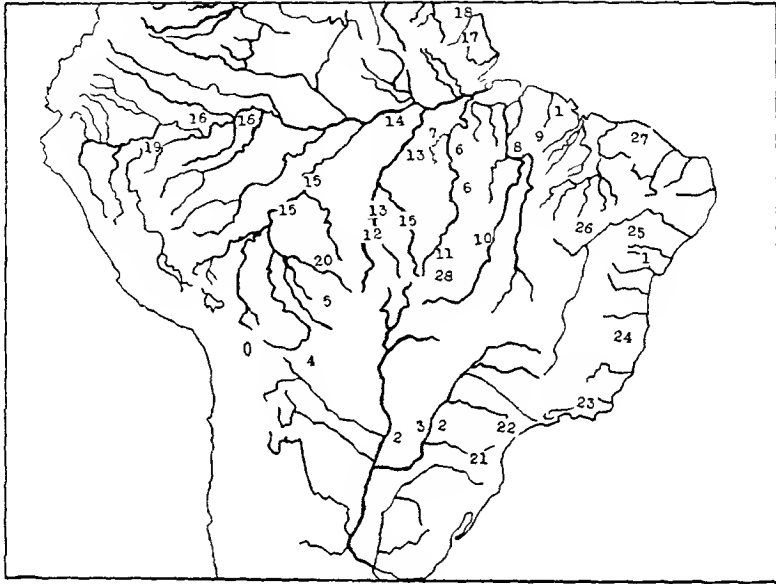
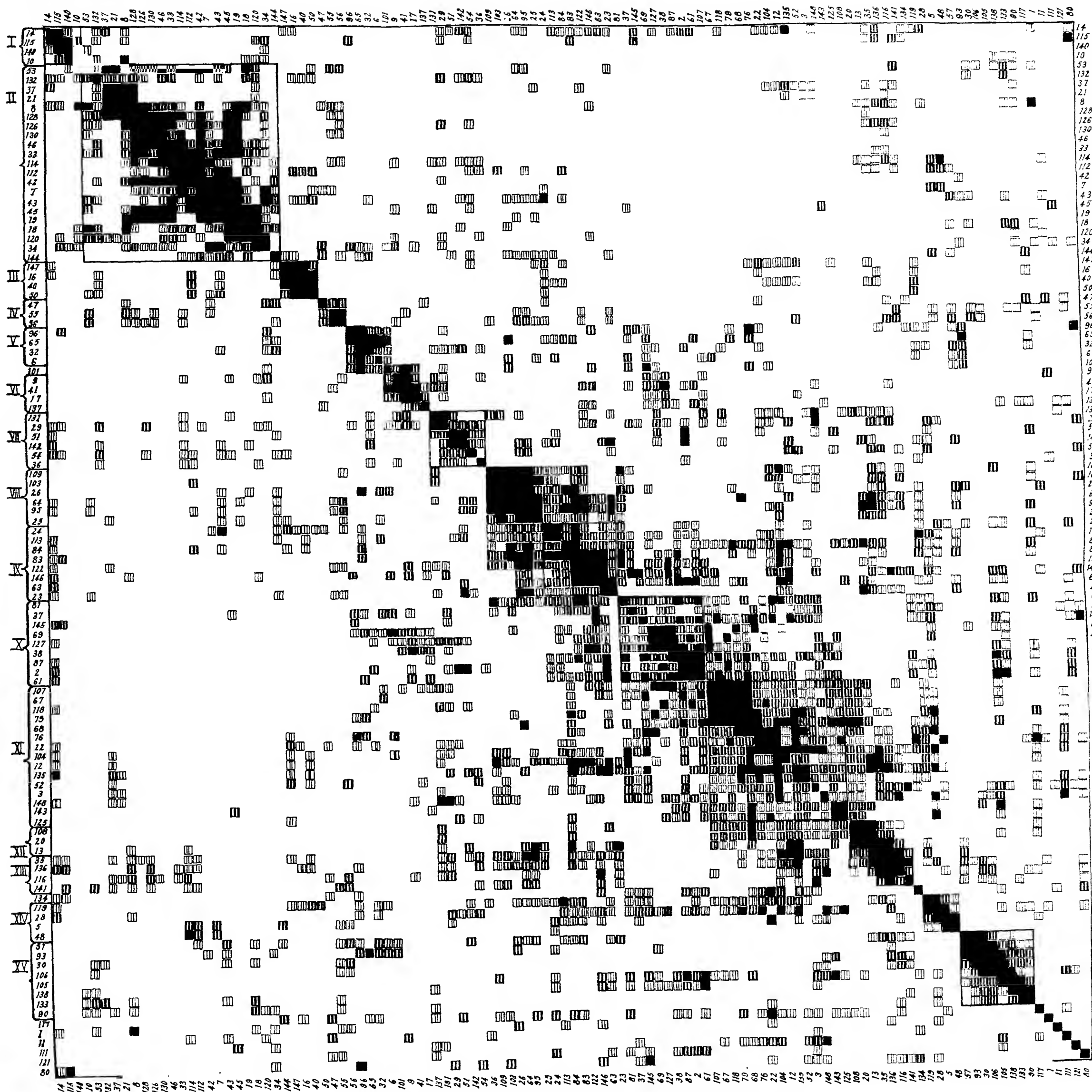


FIG. 4. Distribution of the more important Tupi tribes (After A. Métraux, W. Krickeberg, and W. Schmidt.)

- |                 |                 |                |
|-----------------|-----------------|----------------|
| 1. Tupinamba    | 11. Aueto       | 20. Pauserna   |
| 2. Guarani      | 12. Apiaca      | 21. Cario      |
| 3. Caingua      | 13. Munduruku   | 22. Tupi       |
| 4. Chiriguano   | 14. Mauha       | 23. Tamoya     |
| 5. Guarayu      | 15. Parintintin | 24. Tupiniquin |
| 6. Yuruna       | 16. Omagua      | 25. Tupina     |
| 7. Chipaya      | 17. Oyampi      | 26. Amoipira   |
| 8. Ararandeuara | 18. Emerillon   | 27. Potiguara  |
| 9. Tembe        | 19. Cocama      | 28. Kamayura   |
| 10. Tapirape    |                 |                |

<sup>6</sup> We have enumerated the elements on the basis of their order in Métraux's table, to which we add square house (147) and men's house (148). The following elements have an identical distribution (in the diagram sometimes only the first appears): 51 and 27; 109, 102, 66; 64 and 94; 23 and 49; 87, 88, 86, 85, 78, 75, 74, 73, 71, 70, 62, 60; 59, 68, 98; 12, 58; 108, 100, 99; 20; 91, 166, 139; 77, 79.

FIG. 5. Graphic representation of the coefficients of association ( $Q_a$ ) between the distribution of elements.



This diagram proves that we have to do with different trait groups. On the other hand, the prevalence of positive coefficients is another indication of extensive agreement in the cultural possessions of the Tupi peoples. Let us follow the various trait groups in order. A small group (I) at the upper left of the diagram consists, among other things, of fire-drill, dagger, and Arara methods of affixing feathers to arrows; thereon joins a large group (II) to which belong penis sheath, feathering of east Brazilian type, bow with convex-plain cross-section, digging stick, smoking grate, fire implement with incision, dug-out canoe, bitter manioc, manioc basket, manioc-grater, punt poles, skin canoe, earplug, and Pan-pipe. Two other smaller groups follow. The first (III) contains square houses, fish poisoning, tattooing, and Urupe; the second (IV) contains, among other things, feather cap and feather coat.

A sharply defined group (V) is formed among other things, of nose-piercing, feather fans, head deformation, signal drums of wood, secondary burial in urns, wickerwork of Ucayali type. The next group (VI) is characterized by feathering of Xingu type, sieve net, and conical club. In the following (VII) we find, among other things, bow with round cross-section, clay for pottery mixed with bark, and ant ordeal.

Seven groups which appear strongly interconnected follow. The first is characterized by fur blanket, bola, nutcracker, entrenchment and shield (VIII). To the following (IX) belong, among other things, varnishing of pottery, direct burial urns, Peruvian metal artifacts, and squared pots. A large group (X) follows, comprising, among other things, Peruvian weaving technique, Peruvian games, sandals, metal tweezers, blackening of the teeth, Peruvian feathering, and the sling. A closely related group (XI) includes, among other things, shirt, head-band, calabash with lid, bed of fur, and arrow for birds. Another small group is connected (XII): to this belong cooking sack, sleeping mat, and straw mosaic. In the next group (XIII) we find, among other things, tonsure, fire-arrow, Stampf-rohr, and pole club. Four elements, lip plug, basket with lid, stool, cigar, constitute group XIV. We have, finally, one more well marked group (XV) which contains blow-gun, poison arrow, throwing spear, spear, and round house.

Finally, a number of elements remain that do not unite with any group. These are apron, maloka, musical arrow, weir, ring, and Bororo spindle. The isolation of these elements may be due to various causes. They may really be independent; their distribution may be insufficiently known; or finally a confusion of genetically quite different phenomena may exist. Further, seven elements are not represented in the diagram, since they appear everywhere, so far as is known, so that the coefficient is constantly



TABLE 2. DISTRIBUTION OF ELEMENTS (CONT'D)

[illegible]

indefinite. These are hammocks, wooden mortars, streams choked for fishing, spindle of Bakairi type.

3. Now we can prepare a table showing both peoples and culture elements in the order established by calculation (table 2). This table allows us to make decisions concerning the distribution and character of individual trait groups. Group I is characterized by its appearance among Parintin. Group II is well represented among all Tupi peoples with the single exception of Chiriguano. Group III has a similar distribution, but it is also present among Chiriguano and absent among Aueto. Group IV is represented primarily among Chipaya and Yuruna; further among Tupinamba, Munduruku, and Aueto. In our first four groups we have to do with elements of that horticultural complex which Krickeberg<sup>7</sup> designated as characteristic of tropical South America. This culture is represented with special intensity in the Amazon basin. Therefore groups I-IV can be combined as Amazonian.

The fifth group appears almost wholly confined to the Omagua. It contains not a few elements of the Ecuador civilization. The sixth group appears primarily among Aueto. In all probability we have to deal here with the equivalent of one of those acculturation processes in the Xingu headwaters which K. v. d. Steinen<sup>8</sup> has described. We therefore believe that we might call this the Xingu group. The seventh group appears mainly among Oyampi. We might see in it the result of culture exchanges with the Guiana peoples and call it, accordingly, the Guiana group. The elements of group VIII are peculiar to Guarani alone, in part, and in part to Guarani and Tupinamba. Among those to be found only among Guarani are such elements as undoubtedly go back to the culture of Pampas peoples. And they were not present among all Guarani, but only among Guaranized Cario.<sup>9</sup> The elements common to Tupinamba and Guarani are puzzling as to origin. Thus the shield appears again in greater intensity only in the Orinoco region. We find the ninth group among Chiriguano, Guarani, Tupinamba, and partly among the Omagua. It consists, for the most part, of elements of the Peruvian civilization, which, through trade relations have spread as far as the Brazilian coast. Group X is mostly limited to the Chiriguano.

<sup>7</sup> Krickeberg, *Amerika*.

<sup>8</sup> von den Steinen, *Unter den Naturvölkern Zentral-Brasiliens*.

<sup>9</sup> Once more it should be emphasized that the results of the analysis depend, of course, completely on the reliability of the material taken as the basis; in our case, on the reliability of Métraux's table. Since we are not convinced of its reliability in all points and above all have many an objection to his typology, we prefer to see the value of this paper less in its concrete results than in the demonstration of the advantages of the statistical methods for ethnological work.



We can say with certainty that this is a case of a quite recent influence, mostly from the Peruvian high-culture. The eleventh group appears among Chiriguano and Caingua, further among Guarani and Guarayu. We are dealing here, most probably, with a younger component of the mixed culture of the Gran Chaco, into which not a few Peruvian elements intruded.

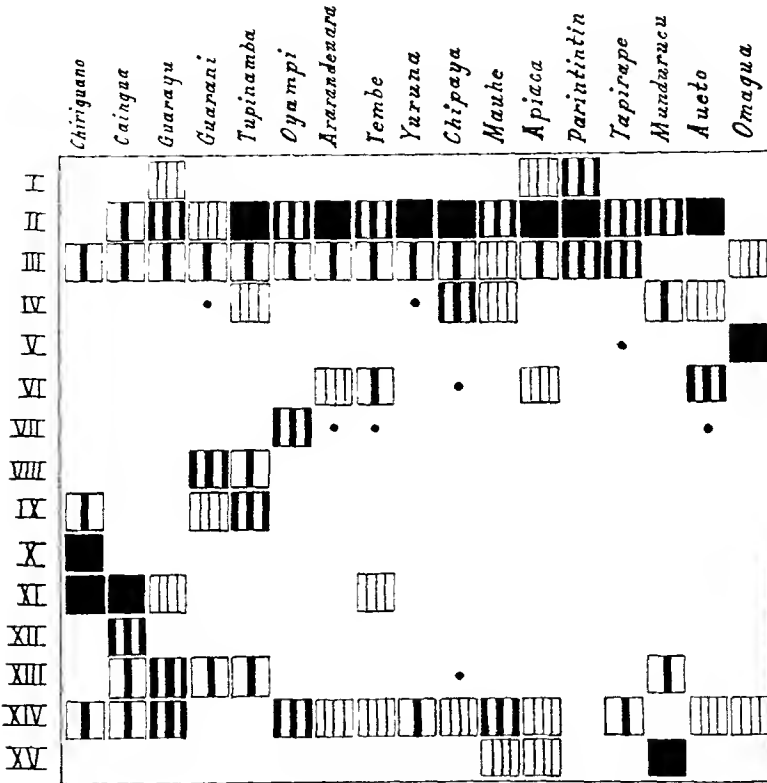


FIG. 6. Graphic representation of the coefficients of association ( $Q_6$ ) between particular tribes and groups of elements.

Our group XII is limited to Caingua. There is no doubt that the case here is one of influence from the primitive culture of the east Brazilian high land. Group XIII is lacking among Chiriguano; it is, however, present among Caingua, Guarayu, Guarani, and Tupinamba. Most probably we have to do here with an older component of Chaco culture. Group XIV shows a relation to our first four groups: it is characterized by its absence among the Parintintin. Group XV is rather exclusively limited to the

Munduruku. Its elements are represented, above all, in northwest Brazil.

4. In order to make the structure of the culture of particular peoples more conspicuous, we have established statistically the affinity between peoples and trait groups. We count how many elements of individual trait groups an individual people has (table 3). To the numbers so obtained the Pearson's formula can then be applied: the accompanying diagram is the result (fig. 6).

We see that the isolation of the Chiriguano is caused by the fact that they have a strong affinity with the recent Peruvian group (X) and also one quite as considerable with the younger Chaco group (XI). The special situation of the Caingua is explained by its great affinity with the younger Chaco group and with group XII, which we consider as derived from the primitive culture of east Brazil. The cultural structure of Caingua is very complicated, since the Amazonian and both the Chaco groups are represented by positive but insignificant coefficients. For the Guarani group

TABLE 3. THE NUMBER OF ELEMENTS IN EACH GROUP IN THE INVENTORIES OF PARTICULAR TRIBES

	Chiriguano	Caingua	Guarayu	Guarani	Tupinamba	Oyampi	Ararandeuara	Tembe	Yuruna	Chipaya	Mahe	Apiaca	Parintintin	Tapirape	Munduruku	Auclo	Omagua	
	4	3	5	2	1	17	8	9	6	7	14	12	15	10	13	11	16	
I	1		2		1	1						1	4					4
II	2	10	13	8	19	13	11	8	16	14	9	13	15	7	13	14	6	20
III	4	4	4	4	4	4	2	2	3	3	1	2	4	3	1		2	5
IV				1	2				3	3	1				2	2		3
V			1			1								1			7	7
VI	1						1	2		1		1				4		4
VII		1	2	1	2	7	1	1			1					2		7
VIII				9	4									1			1	9
IX	7	1	1	5	9	1				1				1		2	4	9
X	21		2	3		2										4	2	21
XI	19	19	7	6	3	4	1	3	1	1				1	3	2	3	19
XII		5			1													5
XIII		3	4	3	4					1					3	1		4
XIV	4	3	4	3	3	4	2	1	2	2	3	1		2	1	2	2	4
XV	2				1						2	3			10	1	2	10
	61	46	40	43	53	37	18	17	25	26	17	21	23	16	33	34	29	131

VIII is characteristic, beside group IX, which for its part is significant for the Tupinamba.

The Oyampi are distinguished by a high coefficient with the Guiana group (VII). And again the culture of Ararandeuara is very mixed and without a definite characteristic component. Here both the Amazonian and Xingu (VI) are involved, as well as the Guiana groups. The case is similar for the Tembe, only here a much higher coefficient with the Xingu group is conspicuous. The culture of the Yuruna and the Chipaya appears almost exclusively determined by the Amazonian groups. The Apiaca and Mauhe show in addition a stronger northwest Brazilian group (XV) intrusion. Parintintin culture is fixed by groups I-III: Tapirape shows in addition to Amazonian affinities others to group VIII and to the northwest Brazilian group. The culture of the Aueto is predominantly characterized by the Xingu group and that of Omagua, above all, by group V.

5. The statistical analysis has set us in a position to distinguish the groups of peoples and the groups of elements, and to explain that the specific culture of particular peoples results from the combination of different groups of elements. Only now, having arranged the ethnographical material, can we proceed to an historical explanation. Above all, we must try to establish the chronological position of the various trait groups in the culture history of the Tupi peoples. Had the other large groups of peoples of tropical South America, such as the Arawaks, Carib, and Gêz, already been the subject of such an analysis as we are attempting here, then it would be easy to establish a connection between their trait groups and ours. Since unfortunately such investigations do not exist, we are limited mostly to suppositions.

Considering both their universal distribution and the primary position they take in the economic life of the Tupi, groups II, III and XIV must be considered as the most ancient stratum of the Tupi culture. The separation of this stratum into three different groups is due to the fact that some elements of this ancient stratum became lost among certain peoples. Also we must credit the seven universal elements to the original Tupi culture.

Thus Tupi peoples were originally endowed with the plant culture of the Amazon basin. This has been preserved not only among the peoples of the southern Amazon basin but also among the Tupinamba of the eastern coast, and there in large measure. The distinctness of the eastern and southern Tupi rests, then, not upon a different initial culture, but upon influences which can be proven secondary in time and function.

We are, therefore, forced to assign the original home of the Tupi to the

Amazon basin. Whether it was situated indeed, as Métraux assumes, between Madeira and Xingu, or north of the Amazon, is for the present not possible to discern.

The ancient cultural structure of tribes which are unified in the group A<sub>2</sub> has been modified in very small degree. Group III, very puzzling in its distribution, has only inconsiderably enriched the inventory of the Yuruna and Chipaya. On the other hand, the Parintintin, probably under the influence of their neighbors of the opposite side of the Madeira (the Carib Arara) have lost group XIV and substituted for it group I. Since the Parintintin have occupied their present home (according to the investigation of Nimuendajú) only in the nineteenth century, due to Munduruku expansion, it seems that here is a case of quite recent development.

Otherwise, particular elements of most varied groups are spread erratically among the central Tupi. The proximity of the Munduruku makes itself felt in some traits of group XV among the Apiaca and Mauhe.

Also the cultures of the Tembe and Ararandeuara, who adjoin the central Tupi on the east, have not experienced any specific decline. The original Tupi culture is somewhat more strongly leveled, and we might ascribe some new scattered acquisitions to their Arawak and Carib neighbors. The cultural destinies of Oyampi also seem not to have been revolutionized to any extent until they migrated in the seventeenth century from the region of the mouth of the Amazon to their present territory. Group VII represents the results of this new cultural environment. When, then, some elements of group XI appear among the Oyampi, we might explain this fact through the Arawak component of Chaco culture, coming from Chané.

The Tapirape appeared in the Araguaya region only in the seventeenth century from the region of Rio de Janeiro, according to a tradition mentioned by Métraux. Despite an element of group VIII, this report cannot be supported on the basis of their culture. The Tapirape have apparently lost relatively much of group II. Otherwise they connect completely with the central group.

The character of group XV makes it appear, without any doubt, that the Munduruku lived at some time in the neighborhood of the northwest Brazilian peoples. Since closer Arawak and Carib relations are apparently lacking, this stay might be set in a zone beyond the Carib-Arawak belt, opposite to that of the Tupi. When, then, the Munduruku burst into their present seat in the nineteenth century, they carried foreign elements from their surrounding peoples into the midst of an archaic Tupi culture.

The Aueto have been subject to the acculturation process in the upper

Xingu region; they have lost group III and exchanged for it group VI. The appearance of some elements of groups IX and X among the Aueto points to the fact that at least one of the isolated peoples inhabiting the Xingu source region show relations to the younger component of Chaco culture. Since the small group IV also appears among Aueto and places them in a closer relation with Yuruna and Chipaya, they might have lived in the vicinity of these tribes before their separation, although not very early.

The Omagua, already at the beginning of the sixteenth century living in the region of the mouth of Rio Napo, have migrated along the upper course of the Amazon into the area of the culture of northeastern Peru, by which ancient and high-culture elements have associated in a complicated manner. This explains the far-reaching loss of the original Tupi culture and exchange of group V. Since the Andean component, active here, is noticeable also in the Chaco, the appearance of some elements of groups IX and X among the Omagua is intelligible. However, it is of course inadmissible to place the home of the eastern Tupi (if not of all Tupi tribes) on the upper course of the Amazon for the sake of this partial correspondence, as W. Schmidt has done.

The question of the fate of the eastern and southern Tupi is the most difficult we have to answer. The conditions are extremely complex: this is already shown in the considerable number of trait groups involved. It is, therefore, to be regretted that Métraux did not include in his table more of the peoples of the Brazilian east coast, and, above all, that he takes the Guaraní as one unit despite their cultural differences. Due to this, statistical analysis here labors under an especially great factor of uncertainty and the following reconstruction of the culture historical course can be of value only as an experiment.

First, one more word about the late, historically certain movements of the peoples concerned. The expansion of the eastern Tupi to the Brazilian coast is rather late, falling mostly in the fifteenth century. They pressed in a northeasterly direction, primarily down the valley of Rio San Francisco in all probability. That the starting point of this movement lay, therefore, on the highland between the Parana and Paraguay is in no way proven and for cultural reasons improbable. Rather, the common center of expansion of the eastern and southern Tupi might have been situated north of the given territory.

The eastern and southern Tupi may have taken over group XIII before their separation. The next phase seems to represent the acquisition of group IX by the Guaraní (together with the Chiriguano) and Tupi-

namba. The Guarayu and Caingua took no part in this development: they had already separated from the rest of the southern Tupi. Perhaps by that time the Caingua had entered into relations with the Gêz peoples east of Parana and acquired group XII.

As the next stage we have to consider the split of the Chiriguano from the Guarani and their migration to the northwest.<sup>10</sup> Perhaps only after this process the acquisition of group VIII by the Tupinamba and Guarani followed. But since the cultural unity of Guarani and the homogeneity of group VIII are quite doubtful, this conclusion may be mistaken.

The Chiriguano arrived by rapid movements as far as the foot of the Andes. Here they fell in the path of the Peruvianized Chané. Under this influence, they lost groups II (and thereby the largest part of the original Tupi culture) and XIII and enriched their inventory by groups X and XI.

It is noteworthy that group XI is completely represented also among the Caingua, since this people, as far as we know, never lived west of Paraguay. In what way this group reached the Caingua and whether this, too, happened only in post-Spanish times, is hard to prove, but the latter circumstance is indeed probable.

Approximately contemporary with the Chiriguano, the Guarayu too moved from the Parana region toward the northwest. Since the cultural environment into which they came was in less strong contrast to their own culture and above all were rather untouched by late Andean influences, the cultural possessions of the Guarayu were not changed substantially. Among the scattered acquisitions from various groups, several elements of group XI represent the most important increase.

In conclusion we can establish:

1. The culture history of the Tupi peoples represents the secondary differentiation of an originally uniform people.
2. This differentiation took place by reason of the influence of peoples of foreign culture upon the particular tribes of the Tupi group and is a consequence of the most far-reaching expansion of the Tupi.
3. With each of the separated trait groups (except group IV) can be connected a group of foreign peoples responsible for the gain or loss of the particular trait group.
4. The fact that in the cultures of these foreign populations partly identical components are present is also responsible for the appearance of elements of a definite trait group, among peoples upon whom the population responsible for this trait group has not exerted a direct influence.

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<sup>10</sup> This happened, as is known, only toward the beginning of the sixteenth century.

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## LIFE HISTORIES RECORDED IN SKELETONS

By WILTON MARION KROGMAN

THE archaeologist and the physical anthropologist are more and more looking to one another for aid. The responsibility for the excavation of skeletal material does not fall upon either—it belongs to both. In a sense, however, the physical anthropologist is the more dependent: he must rely upon the skill and completeness of the technique of the archaeologist. Especially is he dependent that *all* skeletal material, no matter how incomplete or fragmentary, be sent to him, in as good condition—and here care in exhumation, preservation, *and* packing is implied—as possible under the varying circumstances of field work.

In February, 1933, the Anatomical Laboratory received from Mr H. O. Bowman, Columbia, Missouri, the skeletons of two children exhumed by Mr Bowman and a friend from an "Indian mound" near Hartsburg, Missouri. In his letter of communication Mr Bowman stated:

... With the skeletons, however, were some fairly old wrought iron nails with fragments of wood still attached to them, and lying in positions which strongly suggested coffins. We came to the conclusion that some early settlers had made the same mistake we had, that is, they had taken the "mound" for an Indian burial mound and had placed the bodies in it so that they would not be disturbed. . . .

When the skeletons were unpacked and assembled they were found to be almost complete, with the exception of several of the smaller hand and foot bones and some of the epiphyses of the long bones. The completeness of these very young skeletons is a tribute to the care with which the exhumation was done, and lends credence to the interpretation of burial in a "coffin."

The problem of the racial identification of the remains is at first glance the main and only one: were these Indian children buried in accordance with the White man's custom; or were these White children—the children of early settlers or of emigrants on their way to the West—buried in either an "Indian mound" or a natural mounded eminence?

The question of the type of burial place is a difficult one, judging from photographs alone. The Indians often built burial mounds on natural eminences, merely "capping" an existing structure. Or, again, as in the glacial kame burials of Ohio, they utilized natural glacial mounds for intrusive burials. To come directly to the point, I do not think the burial place of the two skeletons was an artificial mound. This conclusion is based solely on the photograph of the burial site, which shows a low conical eminence of very large base diameter. I have no evidence from Mr Bowman of in-



trusion, but this may have been overlooked by the excavators. On the other hand there were no artifacts of Indian make associated with the burials, nor is there any record, as far as I know, of Indian artifacts being found in the "mound." Finally, the presence of iron nails and a "coffin" are certainly evidence of a post-European burial date, and are presumptive evidence of a White burial. This statement does not neglect the fact that Indians did build mounds during the historic period, or that they are known to have made intrusive coffin-burials in mounds already present. But unless there is direct and positive evidence to the contrary, it seems to me that the logical inference is the wisest one: that these children are historic and are White.

This conclusion is supported by a study of the skull of Skeleton A, the older and larger of the two. This skull is complete, except for the left nasal bone. It was received in about thirty pieces and has been admirably restored by Mr W. H. Sassaman, a Research Fellow in the Anatomical Laboratory. His skill and patience enables accurate measurements on the restored skull. The skull of the smaller child was so badly warped and broken, and so incomplete, that reconstruction and measurements were impossible.

The measurements of Skull A are as follows:

<i>Cranial</i>		<i>Facial</i>		<i>Indexes</i>	
	mm.		mm.		
Max. length	157.0	Bi-zygomatic	94.0	Cephalic	82.5
Max. breadth	129.5	Bi-gonial	67.0	Nasal	54.1
Auric. height	101.0	Total face height	83.5	Orbital	95.3
Min. frontal	79.5	Upper face height	49.5		
		Interorbital width	16.5		
		Nasal breadth	20.0		
		Nasal height	37.0		
		Orbital breadth	32.0		
		Orbital height	30.5		

The foregoing measurements are non-committal, for we do not know enough about racial distinctions in the skull of the child—if, indeed, that there are distinctions so early. The dimensions and the indexes certainly do not absolutely discriminate either White or Indian. The general appearance of the skull is that of a child of about four years: prominent parietal eminences, forehead slightly protuberant, face small compared to cranium, and orbital and nasal apertures relatively large compared to facial skeleton. The least forehead diameter and the bi-zygomatic width are both quite narrow and hence, by inference, non-Indian. The skull is crypto-

zygous, a condition which is certainly due to lack of bi-zygomatic development of face as a result of the process of growth.

Granted that racial craniology in the child is little known, and that racial characters may not appear until later, I venture the conclusion that the skull of Skeleton A is that of a male (?) White child. It is entirely probable, therefore, that Skeleton B—the smaller and younger—is White also, since the two were found buried rather close together. The two children may have been from the same family: brothers, or brother and sister.

In this report I have now proceeded about as far as the usual discussion of random skeletal material found while hunting for "Indian relics." Indeed, all too often associated skeletal material is carelessly thrown to one side and never made available for study. The reports that are made offer a few notes on measurements and a general assessment of age based on tooth eruption.

There is an obvious need for a more thorough and more detailed method of attack. The material offers itself for study and analysis, but to date accurate method has not replaced more or less casual observation. There must be a series of "checks" upon estimates of age, sex, and stock.

Recent studies at the Anatomical Laboratory and Associated Foundations have demonstrated the possibility of an age assessment based on teeth, cranio-facial growth, bone growth, and skeletal maturation in terms of epiphyseal ossification and union. Even more than that, the bones, studied under the X-ray, yield intrinsic evidence of the health vicissitudes of childhood. We may unfold, step by step, the health progress of the child.

We shall now turn our attention to the several detailed studies of the skeletal material, individuals A and B.

#### *Dental Age.*

The dentition of Skeleton A is represented by the formula:

$$\frac{m2 \ m1 \ c \ i2 \ i1}{m2 \ m1 \ c \ i2 \ i1} \quad \frac{i1 \ i2 \ c \ m1 \ m2}{i1 \ i2 \ c \ m1 \ m2}$$

All the deciduous teeth are completely erupted. The first permanent molar is to be seen in its crypt.

The dentition of Skeleton B is represented by the formula:

$$\frac{(m2) \ m1 \ -\bar{i}2 \ i1}{(m2) \ m1 \ -\bar{i}2 \ i1} \quad \frac{i1 \ \bar{i}2 - m1 \ (m2)}{i1 \ \bar{i}2 - m1 \ (m2)}$$

The central incisor and the first upper milk molar are the only teeth completely erupted. The lateral incisor and lower first milk molars are



X-rays of Skeleton A to show tooth calcification



B

A

X-ray of long bones to show transverse striations on ends of shafts:  
A, Skeleton A; B, Skeleton B.

erupting, while the second milk molar is still well within its crypt. The canine is not to be seen.

Judging by tooth eruption alone Skeleton A is at least 30 months old and Skeleton B is about 14 months.

The tooth eruption schedules are obviously rather gross, e.g., on the eruption basis alone age estimates are uncertain between completion of the milk dentition at 30 months and appearance of first permanent molar at 6 years. To remedy this situation Broadbent<sup>1</sup> has prepared the more exact tooth calcification age estimate, based on the X-ray study of calcifying teeth.

The skull of Skeleton A has been X-rayed, that of Skeleton B has not. In the X-rays of A (plate 1) it will be seen that the crown and neck of M1 are completely formed, and that the neck of M2 shows beginning calcification. On the basis of this condition the age of Skeleton A may be placed as approximately five years.

Skeleton B, which is fragmentary, cannot be studied in the above detail, but through the care of Mr Bowman and his associate the crowns of M1 were recovered, and enough of mandible and maxilla to demonstrate that M2 has not started to calcify. On this basis the age of Skeleton B may be stated as approximately one and one-half years.

#### *Age According to Estimated Stature.*

Pearson<sup>2</sup> has worked out a series of formulae for the calculation of stature from individual bones.<sup>3</sup> These, however, apply to adults only.

Martin,<sup>4</sup> quoting Saller, and Sawtell-Wallis<sup>5</sup> offer data on the body proportions of growing children. From their data we learn that at about 2 years of age thigh (femur) length is 18% of total height; at 4 years it is 22% of total height.

If we apply these figures to Skeletons A and B, in whom femur length is 225.0 mm. and 157.0 mm. respectively, we calculate total statures of 1022.7 mm. and 872.2 mm. respectively. These may be referred to the Baldwin-Wood Tables:

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<sup>1</sup> B. H. Broadbent, Unpublished research on dental calcification, 1932.

<sup>2</sup> K. Pearson, On the reconstruction of the stature of prehistoric races (Phil. Trans. Roy. Soc., Ser. A, Vol. 192, 1899, pp. 169-244).

<sup>3</sup> For example: Stature =  $813.06 \text{ mm.} + 1.880 \text{ Femur; } 706.41 \text{ mm.} + 2.894 \text{ Humerus; } 786.64 \text{ mm.} + 2.376 \text{ Tibia. Male Whites.}$

<sup>4</sup> R. Martin, *Lehrbuch der Anthropologie* (Jena, 1928), Vol. 1, p. 414.

<sup>5</sup> R. Sawtell-Wallis, Relative growth of the extremities from 2 to 18 years of age (Amer. Jour. Phys. Anthropol., Vol. 16, 1931, pp. 171-93).

		<i>Males</i>	<i>Females</i>
B	872.2 mm.	26-27 mos.	28-29 mos.
A	1022.7 mm.	53 mos. plus	54-55 mos.

If we assume both skeletons to be male, then B is just about a little over two years, A just about four and one-half years.

Growth in height does not seem to have been retarded in either skeleton, unless we attach significance to the fact that A is not quite up to the estimated dental age of five years. But this difference is well within the accepted range of variation.

*Age According to Length of Long Bones.*

The problem of growth in length of femur, tibia, and radius has been studied in detail by Seymour, using the skeletal and X-ray material available in the Laboratory of Anatomy.<sup>6</sup> According to him the length of these bones at birth is as follows:

Femur	101 mm.
Tibia	87 mm.
Radius	68 mm.

TABLE 1. MEASUREMENTS OF THE LONG BONES

<i>Bone</i>	<i>Skeleton A</i>		<i>Skeleton B</i>	
	<i>Right</i>	<i>Left</i>	<i>Right</i>	<i>Left</i>
<i>Femur</i>				
Length—total	225.0 mm.	226.0 mm.	157.0 mm.	157.0 mm.
Length—shaft	203.0	206.0	142.5	143.0
Width—mid	14.0	14.0	11.0	11.0
<i>Tibia</i>				
Length—total	190.0	187.0	132.0	133.0
Length—shaft	171.0	171.0	118.5	119.5
<i>Fibula</i>				
Length—total	—	172.0	—	—
Length—shaft	—	167.5	116.0	115.5
	Prox. end broken	Prox. epiph. missing	No epiph.	No. epiph.
<i>Humerus</i>	Head and great tub. united		Head and great tub. not united	
Length—total	164.5	165.5	116.5	117.5
Length—shaft	155.0	156.0	112.5	112.5
	No dist. epiph.	No dist. epiph.	No dist. epiph.	No dist. epiph.

<sup>6</sup> W. Seymour, Unpublished research on the growth of the human long bones, 1933.

*Radius*

Length—total	—	—	—	—
Length—shaft	116.0	—	83.0	83.5
	No epiph.	No epiph.	No epiph.	No epiph.
		Dist. broken		

*Ulna*

Length—total	—	—	—	—
Length—shaft	—	130.5	93.5	94.0
	No epiph.	No epiph.	No epiph.	No epiph.
	Prox. broken			

Reference to Table 1 will serve to establish the amount of growth in each of the long bones studied in Skeleton A and B.

<i>Skeleton A</i>		<i>Skeleton B</i>	
Femur	225-101 mm. = 124 mm.	Femur	157-101 mm. = 56 mm.
Tibia	190- 87 = 103	Tibia	132- 87 = 45
Radius	116- 68 = 48	Radius	83- 68 = 15

In all femora and tibiae the epiphyses were present. In both radii they were absent.

Seymour tells us that the annual rate of growth in the ends of these bones is as follows, including both epiphyseal and diaphyseal growth:

Proximal femur	6.2 mm. per year
Distal femur	16.8
Proximal tibia	11.0
Distal tibia	10.3
Distal radius	10.3
Proximal radius	1.35

If the total observed growth be divided by the annual rate we will ascertain the age in years. The resultant age figures are given:

<i>Skeleton A</i>		<i>Skeleton B</i>	
Femur	5.4 years	Femur	2.4 years
Tibia	4.8	Tibia	2.1
Radius	4.1	Radius	1.3

If we take an arithmetic average, then Skeleton A is 4.8 years old, Skeleton B is 1.9 years old. The average deviation in both is 0.4 years. In both skeletons the estimation of age from the femur is above the average, and the estimation of age from radius is below the average. Tibia seems to be the more exact. This variability, however, is really no more than may be expected on either side of the arithmetic mean. To a certain extent the range may be explained by the difficulty of measuring rate of

growth in proximal femur, and the absence of epiphyses in the radii of A and B.

The evidence from the study of growth of femur, tibia, and radius may be summarized by stating that A is about five years old, B about two years old.

*Age According to the Femoral Shaft Index.*

In Table 1 the diameter of the shaft of the femur at its mid-length is given as 14.0 mm. for Skeleton A, 11.0 mm. for Skeleton B.

The relation of thickness of compacta to medullary cavity has been studied by Venar<sup>7</sup> using the X-ray as the basis of measurement and analysis. He has formulated the femoral shaft index as the ratio between the combined thickness of the compacta and the medullary cavity in the index:

$$\frac{\text{compacta} \times 100}{\text{cavity}}$$

The diameter of the femoral shaft, when measured on the X-ray film is 14.1 mm. for Skeleton A, and 11.1 mm. for Skeleton B, a magnification of but 0.1 mm. in each instance. This is so slight as to be negligible. If, now, we measure the compacta and cavity on the film, we obtain the following results:

	<i>Compacta</i>	<i>Cavity</i>
Skeleton A	6.9 mm.	7.2 mm.
Skeleton B	6.1	5.0

The resultant index values are A, 0.96 and B, 1.2. According to Venar the index value ranges from 3.0 at birth to 1.0 at 18 months. The value 1.2 is found at about 17 months, the value 0.9 at about 24 months and thereafter.

These index values are at a variance with other estimates of age. This is of significance, and points to retardation, for not only are the proportions behind, but shaft width as well: 11.0 mm. is reached at about 11 months; 14 mm. at about 16 months. It is very probable that demineralization has retarded bone growth.

The femoral shaft index in the present instances does not help us much with chronological age. It does, however, throw considerable light on skeletal age, a problem to which we shall now turn as we consider the skeletal indications of health history.

<sup>7</sup> Y. Venar, Unpublished research on the growth in femoral width, and on the changes in the femoral shaft index, 1933.



*Health Vicissitudes.*

Mention has already been made of the transverse scars of the long bones, with particular reference to their import in the measurement of growth increments. We must consider them in a different light: their true meaning in terms of basic cause.

Todd,<sup>8</sup> Harris,<sup>9</sup> Park and Howland,<sup>10</sup> and others have discussed the problem of the cause of the transverse lines or "scars" observed not infrequently in the X-rays of the ends of growing long bones. Todd, in Part II of the *Growth and Development of the Child*, states as follows:

Certain of the deficiency diseases, notably scurvy and rickets, present very striking interferences with growth and development. Even when a child with scurvy is apparently well nourished there is an arrest of intercellular matrix formation at the growing ends of the bones which shows in the roentgenogram as a dense white line. Arrest of bony growth is the obvious consequence. Rickets does not affect the foundation substance so much as the actual texture of the bones. The secondary trabeculae, constituting the supply of mobile calcium, are absorbed . . . (p. 39).

Associated with defects in appearance of ossific centers are the rings which are evident in all long bones after exanthemata. They mark pauses in bony growth. A crowded mass of these rings at the lower end of the tibia and less frequently at the lower end of the radius indicates a feeding problem or long continued low or intermittent fever . . . (p. 89).

If the X-rays of the long bones of Skeletons A and B (plate 2) be examined the following transverse lines will be noted (measurements in mm. are distances from ends of shaft):

<i>Bone</i>	<i>Skeleton A</i>	<i>Skeleton B</i>
Dist. Radius	2 scars: 7.2, 6.0 mm.	6 scars: 12.5, 7.0, 6.0, 4.3, 3.0, 1.0 mm.
Dist. Tibia	7 scars: 15.0, 12.5, 11.5, 9.8, 7.0, 5.2, 1.5	6 scars: 16.0, 10.1, 8.2, 6.0, 5.0, 3.3
Prox. Tibia	3 scars: 9.8, 7.5, 6.2	4 scars and cessation: 14.0, 7.5, 6.0, 3.0
Dist. Femur	4 scars: 16.0, 14.2, 12.0, 2.5	2 scars: 5.0, 2.5
Dist. Fibula	Scar at end is indication of cessation of growth	5 scars and cessation: 12.0, 10.0, 6.0, 4.0, 2.5
Humerus	—	Cess. of growth, distal and proximal

<sup>8</sup> T. W. Todd, *Growth and Development of the Child* (New York, 1933).

<sup>9</sup> H. A. Harris, The growth of the long bones in childhood (*Archives Internal Medicine*, Vol. 38, 1926, pp. 785-806)

<sup>10</sup> E. A. Park and J. Howland, The radiographic evidence of the influence of cod liver oil in rickets (*Bulletin, Johns Hopkins Hospital*, Vol. 32, 1921, pp. 341-44).

In both individuals the scars of arrested development are evidence of either repeated illnesses or an intermittent illness of approximately one and a half years duration. It is not without significance that in distal tibia of Skeletons A and B there is a record of an illness at the same time: 15.0 mm. and 16.0 mm., respectively. This virtual identity does not hold for the other bones, nor is there such complete uniformity thereafter. The fact remains, however, that the onset of illness was about the same for both, and that for one and one-half years prior to death both the children were very sickly.

The problem of the nature of the illness cannot be solved so easily. The history of emigration to the West is replete with struggles against almost overwhelming odds; far more against problems of diet than the bitter enemy of expansion, the Indian whose domains were being invaded. The difficulty was not so much food *per se* but the right kind of food: a balanced diet. Malnutrition took its toll of young and old, especially the young. It seems probable, therefore, that these scars are dietetic in origin.

The skeletons offer other interesting evidence of arrest in growth. The skull of Skeleton A has been preserved so well that it has been possible to make an accurate restoration, which has subsequently been X-rayed in the Broadbent-Bolton Roentgenographic Cephalometer. In Figure 1a the lateral diameters have been traced and superimposed upon the nasion-sella with those of BS 2324(2), a physically fit and constitutionally healthy male White aged four years. In Figure 1b the facial diameters have been traced and superimposed upon the Frankfort Horizontal and the sagittal plane of the skull, passing through nasion. Sutural details have not been shown due to slight warping and marginal erosion.

The lateral view demonstrates that the cranium of A is uniformly smaller, and that, with the exception of mandible, the facial skeletons are approximately of the same stage of development with respect to length and height. The frontal view also demonstrates smaller size of cranium in A. The significance of this has been noted by Bakwin and Bakwin:<sup>11</sup>

Relative to total body length, infants with acute intestinal intoxication are smaller in their transverse dimensions (diameter of face, bigonial diameter) and in their chest circumference than are healthy infants, from the same social environment . . . (p. 402).

The face of Skeleton A substantiates the evidence of the long bones: there is a skeletal history of long-continued illness.

<sup>11</sup> H. Bakwin and R. M. Bakwin, Body build in infants III. Body build in disease (Jour Clin. Invest., Vol 10, 1933, pp. 369-403).

In this connection let us look at Figures 1c and 1d, superpositions in the FH of the contours of WRU 1801 and WRU 818.

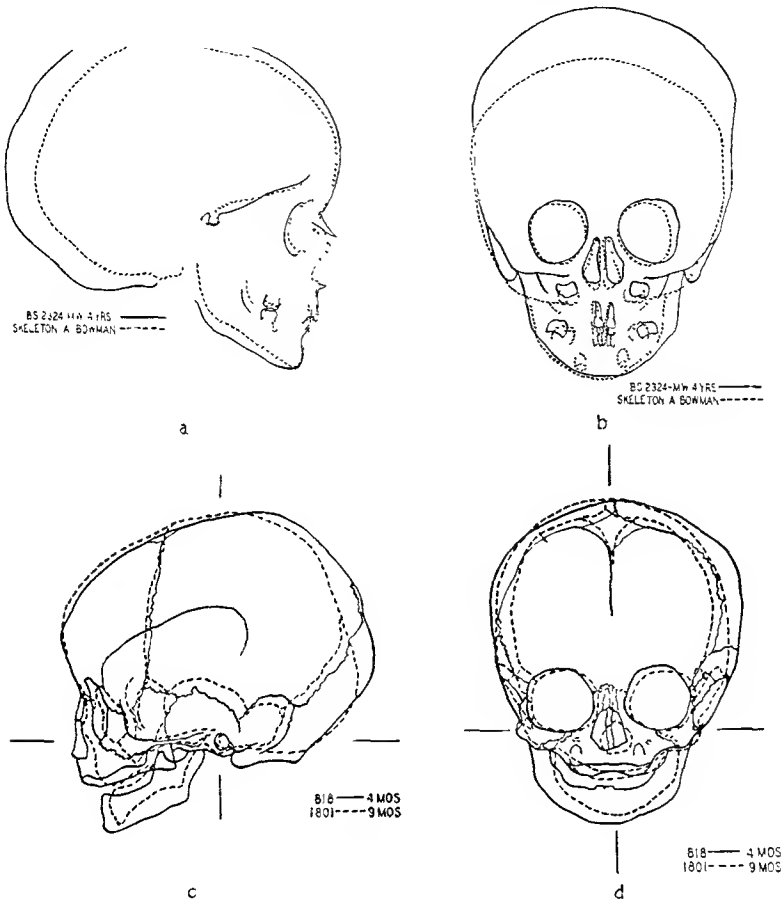


FIG. 1. a, b, Superposition of skull of Skeleton A with contour of BS 2324 (MW aged 4 yrs.): a, norma frontalis to show relative head-size, b, norma facialis to demonstrate relative transverse facial diameters.

c, d, Superposition of B 818, aged 4 months, and B 1801, aged 9 months: c, to show relative head-size; d, to demonstrate relative transverse facial diameters.

WRU 1801. Female White. Age nine months, thirteen days. Supposed to be a six months baby. Birth weight three pounds. Father ran away and child had been in boarding home for six weeks before coming to hospital. It had been ill all this time, with severe cough and fever for last week at least. Child was acutely ill and

gradually went from bronchitis to broncho-pneumonia. Also subcutaneous abscesses, otitis media, and decomposition. Blood culture short chain streptococci. This child was ill at least two months before death.

WRU 818. Male White. Age four months. Full term, normal, healthy. Sudden death, intussusception.

The contrast is striking: the one debilitated and demineralized; the other cut off in the full vigor of perfect health; the one handicapped by environment and disease, the other fostered in its postnatal growth and development; the one wasting away, the other stricken by sudden death.

The picture reminds us strikingly of the comparison of A and BS 2324(2): facial and cranial contours are absolutely and relatively smaller in the presence of wasting illness. We cannot escape the conclusion that both sets of superpositions involve identical situations, viz., the comparison of the cranio-facial proportions of a normal healthy child with those of a child in whom illness has arrested, or at least retarded, growth.

### *Summary.*

1. The two skeletons of children, A and B, excavated in an "Indian mound" near Hartsburg, Missouri, presumably buried in coffins, have been adjudged to be White, rather than Indian. Sex has not been definitely stated, but it is probable that Skeleton A, the larger, is a male.

2. The age of the children at the time of death has been estimated according to the several criteria established for the developmental age:

<i>Analysis</i>	<i>Skeleton A</i>	<i>Skeleton B</i>
Dental Age		
Eruption	Over 30 mos., under six years	Ca. 14 mos.
Calcification	Ca. 5 years	Ca. 18 mos.
Estimated stature	Four and one-half years	Slightly over 2 years
Length of long bones	4½ to 5 years	About 2 years
Femoral shaft index	Over 2 yrs., under 2½. There has evidently been retardation.	Ca. 17 mos.
Centers of ossification	Have not been considered in detail because many not recovered. Those present are in general compatible with 4-5 yrs. for A and 2 yrs. for B.	

3. The transverse lines of the long bones of both children reveal that for about one and one-half years prior to death both had been in very poor health. It is probable that improper nourishment was the cause.

4. The transverse facial diameters of A are relatively small, substantiating the conclusion of ill health.

So much for a summary of conclusions. The real import of the present study is to drive home to the archaeologist that skeletal material—even “mere bones”—can no longer be totally disregarded in his work. No matter how fragmentary the skeleton, how incompletely it is present, each part tells its own story in the recording of the age and health and physical history of the individual.<sup>12</sup>

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<sup>12</sup> All cranial X-rays used in this study have been made on the Broadbent-Bolton Roentgenographic Cephalometer. I am indebted to Dr B. H. Broadbent, Director of the Bolton Fund, for permission to reproduce the X-rays.

## A PREHISTORIC CAVE CULTURE IN SOUTHWESTERN TEXAS<sup>1</sup>

By FRANK M. SETZLER

THE state of Texas, characterized by its vast size, diversified climate, fauna and flora, surrounded by divergent aboriginal culture centers, may be considered at the present time as containing the possible solution to many important archaeological problems. Among them may be cited the northern migrations of autochthonous Mexican cultures; the southern development and western spread of a Mississippi Valley mound or proto-historic Indian pattern; the southern extremity of a Plains complex; and last, but most important for consideration here, the eastern migration and influence of the better established prehistoric Southwestern groups.

To be more specific: in the southeastern corner of Texas, around Brownsville, a phase of the Huastec culture has been identified by J. A. Mason<sup>2</sup> and myself.<sup>3</sup> In the northeast, J. E. Pearce reports artifacts similar to Mississippi Valley mound-building cultures, extending as far west as the Trinity River.<sup>4</sup> While in the northwest or Panhandle region, W. C. Holden and F. V. Studer have explored two large pueblo-like ruins in the Canadian Valley which show definite Pueblo influence.<sup>5</sup> The finding of Glaze I and Biscuit A potsherds, which have been dated from approximately 1350 to 1450 A.D. by the Douglass Tree Ring study, makes possible a definite chronology for these ruins. The more distinctive and characteristic pottery from these sites, however, would seem to indicate a predominant Plains or Mississippi Valley culture. West of Texas are the well known Basket Maker and Pueblo phases, whose evidence within the state will be dealt with here in more detail. Finally we have the recent Folsom and Yuma projectiles, discovered by E. B. Howard, associated with certain extinct animal forms, just north and west of Texas in the Guadalupe Mountains.<sup>6</sup> A few points have been recovered from certain areas within the Panhandle region.<sup>7</sup> Five hundred square miles of unexplored territory in Mexico lie to the south. It would seem obvious, then, that Texas might contain the archaeological data so necessary at the present time for developing the relationships of these seemingly unrelated groups of prehistoric Indians.

<sup>1</sup> Printed by courtesy of the Smithsonian Institution.

<sup>2</sup> Mason, *American Anthropologist*, Vol. 32. 371.

<sup>3</sup> Setzler, *Explorations and Field-Work of the Smithsonian Institution in 1931*, Publ. 3134, p. 133.

<sup>4</sup> Pearce, *American Anthropologist*, Vol. 34. 670-87.

<sup>5</sup> Holden, *Bulletin, Texas Archeological and Paleontological Society*, Vol. 5. 39-52.

<sup>6</sup> Howard, *Bulletin, Texas Archeological and Paleontological Society*, Vol. 4. 7-19.

<sup>7</sup> J. D. Figgins, *Natural History*, Vol. 27, No. 3: 229-331; H. J. Cook, *Natural History* Vol. 27, No. 3: 241; F. Bryan, *American Anthropologist*, Vol. 33: 18, 21.

Coming now to the southwestern or Big Bend region of Texas, we find that although a certain amount of investigation has been prosecuted, the published reports dealing with detailed archaeological excavations only tend to show how vast is the field yet to be studied. Since the establishment of Sul Ross State Teachers College in Alpine, Texas, during 1920, a serious attempt has been made by Victor J. Smith to keep a careful record of the material collected in the immediate vicinity. Within the last three years several short reports were written by him describing his work around Alpine.<sup>8</sup> Mr E. F. Coffin has had his excavation of Bee Cave Canyon published by the Museum of the American Indian.<sup>9</sup> The University of Texas,<sup>10</sup> Austin, and the Witte Memorial Museum,<sup>11</sup> San Antonio, have recently published reports on their respective investigations of caves near the Pecos River watershed.

In the spring of 1927, Dr F. H. H. Roberts, Jr., inaugurated near El Paso the Smithsonian Institution's program in the Big Bend.<sup>12</sup> (In this paper I am considering the region bounded on the east by the Pecos River, on the north by the Panhandle and New Mexico, the west and south by the Rio Grande and the states of Coahuila and Chihuahua, Mexico, as the Big Bend region of Texas.) Mr M. W. Stirling made a reconnaissance in 1930 around Marfa, Texas, selecting caves on the Knight ranch for future investigations.<sup>13</sup> In the spring of 1931 I continued the program by excavating one large and several small caves on the Mollie B. Knight ranch, 170 miles southeast of El Paso in Presidio County.<sup>14</sup> From March to June, 1932, excavation of caves numbers 1 and 2 on Mule Ear Peaks and the Cartledge cave near the Chisos Mountains in the extreme southwestern part of the state was completed, and two in Sunny Glen Canyon near Alpine.<sup>15</sup> Two caves were excavated during February to June, 1933, along the Pecos River near its mouth where it empties into the Rio Grande.<sup>16</sup> The data

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<sup>8</sup> Smith, Bulletin, Texas Archeological and Paleontological Society, Vol. 3: 60-69, Vol. 4: 55-62, Vol. 5: 57-65; Bulletin, West Texas Historical and Scientific Society 21: 48; unpublished manuscripts; American Anthropologist, Vol. 29: 286.

<sup>9</sup> Coffin, Indian Notes and Monographs, No. 48, 1932.

<sup>10</sup> Pearce and Jackson, Bulletin, University of Texas, No. 3327; Anthropological Papers, Vol. 1, No. 3.

<sup>11</sup> G. C. Martin, Bulletin, Witte Memorial Museum, No. 3.

<sup>12</sup> Roberts, Smithsonian Miscellaneous Collections, Vol. 81, No. 7, Publ. 3009.

<sup>13</sup> Stirling, Explorations and Field-Work of the Smithsonian Institution in 1930, Publ. 3111, p. 173.

<sup>14</sup> Setzler, *op. cit.*, pp. 133-40.

<sup>15</sup> Setzler, Explorations and Field-Work of the Smithsonian Institution in 1932, Publ. 3213, pp. 53-56.

<sup>16</sup> Setzler, Explorations and Field-Work of the Smithsonian Institution in 1933, Publ. 3235, pp. 35-37.

and conclusions herein offered are based primarily on the Smithsonian's investigations.

Even though the material culture of the prehistoric Indians in the Big Bend is rather simple and very uniform throughout, a detailed technical description of the more important artifacts will be essential. These Indians made coiled, twined and plaited baskets. The coiled baskets consist of three important types. First, baskets made with a single bundle-of-grass foundation, with yucca thread stitches split on the convex or non-work surface, sewed toward the left of the worker on the concave or inside of the basket. This is the most characteristic type in the western part of the Big Bend, but appears as a secondary type along the eastern boundary or Pecos River region, where the interlocking stitch over a single bundle foundation is more prevalent. Neither the split stitch type nor the single *bundle* foundation has ever been found among Basket Maker or Pueblo culture artifacts. While the split stitches on the non-work surface have been reported from Lovelock Cave, Nevada, and Ozark caves, as well as among the modern Maidu, Washo and Ute Indians, the foundation in each case differs from the Big Bend's single bundle foundation.<sup>17</sup>

A second coiled technique has the single *bundle* foundation with interlocking stitches. Several variations in regard to the thickness of the bundle and closeness of the stitches exist. This process is predominant along the Pecos River, but is also found farther west. It somewhat resembles a form considered by Dr Weltfish as intrusive among the typical Basket Makers, but it differs from this type, however, in having a *bundle foundation* rather than the *single rod*, which the Days found in Canyon de Chelly.<sup>18</sup>

The third type is the wrapped twined, or horizontal and vertical single rod foundation, with all the horizontal rods on the convex surface or outside of the basket, and sewn with a yucca leaf. The stitch binds only one horizontal rod to the vertical ones and does not interlock with the adjacent horizontal rod. This type has been described from only one other site, Gypsum Cave, Nevada, by M. R. Harrington.<sup>19</sup> The fragment illustrated by him in Figures 22 and 23 in his final Gypsum Cave report is an exact duplicate of this unusual type.

Plaited baskets are of the diagonal over-two-under-two weave. The edge or selvage is uniquely finished off with the characteristic split-stitch type of coiled basketry technique, described as type one of the coiled wares, with a draw string of lechuguilla fiber. This type of twill-plaited basket, except for the selvage, occurs in numerous Pueblo culture horizons.

<sup>17</sup> G. Weltfish, *American Anthropologist*, Vol. 32: 493.

<sup>18</sup> Weltfish, *Smithsonian Miscellaneous Collections*, Vol. 87, No. 7, Publ. 3169, pp. 10, 40.

<sup>19</sup> Harrington, *Papers, Southwest Museum*, No. 8, pp. 44-45.



Large quantities of checker or over-one-under-one and the over-two-under-two plaited mats occur. On the Pecos River we found an extended burial in the flesh wrapped in five painted over-two-under-two twill-plaited mats. Beside this body, folded in a similar large well-preserved mat, with red lines and dots painted on the outside, were the remains of several cremated adult bones.

The sandals may be classified under three main types, all different from those thus far reported from the Southwest proper. The first and most common type, known as the "Fish-tail," has two opposing warp elements of whole yucca leaves or lechuguilla cordage, and small flat strands braided between the warps. The second type, rectangular in shape, is much the same as the "Fish-tail" except that for thickness and durability a greater number of leaves interlock between the braided strands. The third type consists of simple braiding without the extra reinforcing strands. All types are made from the coarse yucca or sotol leaves which sometimes are shredded and sometimes retain their original form. No indication has ever been found of the typical Basket Maker or Pueblo square-toed, tightly woven and well made sandals. A few leather soles, perhaps a prototype of the moccasin, also served as a protection from the sharp jagged rocks in these canyons.

Cradles are represented by a single complete specimen and several fragments of others. These were found only in the Knight cave in the western part of the Big Bend. The perfect specimen consists of thin twigs of uniform size, approximately two feet long, bent and bundled together at each end and sewn with a two-strand two-ply twisted yucca thread, the sewing cords being at intervals of about two and one-half inches. Other types of cradles found along the Pecos River region are mentioned in the Witte Museum report.<sup>20</sup>

Numerous fragments of coiled netting indicate the use of snares. Two-ply apocynum cords were used to make aprons, with human hair cordage as tie strings. Many yards of loose lechuguilla and apocynum fibre cordage were scattered about in the deposits.

Along the Pecos River most of the burials were wrapped in fur robes, made by twisting narrow strips of hide with the fur still attached around a lechuguilla cord foundation or twisted about itself without a foundation. The fur robes are similar to those found among Basket Maker II and III artifacts.

Among the wooden specimens are objects which afford a clue to a possible chronology between the Big Bend Indians and those in the Southwest. The most important artifacts of this nature consist of the following

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<sup>20</sup> G. C. Martin, *op. cit.*, pp. 41, 42.

fragments: the proximal end or handle of an atlatl, decorated with criss-cross lines; atlatl shafts, foreshafts, and bunt-points, and numerous fragments of grooved clubs. Two small fragments of painted twined woven bags, and a rectangular piece of cloth complete and selvaged at both ends (three and one-half inches long and one inch wide) made from painted two-ply apocynum cordage, the fibres sewn together with a very light thread (unidentified) in the same manner as the unique technique on the so-called cradle, are suggestive of the Southwest. These specimens together with the aforementioned fur robes are similar to Basket Maker III and Pueblo I artifacts.

Associated with these were many cane arrowshafts and long slender wooden arrow foreshafts, pointed and tapered at one end to fit into the cane shaft, and split at the other end to receive the projectile point. In one of the Pecos River caves we found the nock end of a wooden bow. Some of these nocked shafts and foreshafts were found at a greater depth in the deposit than the atlatl fragments. This probably indicates that both the atlatl and the bow and arrow were used contemporaneously by the inhabitants of these caves. Coffin reports a similar association in Bee Cave Canyon, Brewster County, in the western part of the Big Bend.<sup>21</sup> Miss Lois Gould found a similar association of artifacts while studying part of a collection from a cave seventy miles northeast of El Paso, Texas. These artifacts consisted of typical Big Bend sandals, grooved club, atlatl bunt-point, arrowshaft and arrow foreshaft, fur and yucca cords and miscellaneous objects.<sup>22</sup>

Cane tubes, containing numerous minute seeds, had one end closed by the septum in the cane and the other plugged with lechuguilla fibres. Large pieces of wood were cut and shaped to form scoops. Split pieces of small twigs were bent into elliptical loops, the ends tied together with yucca fibres. Their purpose has not yet been definitely determined, but since the rounded ends show a certain amount of wear, I have temporarily considered them as a form of scraper. Numerous sotol and lechuguilla flower-stalks were split in half for small fire hearths, while rounded pieces of wood were used as fire drills. A number of small twigs tied together at both ends might have served as parts of a snare.

Animal, bird and fish bones, snail and mussel shells give evidence of the fauna used for sustenance. From cave number 1 at Mule Ear Peaks parts of three California condors, not associated directly with the human deposit, were identified by Doctor A. Wetmore.<sup>23</sup> It is now extinct except

<sup>21</sup> Coffin, *op. cit.*, pp. 28, 61.

<sup>22</sup> Gould, Oklahoma Academy of Science, New Series, Vol. 9, No. 456, 1929, pp. 155-59.

<sup>23</sup> Wetmore and Friedmann, The Condor, Vol. 35, No. 1: 37-38.

in certain portions of California. Ornithologists consider this bird belongs to the Pleistocene fauna. Other fragments have been found in cave debris from the Guadalupe Mountains in New Mexico, as well as in the Gypsum cave in Nevada. Numerous leg bones of the deer were fashioned into awls and other implements. Snail shells were painted and pierced for stringing, opercular bones of suckers were made into gorgets.

The stone artifacts consist of large elliptical mortars and flat metates, numerous manos, several large flint knives, and a variety of projectile points and scrapers and stone disc beads. The Pecos caves on the eastern boundary contained a much larger number and greater variety of perfect projectile points and knives than the caves farther west. Some of these notched points are similar to those recovered in the burnt rock mounds of central Texas.<sup>24</sup> A few of the artifacts besides the mats were painted with red and black lines, namely, the scapula of a deer, a small section of split sotol stalk, snail shells, large quantities of small water-worn pebbles, and a buckskin bag.

So much for the artifacts. The only skeletal remains recovered in the western portion of the Big Bend are infants. One restored skull of a child, approximately two years old, sex undeterminable, has a cephalic index of 70.3. Two restored skulls of desiccated adult female bodies, found along the Pecos, have cephalic indices of 65.3 and 67.2 respectively, indicating decidedly long-headed individuals. No evidence of deformation was apparent.

The following general conclusions, based primarily on the eight completely excavated caves, seem justified. First, the caves were used only as temporary shelters, indicated by the lack of deep deposits and absence of adobe structures within the caves. Second, the Indians were semi-nomadic, they cultivated corn, beans and squash extensively in the western part of the Big Bend but no evidence of corn has been found around the Pecos River region. They depended to a large extent upon the fauna, berries, and yucca plants to eke out their livelihood. Third, their material culture is notably uniform.

The correct allocation of these Indians to their proper chronological and cultural position cannot as yet be too definitely stated. Thus far no fired or sun-baked pottery has been found, so one reliable criterion for chronology is denied us. No European evidence has ever been recovered. The presence, however, of atlatl paraphernalia, grooved clubs, twined woven bags, fur robes, together with the bow and arrow, in caves where no apparent stratification occurs, does offer a clue. A somewhat similar association of artifacts has been established by Morris in Canyon del

<sup>24</sup> Pearce, Bulletin, Texas Archeological and Paleontological Society, Vol. 4, pl. 10.

Muerto and Roberts in Chaco Canyon, New Mexico, caves which produced late Basket Maker III and Pueblo I material. This seems to be the only indication of similarity to the Southwestern cultures or to a possible chronology. The coiled basketry and sandal types as well as numerous less important objects are entirely different in their technique from those found among Basket Maker or Pueblo remains. Where then should this material complex be placed relatively speaking?

My personal opinion, at the present time, is that we should regard the Cave-dwellers of the Big Bend region of Texas as a more or less isolated or independent group, primarily, because of the variation in similar types of artifacts and the lack of sufficient evidence showing a definite relationship with the well established Basket Makers or Pueblo Indians. The variations seem to outweigh the few obvious similarities. The Texas cave Indians may be a part of a larger prehistoric basic culture centering in northern Mexico, especially in Coahuila. Certain similarities can be found among the modern Pima, Papago, Maidu, Washo and Ute Indians, especially in the coiled basketry. Prehistoric similarities are evident in the material culture from Lovelock and Gypsum caves in Nevada. Perhaps the cave-dwellers in the Big Bend of Texas represent a cultural lag retaining the basic features of a cave culture originating in the unexplored northern regions of Mexico, while the Basket Makers, with a similar origin, developed their own specialized complex or phase after residing in the Southwest. The Indians to the east and west of the Southwest may have retained all of the basic features and were not influenced by the later Pueblo cultures. Granting this hypothesis, then the few cultural similarities, which may have been retained from the original basic pattern, would account for artifacts similar to the Basket Maker III and Pueblo I periods. If the cave-dwellers in Texas and the inhabitants of Lovelock and Gypsum caves originated from the Southwest Basket Makers, we should have to account for the complete loss of the typical coiled basketry and sandal techniques as well as all other variations.

When all the data is considered as a unit it seems to me necessary to treat this cave culture of the Big Bend region in Texas as a separate entity rather than an example of, or cultural development out of, the classical Basket Maker, at least until future excavations produce more direct connections with the Southwestern cultures. These hypotheses do not imply an antiquity for the Big Bend cave cultures greater than the Basket Maker III or Pueblo I cultures. Even a relative chronology must be very indefinite for the present.

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## SOUTHERN PUGET SOUND SALISH KINSHIP TERMS

By ARTHUR C. BALLARD

THE kinship terms in the appended list have been obtained from informants of the Puyallup, Duwamish, and Snoqualmi groups, residing in King and Pierce Counties, Washington. All speak the same language, with slight dialectic variations. These terms will in the main be valid, I believe, for adjoining groups on the south, southwest and north, whose culture is almost identical with that of the groups whose kinship terms are here listed.

There seems here to be no distinction between parallel cousin relationship and cross cousin relationship nor between other relationship on the father's side and that on the mother's side. There is no evidence of cross cousin marriage. Residence was predominantly but not invariably patrilocal. Exogamous marriage was a favored practice, almost universally followed. A certain group having a tradition of endogamy was considered "low class" by its neighbors. Polygyny was widely practised among those of high rank. Numerous related persons lived in a single communal house during the winter months. Certain derivative expressions whose exact significance has not yet been made entirely clear to me seem intended to distinguish in some degree the varied relationships arising from these practices.

In listing the terms it has seemed best to recognize two main groups, kindred by blood and kindred by marriage, and within each group to list the derivative, or compound, terms separately from the simple.<sup>1</sup>

Noteworthy is the change in nomenclature employed in certain cases upon the death of an intervening relative. Thus upon the death of one's spouse the term *sba'lotsiɔ* is substituted for *kwel<sup>xu</sup>*, in so far as any relationship is presumed to continue. Incidentally, it seems to have been obligatory for brother, or sister as the case may be, to marry the surviving spouse of the deceased. Upon the death of one's parent the surviving brother, sister or cousin of the deceased is called *yela'b*. Upon the death of *yela'b* he or she is again called *kasi''* or *pus*. Reciprocal to *yela'b* is *skula'djitaɭ*, substituted for the term *sta'laɭ*. Accordingly the expressions *yela'b* and *skula'djitaɭ* are used between elderly persons and young persons distantly related.

Certain terms, such as *kwel<sup>xu</sup>*, *sba'lotsiɔ*, *ska''*, *so'qwa* and *sxa'xa*, are reciprocal in themselves. The term *ka'ya*, grandmother, is sometimes

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<sup>1</sup> The phonetic scheme is that of the *Phonetic Transcription of Indian Languages* (Smithsonian Misc. Coll., Vol. 66, No. 6, 1916).

used by an elderly person in addressing a child. The derivative term *tcɪba'dəb* signifies not only husband of speaker's aunt but also speaker's stepfather. This double significance is apparently based upon certain marriage customs formerly prevailing. Likewise, the term *tcɪta'dəb* signifies either speaker's stepmother or wife of his uncle.

In the use of terms such as *ska''*, *so'qwa* and others, equally applicable to persons of either sex, the sex of the person to whom reference is made is indicated by the use of a demonstrative: for example, *tiɪ ska''*, that elder brother; *tsiɪ ska''*, that elder sister. In but few instances is the choice of terms used based upon the sex of the speaker. Terms in question are *xα'ltəd*, brother-in-law, and *stc'α'bac*, sister-in-law, brother-in-law. The former term is used reciprocally between speakers of the male sex only, while the latter is used not only reciprocally by female speakers but by a person of either sex in referring to a person of the opposite sex. A different usage seems to prevail in the employment of the terms *a''cɪd* and *alc*. The term *a''cɪd* is reciprocal between persons of the same sex, while the term *alc* is used between persons of opposite sex to designate a sibling or merely a friend.

Persons whose respective wives and husbands are sisters, brothers, or brother and sister, while perhaps not considered as kindred, stand in a special relationship to each other and are called *sq'o'idup*. Full brothers and sisters are called particularly *teuxsq'o''ab*. Both of these terms are built upon the root syllable *q'o'*, signifying "join," "unite," "assemble."

The reciprocal term *sxa'xa* not only designates son-in-law, father-in-law, etc., of the speaker but may also designate brother or sister of the speaker's father-in-law or mother-in-law and likewise son-in-law or daughter-in-law of speaker's brother or sister.

All blood kindred of a married person address all blood kindred of his or her spouse as *kwel<sup>xu</sup>*. He himself addresses only the remote kindred of his wife as *kwel<sup>xu</sup>*; the near kindred being called specifically *sxa'xa*, *xα'ltəd*, *stc'α'bac*, etc. Apparently *kwel<sup>xu</sup>* is the all-inclusive term for marriage relative.<sup>2</sup> Upon the death of one's spouse the surviving marriage partner calls all the blood kindred of the deceased by the term *sba'lotsɪd* and is so addressed by them. This does not apply to the blood relatives of the surviving spouse, however. Any relationship formerly existing between

<sup>2</sup> The term *kwel<sup>xu</sup>*, "marriage relative," seems to be etymologically related to the verbal forms *ukwe'liu* and *ukwe'liu'a'lik*. The former of these expressions means "he goes to another country, gets a wife and sojourns with his wife's people for a time." The latter means "he brings his wife home to live with his people." The expression is applicable to a person of either sex.

them and the blood kindred group of the deceased is regarded as severed. They are called *tsi'ləḅskayu'*, "related through the dead."

There is no exact counterpart to the English word family.

The expression *tɪ'liḅ*<sup>u</sup> preceding a simple term seems intended to restrict its meaning by indicating blood relationship or at any rate a more intimate relationship than would necessarily be shown by the term which it precedes. For example the compound term *tɪ'liḅ*<sup>u</sup> *so'qwa* may be intended to distinguish younger brother from younger cousin. Two brothers whose respective wives are mother and daughter are called *tɪ'liḅ*<sup>u</sup> *sxa'xa*.

Likewise the expression *bi'tigwə'l* following a simple term seems to indicate close association or propinquity of residence. It is known that persons of varying degrees of kinship lived in a common house. Such expressions as *tsi'ləḅska'ya*, "related through the grandmother," and *tsi'ləḅspu's*, "related through the aunt," may be intended to distinguish cross cousins from parallel cousins. These terms are seldom used, however. A further study, with concrete examples, should clarify their significance.

#### KINSHIP BY BLOOD

##### A. Simple terms

1. <i>bad</i>		father
2. <i>skoḷ</i>		mother
3. <i>kasi''</i>	Pl. <i>kaskasi''</i>	brother or male cousin of speaker's father or mother
4. <i>pus</i>	<i>pupu's</i>	sister or female cousin of speaker's father or mother
5. <i>tša'pa</i>	<i>tša'ptsapa</i>	grandfather, brother or cousin of speaker's grandparent
6. <i>ka'ya</i>	<i>ka'ikaya</i>	grandmother, sister or cousin of speaker's grandparent
7. <i>tca'biq<sup>u</sup></i>	<i>tca'btcabiq<sup>u</sup></i>	great grandfather, great grand- mother etc.; reciprocally great grandchild, etc.
8. <i>kwə'liyə''q<sup>u</sup></i> <sup>3</sup>		great great grandfather, etc.
9. <i>he'wɫyə''q<sup>u</sup></i>		great great great grandfather, etc.
10. <i>bə'da</i>	<i>bə'dbədə</i>	son, daughter: sex indicated by preceding demonstrative
10a. <i>bi'bda</i>	<i>bi'bdabədə</i>	diminutive of preceding
11. <i>sta'laḷ</i>	<i>sta'ltalaḷ</i>	nephew, niece, son or daughter of speaker's cousin

<sup>3</sup> The term *kwə'liyə''q<sup>u</sup>* is said to mean "beyond;" *he'wɫyə''q<sup>u</sup>* is said to be derived from *hek<sup>u</sup>*, "great" and another word meaning "beyond."

12. e' bats	e' byebats	grandchild
13. ska'	<sup>tu</sup> ska'' təd	elder brother-sister, elder cousin
14. so' qwa	so' qsoqwa	younger brother-sister, younger cousin
14a. so' soqwa	so' səssoqwa	diminutive of preceding
15. a' cɪd	i' hɪcɪd	sibling, friend, brother, sister: spoken by person of same sex
16. alc	a' lalc	sibling, friend, sister, brother: spoken by person of opposite sex
17. qαq <sup>hu</sup>		elder brother or sister: term of address
18. yɛla' B	yɛlyɛla' B	brother, sister or cousin of deceased parent of speaker
19. skɔla' djital	skɔlkɔla' djital	son or daughter of deceased brother, sister or cousin of speaker: reciprocal to preceding term
20. tsaya' ya	tsaya' i' aya	kinsman, kinswoman: general term, not clear whether restricted to kinship by blood

## B. Derivative terms

21. bada' ligwad	related on the father's side
22. skoi' a' ligwad	related on the mother's side
23. <sup>tu</sup> xsq' o' aB	having the same father and mother: "joined"
23a. tsi' ləBsq' o' a bi' tigwα' l	preceding term amplified: precise meaning obscure, perhaps collective
24. tsi' ləBsbə' d	having the same father: "related through the father"
24a. tsi' ləBsbə' d bi' tigwα' l	preceding term amplified
25. tsi' ləBsko' i	having the same mother: "related through the mother"
25a. tsi' ləBsko' i bi' tigwα' l	preceding term amplified
26. tsi' ləBskasi''	"related through the uncle"
26a. tsi' ləBskasi'' bi' tigwα' l	preceding term amplified
27. tsi' ləBspu' s	"related through the aunt"
27a. tsi' ləBspu' s bi' tigwα' l	preceding term amplified
28. tsi' ləBstsa' pa	"related through the grandfather"
28a. tsi' ləBstsa' pa bi' tigwα' l	preceding term amplified
29. tsi' ləBska' ya	"related through the grandmother"
29a. tsi' ləBska' ya bi' tigwα' l	preceding term amplified
30. ska'' bi' tigwα' l	elder brother, term amplified, perhaps collective
31. tɪ' lix <sup>u</sup> ska''	elder brother, perhaps as distinguished from cousin



- 31a. tɪ'liḡ<sup>u</sup> ska'' bi'tigwα'l  
 32. so'qwa bi'tigwα'l  
 33. tɪ'liḡ<sup>u</sup> so'qwa  
 33a. tɪ'liḡ<sup>u</sup> so'qwa bi'tigwα'l  
 34. a''cɪd bi'tigwα'l  
 35. tɪ'liḡ<sup>u</sup> a''cɪd  
 35a. tɪ'liḡ<sup>u</sup> a''cɪd bi'tigwα'l  
 36. a'lc bi'tigwα'l  
 37. tɪ'liḡ<sup>u</sup> a'lc  
 37a. tɪ'liḡ<sup>u</sup> a'lc bi'tigwα'l

combination of preceding terms  
 younger brother: term amplified,  
 perhaps collective  
 younger brother, perhaps as dis-  
 tinguished from cousin  
 combination of preceding terms  
 sibling or friend of same sex as  
 speaker: term amplified, perhaps  
 collective  
 sibling or friend of same sex as  
 speaker, probably indicating close  
 relationship  
 combination of preceding terms  
 sibling or friend of opposite sex to  
 speaker: term amplified, perhaps  
 collective  
 sibling or friend of opposite sex to  
 speaker, probably indicating close  
 relationship  
 combination of preceding terms

## KINSHIP BY MARRIAGE

## A. Simple terms

- |                         |                              |   |
|-------------------------|------------------------------|---|
| 38. tcist <sup>xu</sup> | Pl. tci'stcist <sup>xu</sup> | husband (Puyallup dialect)  |
| tcict <sup>xu</sup>     | tci'ctcict <sup>xu</sup>     | husband (Snoqualmi dialect)   |
| 39. tcα'gwac            | tca'hagwαc                   | wife  |
| 40. sxa'xa              | sxa'xaxa                     | son-in-law, daughter-in-law of<br>speaker, speaker's brother or sis-<br>ter; reciprocally father-in-law, etc. |
| 41. xα'ltəd             | xα'lxα'ltəd                  | brother-in-law, husband of one's<br>cousin, man speaking  |
| 42. stc'α'bac           | stc'a'habαc                  | sister-in-law, brother-in-law, wom-<br>an speaking; sister-in-law, man<br>speaking                            |
| 43. kwel <sup>xu</sup>  | kwe'lkwel <sup>xu</sup>      | related by marriage: general term   |
| 44. sba'lotsid          | sba'lbalsid                  | surviving marriage relative after<br>death of spouse: reciprocal term   |

## B. Derivative terms

45. tsi'ləbstci'st<sup>xu</sup>  
 46. tcɪlba'dəb  
 47. tcɪlta'dəb
- co-wife, "related through the hus-  
 band"  
 step-father; husband of speaker's  
 aunt  
 step-mother, wife of speaker's  
 uncle

48. tcɿbada'ɓ stepson, stepdaughter, son or daughter of wife's brother or sister. son or daughter of husband's brother or sister: reciprocal to the two preceding terms
49. tcɿtsa'pa<sup>AB</sup> step-grandfather, husband of grandparent's sister
50. tcɿka'ya<sup>AB</sup> step-grandmother, wife of grandparent's brother
51. tcɿle'bats<sup>AB</sup> step-grandchild, grandchild of speaker's wife's (or speaker's husband's) brother or sister. reciprocal to the two preceding terms
52. tsiɿ<sup>w</sup>e'bats husband or wife of grandchild of speaker or speaker's brother or sister; term possibly reciprocal
53. tcɿsxa'xa<sup>AB</sup> son-in-law or daughter-in-law of speaker's wife's brother or sister, husband of speaker's stepdaughter: reciprocally uncle by marriage or stepfather of speaker's wife, etc. kin by marriage; term used in special sense to designate stepbrothers and stepsisters
54. kwe'l<sup>xu</sup> bi'tigwα'l "related through the dead," previously related by marriage
56. sq'o'idup persons whose wives are sisters; whose husbands are brothers; whose respective husband and wife are brother and sister: term derived from root syllable q'o', "join"
57. tsi'ɿɓsto'bc man whose wife is sister to the wife of another (?); "related through the man"
- 57a. tsi'ɿɓsto'bc bi'tigwα'l preceding term amplified
58. tsi'ɿɓsɿa'dai woman whose husband is brother to the husband of another (?); "related through the woman"
- 58a. tsi'ɿɓsɿa'dai bi'tigwα'l preceding term amplified
59. tɿ'liɿ<sup>u</sup> sxa'xa son-in-law related by blood to parent-in-law

## KNOTLESS NETTING IN AMERICA AND OCEANIA

By D. S. DAVIDSON

THE question of trans-Pacific influences in American cultures has been seriously debated for a number of years. Those who favor a trans-oceanic movement have pointed out many resemblances and several striking similarities between certain culture traits of the New World and Oceania. The theory of a historical relationship between these appearances is based upon the hypothesis that independent invention and convergence in development are not reasonable explanations either for the great number of resemblances or for the certain complexities found in the two areas.

The well-known objections to the trans-Pacific diffusion theory can be summarized as follows:

1. That many of the so-called similarities at best are only resemblances between very simple traits which might be independently invented or discovered.

2. That most of the traits in question are not present among the Polynesians, the great navigators of the Pacific and the only Oceanic people known either to have made great voyages, or to have been equipped in early times with watercraft capable of making extended journeys, but limited to the Australians, Melanesians or Indonesians who are either too poorly equipped with watercraft to have made such excursions or so far removed geographically from America that a trans-Pacific movement, leaving no traces in intermediate areas, would seem most unlikely.

3. That most of the similarities are not concentrated severally either in the Americas or in Oceania but individually are so localized in each region that the diffusion of a large number of traits would entail many independent crossings of the Pacific, a conclusion which seems hardly plausible.

4. That most of the traits involved do not appear on the west coast of the New World, the most likely place for them if brought across the Pacific, but are distributed largely east of the cordillera.

5. That there is no evidence to show that the peoples in the western Pacific who now possess the traits ever reached Polynesia, through which they must have passed if they ventured to the Americas.

6. That the records of the Polynesians do not refer to any visit of peoples passing through their region and that if diffusions took place prior to the arrival of Polynesians, they would have happened at a time when watercraft were hardly developed to the point of successful trans-oceanic navigation.

In spite of these objections, many of which are formidable, it is to be

admitted that many of the resemblances are indeed puzzling, and the chance that some traits may have been carried across the Pacific purposely or inadvertently must be recognized. However, it should be emphasized that the establishing of a trans-Pacific derivation of one trait does not prove that others have had a similar history, nor, vice versa, does the proof that one trait developed in America constitute evidence that the others were also independently invented in the New World.

It is important to note that practically all of the traits listed as examples of a direct historical unity between America and Oceania are perishable or non-material, with the result that archaeology has played but little part in the controversy.<sup>1</sup> This condition is disconcerting to those who are interested in an unbiased appraisal of the situation, although it may be considered as advantageous to the arguments of the extremists of both sides who either find no disproof that certain traits may not have been introduced along the western coast of America where they are now lacking, or no evidence contrary to the supposition that these traits may be but relatively recent independent developments in the American areas they now occupy.

As yet many of the objections to a trans-Pacific route of diffusion, aside from the fact that there is no evidence to show how or when such movements could have happened, have been based upon the supposition that many of the traits could have developed locally in the New World, but very little attention has been given to the evidence in Oceania which may indicate the improbability that certain traits could have been brought to America. It has been more or less tacitly assumed by both sides that the contemporary distributions in the Pacific have been more or less unchanged during the time elapsed since the supposed trans-oceanic diffusions took place, a supposition which seems contrary to logic as well as to the little evidence which is available. Although archaeological material is most fragmentary from Oceania and, in spite of the fact that what we have does not bear upon the problems of the trans-Pacific controversy, there seems to be no doubt but that there has been a constant eastward diffusion of many traits from Indonesia to Melanesia. A few centuries ago, therefore, the eastern boundaries of many of these traits may have been farther west than we observe at present and successively 1000, 1500 or 2000 years ago the distribution may have been much more restricted. Indeed some traits were possibly not in existence in these times. Whether it will ever be possible to come to any satisfactory conclusion as to the antiquity in Oceania of many of the traits concerned in the trans-Pacific controversy

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<sup>1</sup> To mention only a few: bark cloth, blow guns, bullroarers, masks, pan-pipes, poison for arrows and for fishing, chewing of narcotics, couvade, etc.

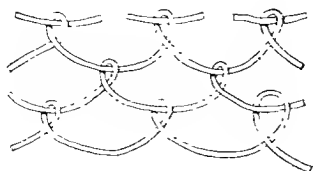
remains to be seen. Certainly the probabilities can be determined in some instances. Investigation into this field may throw important light upon the question of trans-oceanic cultural relationships.

#### KNOTLESS NETTING

There is one trait which generally has not appeared in the lists of resemblances between the New World and Oceania and that is knotless netting. In the Pacific it is found in most of New Guinea and some of the adjacent islands, is present throughout the eastern half of Australia, occurs in the Gilbert Islands, and similar techniques in stiff basketry are used somewhat differently in Indonesia where they appear to be relatively unimportant. In America, knotless netting is not localized, like many of the parallels to Oceanic traits, but is distributed, with the exception of a few regions, from the habitat of the northernmost Athabascan tribes to Tierra del Fuego. It seems to be prominent along the west coast of South America as well as east of the cordillera and is found archaeologically in pre-Incan graves at Arica. In addition, it is important to note that not only is knotless netting applied primarily to the construction of soft pliable carrying bags in Oceania and the Americas, but also that in the two regions the techniques employed are identical in some cases and very similar in others. On the basis of all factors, ethnological, archaeological, geographical and technological, it would seem that knotless netting lends itself much better to a discussion of the possibilities of trans-Pacific influence in American cultures than do many of the traits usually given as examples of such a movement.

Knotless netting is a subject which has received very little attention in Oceania while in the New World, in spite of its widespread distribution, it has been hardly more than mentioned by a few writers for localized appearances. It is important to note that knotless netting is well suited to the development of many variations in technique. However, in so far as available material indicates, only a few of the possible arrangements seem to have been discovered, or if discovered, accepted by native peoples as patterns to be perpetuated.

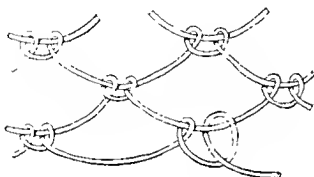
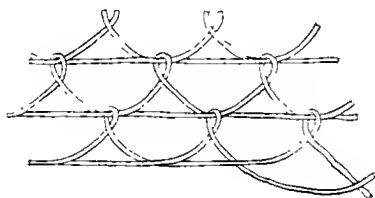
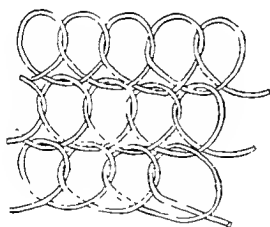
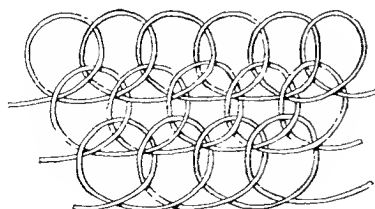
Of the techniques observed in Oceania and America there seem to be three basic patterns from which all the others have been derived, and of these three, one, the Simple Loop, is fundamental to the other two. All knotless netting techniques, therefore, seem to be derived from one basic pattern. It is convenient, however, to classify the varieties under the three-fold division of Type I, Simple Loop; Type II, Loop and Twist(s); and Type III, Hourglass. Within this classification, the remaining patterns can be arranged as sub-types and varieties as in the following diagrams.

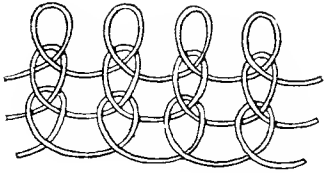
*Type I*

Simple loop

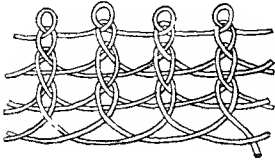
North America,  
South America,  
Melanesia and  
Australia

(see table 1)

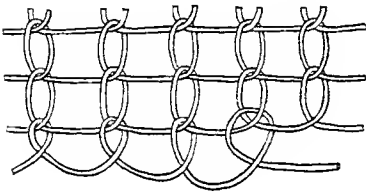
*Type I**Variety A*Double simple  
loopGran Chaco  
(Ashmuslay,  
Nordenskiöld,  
p. 196, fig. 4c;  
Lengua,  
Singer).*Type I**Variety B*Simple loop with  
two inter-twining  
and alternating  
strandsColombia  
(Mochila,  
Pueblo Viejo).  
Singer.*Type I**Sub-type 1**Variety A*Simple loop in-  
terlocking with  
one loop on all  
sidesEcuador and  
northeastern  
Peru  
(Yahua R.  
and mouth of  
Napó R.).  
Singer*Type I**Sub-type 1**Variety B*Simple loop in-  
terlocking with  
each adjacent  
loop of its own  
row and with  
two loops of the  
adjacent rowsNakanai district  
New Guinea.  
Graebner



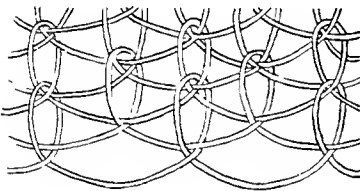
*Type I* Half-hitch East coast of  
*Sub-type 2* around half- Dutch New  
*Variety A* hitch Guinea to  
Finschhafen.  
Graebner.



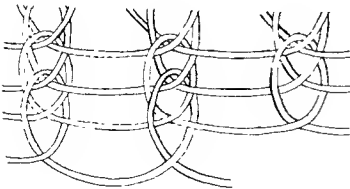
*Type I* Half-hitch Northeastern  
*Sub-type 2* around half- Brazil  
*Variety B* hitch with two (Patamon  
and Makusi)  
intertwining Roth, p. 440.  
strands (used in  
belts)



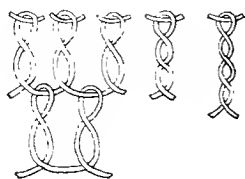
*Type I* Half-hitch Costa Rica  
*Sub-type 3* through half- (Bribri,  
*Variety A* hitch Valiente?).  
Skinner, pl. 5.



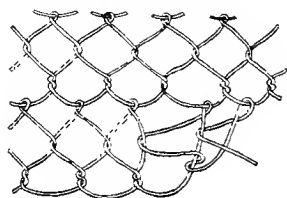
*Type I* Half-hitch Costa Rica  
*Sub-type 3* through half- (Bribri).  
*Variety B* hitch with two Skinner, pl. 7;  
alternating and North coast of  
intertwining New Guinea  
strands and New Britain  
(Baining  
tribe).  
Graebner.



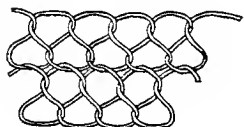
*Type I* Half-hitch Costa Rica  
*Sub-type 3* through the (Bribri).  
*Variety C* half-hitches of Skinner, pl.  
two adjacent 11.  
rows



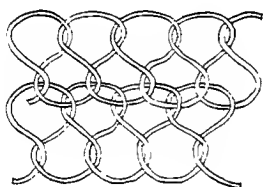
- Type II*      Loop and twist(s)      North America, South America, New Guinea and Australia  
(See table 1.)



- Type II*      Simple loop interlocking with adjacent loops on each side, thence with a double twist about itself      Venezuela. Singer.

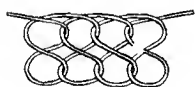


- Type III*      Hourglass pattern      North America, South America, Melanesia, Australia.  
(See table 2.)

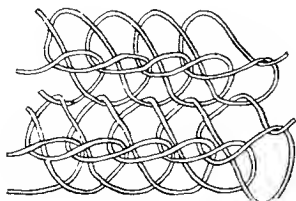


- Type III*      Hourglass interlocking with two loops of each adjacent row      New Guinea. Graebner.

Note: There is an error in the accompanying figure. The working strand should be brought forward through both loops of the preceding row rather than through only one loop.



- Type III*      Figure of eight      Maya of southern and central Honduras. Thompson, pl. 11.



- Type III*      Hourglass with lower loop interlocking with two adjacent loops to the right      Gran Chaco (Lengua and Charoti), Northeastern Peru (Yahua R.). Singer.



TABLE 1. SIMPLE LOOP AND LOOP AND TWIST TECHNIQUES

Bags unless otherwise indicated

	<i>Simple loop</i>	<i>Loop and twist</i>	
NORTH AMERICA NORTH OF CENTRAL MEXICO			
Northern Maidu (caps)	x	x	Dixon, 1905, p. 144.
Karuk		x	Harrington, J. P., pl. 11b.
Hupa	x		Goddard, pl. 6.
Pima and Papago (carrying frames)	x	x	Mason, 1902, pp. 248, 294; Kissell, p. 227, fig. 78.
Kodiak Island (gamebags)	x		Mason, 1887, pp. 210-11.
Mackenzie (muskemoots and hunting bags)	x	x	Mason, 1902, p. 249.
Dogribs (babiche wallets)	x		Mason, 1902, p. 379.
Huichol, and Tarahumare (?) (pack net and netted shield of a game)	x		Lumholtz, I: 240 (?); II: 87, 203.
*Ozark Bluff Dwellers	x		Harrington, M. R., p. 11.
*Hopewell Mounds	x	x	Mason, 1902, p. 380.
*Brewster Co. Rock Shelter, Texas	x	x	Coffin, p. 41.
*Val Verde Co., Texas	x	x	Pearce and Jackson, p. 93.
*Shumla Caves, Big Bend Area, Texas	x	x	Martin, pp. 48-49.
*Basket Maker II and III	x		Guernsey and Kidder, 1921, p. 65; Guernsey, p. 116.
*Prehistoric Pueblos I and II (?), III, IV (bags, gaming wheels, sandals, leg- gings, caps)	x		Kidder and Guernsey, 1919, fig. 45, pls. 34b, 46c; Guernsey, p. 116.
Montagnais-Naskapi (except in far north where hare is lacking)†	x		Dr F. G. Speck.
Têtes de Boule, Grand Lake Victoria†	x		Fieldnotes.
Eastern Cree†	x		Dr J. M. Cooper, and Miss Regina Flan- nery.
Eastern Cree and Northern Saulteaux†	x		Skinner, 1911, p. 35.
Saulteaux and Cree†	x		Dr A. I. Hallowell.
Slavey,† Bush Cree of Meadow Lake, and Bungi (blankets)	x		Museum of the American Indian.

\* Archaeological evidence.

† Blankets and garments.

	<i>Simple loop</i>	<i>Loop and twist</i>	
Hare, Satudene, Dogrib, Kutchin, Slavey and possibly all other northern Atha- bascans (blankets)			Dr C. B. Osgood; Os- good, 1933, p. 58; MS.

## SOUTHERN MEXICO AND CENTRAL AMERICA

Miskito, Twahka, Panamaka, Ulwa and neighbors (Honduras and Nicaragua)	x		Conzemius, p. 52.
Bribri (Costa Rica) and Valiente (?)	x		Skinner, p. 6, pl. 8.
Common throughout Central America	x		Mason, 1887, p. 210.

## SOUTH AMERICA

*Ica (Ocucaje)	x		O'Neale and Kroeber, p. 53, pl. 6, b.
*Arica (single and double twists)	x	x	Uhle, pp. 6, 9, 12, 32, pl. 25, 1; pl. 8, fig. 2.
*Atacama Desert (woolen hat)	x		Singer.
Jivaro	x		Kreiger, pl. 36.
Pachacamac	x		Schmidt, p. 546.
Tucano of Uaupes R. (scoop-net), Len- gua, Xingu area (Kalapalu, Kuluene R. also masks), Goajira, Mochela and Santa Maria (Colombia), Venezuela	x		Singer.
Chaco, Ashluslay, Choroti, Mataco, Northwestern Brazil (Rio Negro and Rio Yapura), Botocudo, Guayaki, Ijca, Kögga, Motilone, Araucani- ans, Jivaro, Oldest graves at Arica*	x		Nordenskiöld, 1919, pp. 195-202.
Western tributaries of the Rio Negro	x		Van Gennep.
Arawak, Uaupes R. (fish-net)	x		Roth, pp. 198-99, 321, pl. 48, fig. 61.
Brazil, Chiriqui	x		Mason, 1894, p. 487; 1902, p. 532 and pl. 129.
Yahgan, Alacaluf, Chono (basket style)	x		Lothrop, pp. 133-34.
Alacaluf, Chono, Onas; also Araucanians but lacking among Tehuelche	x		Cooper, 1917, p. 204; 1925, pp. 412, 416.
Patagonia, Peru, Tierra del Fuego	x		Mason, 1889, pp. 210-11.
Goajira (Colombia) (single twist)		x	Singer.
Maracaibo (Venezuela) (single twist)		x	Singer.
Fundacion, Columbia (open saddle bag)		x	Singer.

*Simple Loop  
loop and  
twist*

with some rows of four twists, some of three twists)		
Santa Marta, Colombia (basket-like objects, with double or triple twists)	x	Singer.

OCEANIA

Australia: Queensland, New South Wales, Victoria, South Australia, Central Australia, North Australia	x	Davidson, p. 260.
Australia: Queensland, New South Wales, Victoria, eastern South Australia	x	<i>Ibid.</i> , p. 263.
New Guinea: Strickland R., Geelvink Bay, Goodenough Bay and hinterland, Dutch New Guinea	x	Graebner, pp. 26 <i>et seq.</i> , Davidson, p. 260.
New Guinea: Fly R., Papuan Gulf and Strickland R.	x	Davidson, p. 263.
Gilbert Islands	x	Graebner, p. 26.

TABLE 2. HOURGLASS PATTERN

Bags unless otherwise indicated

Mayas of Southern and Central British Honduras	Thompson, pl. 11.
Ashluslay, Choroti, Huari, Mataco, Tapiete, Toba, Tsirakua	Nordenskiöld, pp. 200-201; Radin, p. 938.
Ronuro tribe (Xingu area), Chaco, Chamacoco, Lengua, Mbaya. Similar (?) technique, coast of Peru (Nat. Museum, Stockholm)	Nordenskiöld, pp. 202.
Said to be lacking among Yuracare, Chimane, Guarayu, Chama, Absahuaco, Chacobo, Huanyani, Chiriguano, Chane, Araucanians, Karaja, Caingua, northwestern Brazil, Roraima country, Jivaro, Botocudo	<i>Ibid.</i> , pp. 239-40.
Huari, Bakairi and neighbors	<i>Ibid.</i> , pp. 239-40.
Choroti, Mataco, Misiones, Lengua, Kajabi (Xingu), Tupi (locality?), Mundurucu (hoop-bag), Cayapa (money pouch), French Guiana, Angayti (Paraguay-hammock), Chamacoco (also masks and hammocks), Motilone, Lengua, Guiana.	Singer.
New Guinea: Eilander R., Astrolabe Bay, Huon Gulf, Finschhafen, Collingswood Bay, Cloudy	Graebner, p. 26 <i>et seq.</i> ; Davidson, p. 265.

Bay, Redscar Bay, Ope R., Geelvink Bay to  
northeastern New Guinea, Papuan Gulf to  
Massim area.

New Britain (Baining tribe)

Graebner, *op. cit.*

Now a glance at the list seems sufficient to indicate that many of the patterns are unique and localized and, therefore, have no basis for comparison with appearances in other regions. Unless it will be necessary to revise this compilation when additional material becomes available, it would seem permissible to exclude from further consideration the following techniques which are found only in restricted distributions in America or in Oceania.

Type I	Variety A	Gran Chaco area
Type I	Variety B	Colombia
Type II	Sub-type 1	Venezuela
Type III	Variety A	North coast of New Guinea
Type III	Sub-type 1	British Honduras
Type III	Sub-type 2	Gran Chaco

Of the remaining varieties and sub-types there are a number which show trans-oceanic similarities but which are not identical. These include:

*Type I, Sub-type 1.* In this group there are two varieties, one found in South America, the other in Oceania. The former, Variety A, consists of a simple loop which interlocks with each adjacent loop in its own row and with one loop in each adjacent upper and lower row. Variety B differs only in that it interlocks with two loops instead of with one loop of the adjacent upper and lower rows. Variety A is the simpler but is not necessarily an intermediate step between the basic Simple Loop and Variety B. Granted the presence of the Simple Loop, it would seem to be an easy matter to invent either variety independently. Independent development is also indicated by their distributions. Variety A has been found only in a small area in northeastern Peru and eastern Ecuador, east of the cordillera. Variety B seems to be restricted to the Nakanai area of New Guinea. In view of the direct relationship of each to the Simple Loop, and of their localized distributions, it seems reasonable to believe that the two similar patterns are relatively recent in origin and that they have resulted from independent parallel developments from the Simple Loop within the two localities where found. Indeed, even if it will be found that both varieties are made in the two regions the theory of parallel development, to my mind, would still be the only reasonable explanation for such simple processes.

*Type I, Sub-type 2.* In this group there are also two varieties, A and B, the former found from the east coast of Dutch New Guinea to Finschhafen,

where it is used in bags; the latter in the Patamon-Makusi area of north-eastern Amazonia, where it is employed in the manufacture of belts. Aside from the fact that in South America two alternating strands are used, the two techniques are identical in that half-hitches are taken about the half-hitches of the preceding row. Technologically, it seems rather obvious that this technique is a direct variant of the Simple Loop as well as one which might easily occur independently. The localized distributions of these two varieties would seem to indicate another example of recent parallel developments in South America and Oceania.

*Type I, Sub-type 3.* Sub-type 3 differs from Sub-type 2 in that the half-hitches or bights are taken through the half-hitches of the preceding row rather than around them.

In this group there are three varieties, A, B, and C, all of which are found among the Bribri of Costa Rica, but of which only B has been reported in Melanesia, where it is present along the north coast of New Guinea and among the Baining tribe of New Britain.

The most simple in appearance is Variety A which has been reported only for Costa Rica. Its relationship to and its derivation from the Simple Loop can be plainly seen. Variety C, also limited to Costa Rica, in so far as we know, seems obviously a development from Variety A. The principle of construction is the same except that the working strand is taken through the half-hitches of two adjacent rows rather than through the half-hitch of only one row.

Variety B differs from Variety A in that two alternating strands are employed to the effect that a half-hitch is taken (1) through the half-hitch of the previous row of the same strand and (2) about each pendant loop of the two adjacent rows of the alternate strand.

In spite of the fact that this technique is the same in both New Guinea and Costa Rica, there seems to be no reason for suspecting historical relationship between these two appearances. In the latter region, both B and C appear as likely derivatives from A, which in turn, it cannot be doubted, is only a slight alteration of the Simple Loop.

In Oceania, it is quite possible that B may represent a direct variation of A, although it would not be surprising to learn that A is also in use or was formerly present. In view of the restricted distribution of this technique in the two areas, further support is given the contention that convergence or parallel development is responsible for these appearances.

#### ALTERNATING STRANDS

The use of two alternating strands is another elaboration which is common to both Oceania and America. In the former region, it has been

reported from the north coast of New Guinea and nearby New Britain (Type I, Sub-type 3, Variety B). In America it is found in Costa Rica (I, 3, B), Colombia (I, B), and northeastern Brazil (I, 2, B). It is possible that these American appearances may be continuous in distribution and that the custom of using alternating strands has had but one origin in the New World. Such a possibility is by no means certain, however, for what we have spoken of as two alternating strands may actually be or originally may have been either two separate strands or only one strand which alternates the points of attachment as it spirals row by row. It is quite possible, therefore, for this trait to have originated in one of two different ways and to have been instigated by a variety of causes: experimentation, accident, or repair. There seems to be no reason for regarding the appearances of the use of alternating strands in the New World and Oceania as indicating historical continuity. The process is simple and apparently might occur quite naturally to knotless netters.

#### THE BASIC PATTERNS

We have now to consider the three basic techniques, the Simple Loop (Type I), the Loop and Twist (Type II), and the Hourglass pattern (Type III). All are found in Oceania and in America and the patterns of their construction are identical. As we have already seen, each seems to have given rise independently to certain variants in both regions. It is to these basic patterns, therefore, that we should look for any possible historical relationship between the knotless netting of the two regions.

*Type III.* The Hourglass pattern, it seems clear from a technological point of view, is based upon the Loop and Twist technique (Type II). In Oceania such a derivation is indicated geographically in the Australian distributions.<sup>2</sup> In the New World, however, the contemporary distributions, in so far as we have data, do not show this derivation so obviously. The Loop and Twist pattern occurs in several places north of Mexico and in Colombia and Venezuela, but we have no information concerning its presence in Central America or in other parts of contemporary South America. Archaeologically, however, there is material to show the relative ages of the two patterns, for the Loop and Twist technique has been recovered from a number of ancient deposits in North and South America, whereas the Hourglass technique appears only in existing cultures. At Arica, the nearest archaeological site to the contemporary appearances of the Hourglass pattern, both the Simple Loop and the Loop and Twist techniques occur in sufficient quantities to leave no doubt but that the

<sup>2</sup> Davidson, p. 266.

Hourglass pattern was not made at the time these pre-Incan deposits accumulated. The suspicion that the Hourglass technique may be not more than several hundred years old on the west coast of South America would seem, therefore, to have some foundation, although we are in need of additional archaeological data from other regions.

In Oceania, on the other hand, we have no stratified remains but the chronology of (1) Simple Loop, (2) Loop and Twist, and (3) Hourglass patterns appears to be established on distributional grounds as well as by technological considerations. There can be no doubt but that the Hourglass technique is the most recent of the three, but its actual age cannot be even approximately given.

In view of what seems to be a very recent appearance in western South America and a relatively recent beginning in Oceania, it would seem that the appearances of the Hourglass pattern in the two areas cannot be historically related. It would be hardly possible for influences to have emanated from one area to the other within the past millennium or 1500 years without leaving traces in other areas or without some record being incorporated in the traditions of some people between South America and Melanesia.

However, we have no right to assume that the Hourglass technique has occupied the same distribution in New Guinea during the past centuries as that noted today. Graebner has pointed out some indications of its eastward diffusion in New Guinea and there can be no doubt but that it has been carried to Australia on the south and to New Britain on the north.<sup>3</sup> It appears never to have reached other regions east of New Guinea.

It is quite possible that the point of origin for the Hourglass technique in Oceania is not in New Guinea, for we find the same pattern appearing occasionally as a single row in the stiff basketry in Indonesia.<sup>4</sup> It is impossible at present to affirm or deny the possible relationship between this appearance and the similar pattern in knotless netting, but the chance that the technique may have arisen in Indonesia must be recognized. Whether the origin was in western New Guinea or in Indonesia it will be seen that a diffusion toward America has been in process, but that only a beginning had been made and that several thousands of miles of ocean are still intervening.

In the New World no direction of diffusion has been determined. The technique is widely distributed from British Honduras to the Xingu-Gran Chaco area with appearances also in the Guianas and in Ecuador. The

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<sup>3</sup> Graebner, p. 29; Davidson, pp. 268, 299.

<sup>4</sup> Lehmann, p. 191.

most intensive use at present seems to be in the Xingu-Gran Chaco region and it is possible that the few occurrences west of the Andes may have diffused from that region.

We cannot be reasonably certain that there has been only one development of the Hourglass pattern in America, for the appearance in British Honduras seems to be quite isolated from Ecuador and the Guianas, the nearest known other places where this technique now occurs. Knotless netting techniques, however, are poorly known for Central America and it is possible that the Hourglass pattern may come to light in the intermediate areas. In addition, we must not overlook the possibility that Arawak or Carib influences may be responsible for the Honduran appearance.

With a relatively slight antiquity for the Hourglass pattern suggested for both New Guinea and South America there would seem to be no reasonable basis for believing that this technique is not indigenous to both regions. Certainly there is no evidence to indicate that a trans-Pacific diffusion could be responsible. Technologically, there is no need for a diffusion theory, for a Loop and Twist basis, found in both areas, seems sufficient to explain the parallel development. On the basis of our conclusions the similarity of zigzag ornamentation appearing upon the bags of the north coast of New Guinea and those of the Gran Chaco, for which Graebner has implied a historical relationship,<sup>5</sup> must be regarded as fortuitous.

*Type II.* The Loop and Twist technique, obviously a slight variation of the Simple Loop, appears in widely separated areas in the New World, particularly in North America. So far it has not been reported for Central America, while in South America its contemporary use seems to be confined to Colombia and Venezuela. It would not be surprising, however, to find that such a simple technique is much more widespread than present evidence indicates.

Archaeologically we have seen that it occurred at Arica in South America. In North America a much greater antiquity has been determined by the finding of fragments in the Hopewell Mounds and in cave remains at Shumla Cave and in Brewster County, Texas. Since these latter remains show similarities to Basket Maker culture, a considerable antiquity is indicated.

It is possible that all these ancient appearances have diffused from a common point of origin, but it seems much more likely, in view of the simplicity of this technique and the different manners of its use, that it has been discovered in a number of places. For instance, in the Mackenzie

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<sup>5</sup> Graebner, p. 29



basin and among the Pima, its use seems to be more or less decorative, to relieve the otherwise monotonous series of rows of simple loops. In Colombia the peculiarities of saddle bags seem to have encouraged the development of multiple twists. In other areas it seems to have been the sole technique in a bag, as in Australia and New Guinea. It is impossible to comment upon the ancient uses since, for the most part, only fragments are available.

In the Pacific, the Loop and Twist technique is found in Australia and New Guinea and appears to have developed in the latter region, or possibly in some unknown area farther west. All facts indicate that it diffused to Australia prior to the diffusion of the Hourglass pattern to that region. In the Oceanic specimens, this technique, when used, is consistently employed throughout one bag.

We have no evidence to indicate the antiquity of the Loop and Twist technique in the Pacific. The most we can say is that it appears to have preceded the Hourglass pattern and to have developed from the Simple Loop. Presumably, however, there has been an eastward diffusion in New Guinea which never passed farther than the eastern part of the island.

From the Pacific point of view, therefore, there seems to be no indication that this technique could have diffused to America. From the American point of view we may feel quite certain that there could have been no trans-Pacific navigation early enough to have introduced this technique to the ancient cultures of Texas. Furthermore, the Simple Loop is known to have been used as early as Basket Maker II and with this background there is no need for any derivation of the Loop and Twist technique from some non-American area. Although a more or less continuous distribution would seem to indicate a unitary origin for the Loop and Twist technique in Oceania, it seems quite possible that there may have been a number of independent developments in America.

*Type I.* The Simple Loop, as seems clear, is the foundation for all the varieties of knotless netting considered. In Oceania, it occupies the most widespread distribution, being found throughout eastern Australia, in the regions occupied by the Loop and Twist and the Hourglass patterns, as well as in areas peripheral to them; in a large but sporadic distribution in New Guinea and also in the Gilbert Islands. There are no indications of antiquity in these regions since no archaeological remains have been discovered. However, the distributions support the theory that this basic technique has given rise to the more complex forms, and also indicate that this oldest pattern, in reaching the Gilbert Islands, has been carried farther east than any of the later varieties. Such a distribution is consistent with the material we have already surveyed which showed that knotless netting

has been in the process of invading the western Pacific and that the directions it has followed, if toward America, are only accidentally so.

In the New World, the Simple Loop is also the most widespread. Among living peoples, it is sporadically distributed from Alaska to Fuegia. Archaeologically it appears at Arica in South America and in the various deposits in North America as early as Basket Maker II.

The appearance of knotless netting in Basket Maker deposits is a fact of prime importance to our discussion, for there can be no doubt but that trans-oceanic navigation was impossible in such early times, as well as possibly for some millennia following, depending on the date finally given this culture.

There would seem to be no possibility, therefore, that there can be any trans-Pacific relationship between the appearances of the Simple Loop in America and Oceania. Indeed, knotless netting may not have been known in the Pacific at the time Basket Maker II was a living culture in North America. With the Simple Loop in use in both areas, there seems to be no reason why the different varieties could not have followed independently through processes of parallel development.

It is possible, however, that the appearances of the Simple Loop in the two areas may be historically related by way of the Asiatic continent. The influences which Asia has had upon the surrounding regions in both ethnic strains and culture are well known and it seems more than possible that knotless netting, in the form of the Simple Loop, may have been carried by migrants or diffused at an early time from group to group, on the one hand to America and on the other, to Indonesia, thence to Melanesia. Indeed these influences may have spread also in other directions, for we find the Simple Loop also employed by the Lapps<sup>6</sup> and by numerous tribes in Africa.<sup>7</sup> A more detailed study of these appearances must be made before a theory of a unitary origin for all the occurrences of the Simple Loop can be reasonably upheld, but the peripheral appearances of this basic technique support the probability that it may have originated in Asia to diffuse to America by way of Bering Strait, and to Melanesia via Indonesia.

#### CONCLUSIONS

Our survey of knotless netting in the New World and in the Pacific would seem to indicate again that caution must be exercised in arriving at

<sup>6</sup> Bag in the Buffalo Museum of Natural History.

<sup>7</sup> Thomas, 1924, Mongbettu shield slings; Van Rippen, pp. 90-91, Uintjes food bag of Bushmen; Hall, p. 99, fig. 36, Bapende bodice; Van Gennep, Wangoni of southeastern Africa; Lehmann, 1907, pp. 19 *et seq.*, Middle Nile, Bakuba, Bali, Haussa, Ekoi, etc.

conclusions of trans-Pacific influence in American cultures. At first glance, the numerous complexities in the knotless netting of the two areas appeared to indicate that some trans-oceanic connection was not only plausible but necessary to explain the identical appearances. Although the facts from the historic cultures of the two regions would seem to be sufficient to dispel such a theory, the most convincing evidence has been furnished by archaeology. We cannot help believing, therefore, but that were it possible to obtain some knowledge of the prehistory of other traits involved in the trans-Pacific controversy, many of them might also be found to be independently developed in the two areas or historically related during very early times via the Asiatic continent. At the same time, it must be fairly admitted that other traits may have been brought across the Pacific. Each trait should be judged on its own merits and not in accordance with some pre-conceived theory that all similarities in America and Oceania are the results either of convergence on the one hand or of trans-Pacific diffusions on the other.

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UNIVERSITY OF PENNSYLVANIA  
PHILADELPHIA, PA

## BOOK REVIEWS

### NORTH AND SOUTH AMERICA

*Warpath. The True Story of the Fighting Sioux. Told in a Biography of Chief White Bull.* STANLEY VESTAL. (xv, 291 pp., 9 pls., 3 maps. \$3.00. Boston and New York: Houghton, Mifflin Co., 1934.)

Naturally stressing martial life, this biography adds interesting points to our knowledge of the subject, such as details on Western Dakota honor heraldry (pp. 24, 186), military technique including decoy tactics (pp. 56 ff., 118), and the history of a Bullet-proof society (pp. 131-144). The close contacts of the several Western Dakota groups is clearly brought out. The hero himself was the son of a Minniconjou chief and a cross-nephew of Sitting-bull of the Hunkpapa, and in the course of his life he belonged to societies of both tribes and of the Sans Arc (pp. vii, 26). Apart from warfare, there are several references to visions (e.g., pp. 12 seq., 93, 109, 250), to the heyoka (pp. 13, 261 f.), a shamanistic contest (p. 94), and the Sun Dance (pp. 95, 137), performers of which are said to have used headscratches (p. 95). A prayer to game animals in times of famine is noteworthy (p. 111), and the account of a buffalo scout's report (p. 234 f.) deserves mention. White Bull's fifteen marriages exemplify patrilocality (pp. 88, 215), the fragility of the bond (pp. 103, 124), the tribulations due to jealousy in a polygynous household (pp. 125 ff., 213, 216), and the throwing away of wives at the Grass Dance (p. 215). An incipient trend to hereditary aristocracy is seen to mingle with the principle that merit rests in personal achievement (p. 120). Swimming is described as "dog-fashion or overhand" (p. 7), and the Crow are credited with using the bull-roarer to frighten horses (p. 107).

ROBERT H. LOWIE

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*The Hidatsa Earthlodge.* GILBERT L. WILSON (BELLA WEITZNER, editor). (Anthropological Papers, American Museum of Natural History, Vol. 33, Part 5. 80 pp., 45 figs. \$1.00. New York, 1934.)

All students of Plains ethnology are familiar with the late Gilbert L. Wilson's accounts of particular phases of Hidatsa culture and will welcome this addition to the series. It is a matter for deep regret that Dr Wilson's death will prevent the bringing together of the wealth of material which he had gathered into a single complete study of this tribe. The present work maintains the high standard of the earlier reports. It is unquestionably the most complete account of the plan, construction and internal arrangement of earth lodges which has so far been published. It also contains a unique account of the founding of a new village, the marking out of the site, and the selection of places for lodges and the construction of the village stockade. The work will be of interest not only to ethnologists but to all archaeologists working in regions where earth lodges and village fortifications are found.

RALPH LINTON

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*Ethnology of the Nisenan.* RALPH L. BEALS. (University of California Publications in American Archaeology and Ethnology, Vol. 31, No. 6, pp. 335-414, 1933.)

This report on the Nisenan or Southern Maidu is concerned mainly with the hill and mountain culture which differs markedly from that of the Valley Nisenan. The culture is in an extreme state of decay, for these people suffered heavily during the American occupation of California and most of the old ways were soon abandoned. Dr Beals' work is comprehensive but necessarily sketchy on those points where adequate information cannot be obtained. Considering the present state of Nisenan culture, his account is probably as extensive as will ever be possible. Two characteristics of the report which deserve special mention are the use of a number of informants and, more important, the attention paid to regional diversity within the area. Too often information obtained in one community is assumed to apply to all communities within a tribal area so that a false picture of trait distribution is obtained. The study of variations from community to community in a tribal culture will no doubt assume much more importance in the future than it has so far been accorded, for such study illustrates in a most definite way the tendency of culture to modification.

FORREST E. CLEMENTS

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*Pomo Myths.* S. A. BARRETT (Bulletin, Public Museum of the City of Milwaukee, Vol. 15, 608 pp. \$5.00. Milwaukee: Public Museum, 1933.)

*Wintu Myths.* CORA DU BOIS AND DOROTHY DEMETRACPOULOU. (University of California Publications in American Archaeology and Ethnology, Vol. 28, No. 5, 124 pp. \$1.25. Berkeley: University of California Press, 1931.)

These two folktale collections are major contributions to the mythology of northern California. The first comprises one hundred and eight tales representing Central, Eastern, and Northern Pomo dialectic groups. The author has made his collection more than a mere record by adding apparatus to facilitate its use: a classification of the myths, introductory definitions of local religious concepts and supernatural beings, abstracts, catchword guide, glossary, and an all-inclusive index. Dr Barrett's tales and relevant ethnographic notes were collected twenty years ago. After correlating his findings with more recent data on Pomo religion, he concludes that the master-creator, Madumda, and Coyote are the same being, although Madumda figures but slightly in the tales (pp. 15-18). Tales of Creation, World Fire, Deluge, Ascent to Sky, Sun and Journeys to Sun's Abode, Theft of Fire, Supernatural Beings, Tricksters, Bear and Deer, and Origin of Death are compared on their major points with those from other tribes of central California (pp. 465-91) with the result that Kroeber's regional division of Californian mythology into a north central and south central group is wholly corroborated (pp. 11-13, 491-93).

Although the tales are rendered in chaste English, thereby precluding a consider-

ation of their literary style, their incidental structure is intact and they offer, with their adjuvant apparatus, an ideal group of tales for study. Immediate differences between dialectic groups, the relation of these differences to neighboring mythologies, wide relations of Pomo mythology in general, and the influence of preferred themes upon the local tale-type can be demonstrated by this admirable material.

The seventy-five Wintu myths were collected by an ethnographer and a linguist. While comparative notes of local range accompany the tales the authors omit all comment on their cultural relationships. The tales taken in text have been given in a style which happily preserves the flavor of the original. An able consideration of Wintu literary style and stability based on this collection has been published elsewhere by the same authors.<sup>1</sup>

Since this work was not used by Dr Barrett a brief comparison of salient features in these Pomo and Wintu myths may be to the point here. Creation: Pomo, Coyote and Wolf create men, animals, institute customs; Wintu, absent.<sup>2</sup> Flood: Pomo, activities of animals who save themselves on mountain top; Wintu, detailed activities of certain people just before the flood. Transformation of first people into fauna: Pomo, strong; Wintu, weak. Thefts: Pomo, of daylight, fire; Wintu, of daylight, fire, obsidian, dentalia, food seeds. Contests: Pomo, against evil father-in-law, Sun, cannibalistic bird, Thunder, by heat, shooting opponent; Wintu, against Sun, evil father-in-law, by variety of minor tests, gambling, talking, hunting. Coyote: Pomo, creator-culture-hero-transformer, and dupe; Wintu, trickster and dupe. Important characters: Pomo, Coyote, Wolf, Falcon, Sun, Cannibalistic Bird, Water Monster, Obsidian-man; Wintu, Coyote, Strong Dwarf, Loon Woman, Ogress, Olelbis. Ritual numbers: Pomo, 4, 2; Wintu, 5, 10.

General likenesses between the mythologies are those characteristic of the north central California folktale area; the contrasts are preferences for certain characters and emphasis on different topics.

A. H. GAYTON

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*Two Paiute Autobiographies.* JULIAN H. STEWARD. (University of California Publications in American Archaeology and Ethnology, Vol. 33, No. 5, pp. 423-38, 1934.)

*Ethnography of the Owens Valley Paiute.* JULIAN H. STEWARD. (Same series, Vol. 33, No. 3, pp. 233-350, 1933.)

*Ethnography of the Surprise Valley Paiute.* ISABEL T. KELLY. (Same series, Vol. 31, No. 3, pp. 67-210, 1932.)

The two Paiute autobiographies presented by Dr Steward are of interest in that they illustrate this culture in actual operation and show the way in which two en-

<sup>1</sup> A Study of Wintu Mythology (Journal of American Folk-lore, Vol. 45, 1932), pp. 373-500.

<sup>2</sup> Though given by Jeremiah Curtin, Creation Myths of Primitive America (Boston, 1898), pp. 243-63.

tirely different personalities reacted to their cultural environment. Their chief value lies in the fact that they contain much subjective data regarding individual variations in psychological attitudes and cultural values. Such data are not often found in the usual ethnographic account.

The second and longer publication of Dr Steward is an orthodox description of the culture of the old Eastern Mono, a tribal designation which is shown to be inaccurate, for their relationships are with the Paiute rather than the Mono. The report is comprehensive, well-written, and covers all the main features of Owens Valley Paiute culture. There are a number of illustrations and two excellent maps.

Miss Kelly's work is likewise an orthodox ethnographic account and deals with the Northern Paiute or Paviotso. The data on material culture are very full, while the information on social and political organization is as comprehensive as possible though not extensive. This last is probably due to the lack of complexity in social organization. The account of magic and religion presents the important phases of the subject but the author feels that much is yet to be learned. Information on religion is difficult to obtain because of native reluctance to discuss such esoteric matters. The work is illustrated with a number of drawings and plates.

Like all the publications in this series, these three dealing with the Paiute are thoroughly condensed so that although the longest numbers only a little more than one hundred forty pages, they contain a remarkable amount of information. Their publication fills a considerable gap in existing knowledge of Basin ethnography.

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*Archaeology of Santa Marta Colombia. The Tairona Culture. Part 1, Report on Field Work.* J. ALDEN MASON. (Anthropological Series, Field Museum of Natural History, Vol. 20, No. 1. 130 pp., 64 pls., 2 maps. Chicago: Field Museum, 1931.)

This very interesting publication is the first of an exhaustive report which is planned to appear in three parts. The second and third, which are to appear later, will be respectively "the description of the material" and "the scientific results and deductions." We are thus, in Part 1, furnished with a detailed report of the field work of the expedition which remained in this special locality in the northern Colombian coastal region for a full year. The particular objective of the expedition was a careful scientific study of the cultural remains of the long-extinct Tairona Indians, representative of one of the more advanced aboriginal cultures of America.

In a pleasing narrative style the author first takes us from one to another of the various sites visited along the coast and in the adjacent mountains. We are introduced to the sundry vicissitudes of work in this tropical land while we see brought to light architectural, engineering and other features, such as "paved roads, bridges, walls, covered drains, staircases, circular sites, and stone-lined graves," together with incised petroglyphs, beautifully formed and decorated pottery, and a multitude of other tangible evidences of a once dense population of high culture.



Then follows a very careful and detailed "description of the localities investigated." Each locality is taken up and fully described; its location, topography and environmental setting, and history being given. Then follow in full detail the archaeological features encountered by the expedition: architectural, engineering and mortuary features, artifacts of many kinds, including objects of gold and semi-precious stones, as well as implements of stone, bone, shell and the like, and pottery of mortuary and utilitarian types. Full attention is given to osteological remains and the circumstances of their placement. In short no detail is omitted from the description of each locality in which any work was done.

The paper is excellently illustrated with photogravure plates and many plates of villages and drawings of special features and objects.

It is, in fact, throughout a most careful and thorough report on the field work carried on by the expedition and forms an excellent preliminary for parts two and three which we shall await with much interest.

S. A. BARRETT

MILWAUKEE PUBLIC MUSEUM

## AFRICA

*Myths and Legends of the Bantu.* ALICE WERNER. (335 pp. 15s. London: George W. Harrap and Co., 1933.)

Dr Alice Werner, whose contribution of the African volume to "The Mythology of All Races" has already shown her knowledge of the folklore of the whole continent, has in this new work confined herself to the Bantu-speaking tribes of central and south Africa. These peoples she presents to the reader not only with understanding but with the affection that comes from long and close association.

The book is designed for the general reader. A clarification of the inter-relation of the various African tribes and an account of their habitat and customs prepares the way for the main part of the book, the myths and legends. These are arranged into logical groups, beginning with tales of the gods and the other world and proceeding through hero adventures to ogre and animal stories. These tales have all been retold by the author and joined by a running commentary.

For the general reader this method undoubtedly adds to the interest of the book, as do the frequent pictures of scenes suggested by the stories. Though the folklorist will wish that the stories had been more definitely separated from one another so as to indicate the exact form of the tale type, he must realize that in a popular compilation such as this, he can hardly expect such formal separation any more than he may look for completeness in the list of types or for any attempt at comparative notes. He can at least be particularly grateful for the fullness of the bibliography and index and for the attractive printing and binding of the volume.

STITH THOMPSON

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*Fish-hooks in Africa and their Distribution.* S. LAGERCRANTZ. (Riksmuseets Etnografiska Avdelning. Smärre Meddelanden, No. 12. 39 pp., 14 figs. Stockholm, 1934.)

This short paper, the twelfth of the whole series and the tenth written in English on technological subjects, is published by the Swedish museum. It shows the distribution of fishhooks in the African continent, and is an important contribution to ethnographical literature. The author supplemented this paper by another on fish-hooks during the recent anthropological congress in London.

From a study of the distribution of fishhooks Lagercrantz comes to the conclusion that, as there is no archaeological evidence of fishhooks in Africa (with the exception of Egypt, the Mediterranean coast, and the Canary Islands), they are not indigenous to the continent. He sums up by saying (p. 31):

1. With exception for East-Africa and Congo where fish-hooks are indigenous, they are brought to Africa by European and African traders.
2. In Western Africa can be made out an area where the Hausa have spread European (?) hooks to certain tribes, and
3. crocodile hooks were formed upon the model of fish-hooks.

On the whole the paper is an excellent summary of all that is known of fish-hooks in Africa. It also contains fourteen figures, mostly of fishhooks in different European museums and a map showing the distribution of gorges and round hooks in Africa. It is to be hoped that similar papers will be published about other continents as well. The present reviewer has collected materials dealing with North American fishhooks in European museums which he expects to publish shortly.

BIREN BONNERJEA

BUDAPEST, HUNGARY

*Die Völkerstämme im Norden von Liberia.* PAUL GERMANN. (141 pp., 24 figs., 37 pls., chart. 7 Mk. Leipzig: R. Voigtländer, 1933.)

This is a report of results gained on an expedition of about three months' duration: a sketch of the ethnography of certain tribes in northern Liberia. The brevity of time and the obligation of gathering ethnological specimens were no doubt partly responsible for the sketchiness of the study, which is to be regretted. The ethnology of Liberia is to a great extent still unknown. The older literature is concerned mainly with the coast population, especially the Vai and Mende on or near the western shoreline. Lately Westermann has published a detailed study of the Kpelle (Kpessi), a large group stretching northward from the center of the country.<sup>1</sup> Dr Germann's material is on the neighboring northern tribes of the Gbade, Toma or Buzi, and to a certain extent the Gissi (Kissi). Some rather scanty information was available previously on the congeners of these tribes in French territory where Mandingo influence is felt more heavily. This influence is not negligible on the Liberian side either; Mandingo ("Comande") are found in

<sup>1</sup> D. Westermann, *Die Kpelle: Ein Negerstamm in Liberia* (Göttingen-Leipzig, 1921).

many villages, forming privileged warrior groups, or as priests or leather workers; and they traverse the countryside as itinerant traders and bards.

Excepting the Gissi, a more recent intrusion into Liberia, the tribes under discussion are linguistically and culturally closely allied to Westermann's Kpelle. As Westermann's study concentrates especially on social organization and religion, Dr Germann's material complements it very well; its most complete portions are those dealing with material culture and techniques. Still, one would like to have had more specific and clear-cut information on some aspects of social organization. The sib system is treated within a meager two pages, leaving most of even the fundamental questions somewhat suspended. As Westermann's work is followed rather closely, it is at times doubtful whether the author's statement is merely a quotation from the former on the Kpelle or is a matching of Kpelle data by those from his own tribes. This becomes especially disappointing when the sib system is described. Westermann suggests that under external influence an older matrilineal system in the region is and has long been leaning very gradually toward a patrilineal system.<sup>2</sup> Dr Germann suggests the same thing with the same specific data, and more strongly put (p. 72). It is not clear, however, whether he had found independent confirmation or merely accepted Westermann's data as applying to his tribes also.

A sizable collection of stories, chiefly from the Gbande, closes the volume. In these, two of the West African tricksters figure side by side, the water chevrotain and the spider. Many examples are of the riddle-story type in which the listener decides at the end which of the actors deserves the chief credit or reward. Some of the stories that seem to call for an etiological or moralizing ending, or for a closing proverb, end somewhat up in the air. This may be because most of the story material was collected from young boys temporarily at the mission school.

The descriptions of techniques and material objects are excellent and well illustrated, and the work is well rounded, except for the points already raised. The more reason for the reader to regret that more time could not be spent by the author in enlarging our knowledge of one of the regions in Africa on which any information is most welcome.

GEORGE HERZOG

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*Essai sur la grammaire Banda.* CHARLES TISSERANT. (Travaux et Mémoires de l'Institut d'Ethnologie, Vol. 13. 185 pp. 56 fr. 25. Paris: Institut d'Ethnologie, 1930.)

*Dictionnaire Banda-Français.* CHARLES TISSERANT. (Same series, Vol. 14. 611 pp. 137 fr. 50. 1931.)

*The Phonetic and Tonal Structure of Efik.* IDA C. WARD. (xiv, 186 pp., 22 figs., kymograph tracings, texts. 8s6d. Cambridge: W. Heffer and Sons, 1933.)

<sup>2</sup> *Op. cit.* pp. 55-56; J. L. Sibley and D. Westermann, *Liberia—Old and New* (New York, 1928), pp. 115-16.

In many languages of the African continent, "tone" or musical pitch is an element so fundamental that its violation would keep the speaker from being understood. The average linguist, however, is not trained to perceive musical tone, nor is he necessarily equipped with an accurate musical ear. Thus he finds himself in a situation not unknown to the anthropologist—lacking in a specific technic and at the mercy of his deficiency or of the "expert" he calls in.

The first case is illustrated in Father Tisserant's study of the Banda dialects, found north of the Ubangi River. His essay shows serious work, although the categories of the conventional European grammar loom a little too large to make one feel completely comfortable. The author pleads a deficient ear and in the grammar restricts tonal indication to places where a difference of grammatical meaning can be explained only by a difference of tone. In the dictionary all tones are marked as heard. A perusal of it shows that so very few words occur either on high or low, as against the overwhelming mass of middle tones, that one fears the markings are not always correct. The procedure followed in the grammar is dangerous as it entails selecting what is functionally important in material which may not have been identified correctly. Thus, however unintentionally, a specious certainty is conveyed.

The function of tone in Eñk—a language of the Cross-River region, West Africa—is the chief subject of Miss Ward's study, offered modestly, with no linguistic pretensions. Her background, then, is presumably phonetic and musical, and not originally in the African linguistic field. Certain points are of interest as showing the disadvantages of the expert unacquainted with the setting. For example, consonantal length is represented by writing the sound symbol twice if a nasal prefix is followed by a word beginning with the same nasal. Vocalic length on the other hand is not written because it is "not significant." "Some vowels, however, strike the ear as longer than others, and should be so pronounced." "A vowel on a rising or a falling tone is generally longer than on a high or low level tone" (p. 29). What seems at first merely inconsistently written turns out to be wrongly conceived. Just as, before, an objectively long element was conceptually not a unit but a merging of two, so the "long vowels" may represent some such process; a rising tone must be looked upon as low tone plus high tone. Whether "length" is functional or not cannot be legislated. This leads to the essential limitation of the study: while the tones are marked with a minuteness almost too exact to be meaningful, we do not get an understanding of the system as such. A number of patterns are described and their grammatical meanings ascertained, a great advance over older studies of the language. But one has the feeling that if an out-and-out linguist could collaborate with the author, something more integrated would emerge.

From these comments, two conclusions follow: specifically, that musical tone should by all means be marked, and consistently, in languages where it functions significantly; and in general, that the outside expert must be either intimately acquainted with the setting or context of the material and with the technic of that setting, or must collaborate with one who has this intimacy.

GEORGE HERZOG

## OCEANIA

*The Manobos of Mindanao.* JOHN M. GARVAN. (Memoirs of the National Academy of Sciences, Vol. 23, First Memoir. \$1.00. Washington: Government Printing Office, 1931.)

This volume is the result of a residence of nearly four years among the Manobo, a little known pagan tribe of south central Mindanao in the Philippines. During his stay the author acquired a knowledge of the language and entered fully into the life of the people. As a result he is able to furnish a most intimate picture of the culture, as well as the natives' views concerning their own activities.

The tribe is broken up into many small divisions which the author calls clans, although they appear to be simply groupings of families around noted warriors. Since such leaders are in communication with and under the protection of the warrior deities they possess great prestige, yet they have no real authority, they receive no tribute, and exact no services from their fellows. They are called upon to settle disputes, but their decisions must be according to the customary law—which in turn is closely linked with religious beliefs. The desire of the younger men to achieve distinction as warriors, feuds between the "clans" and hostile relations with surrounding tribes have resulted in a constant state of warfare and a lack of security which is reflected in every phase of the life.

While they are poor in material possessions and appear backward in many respects, they have well defined ideas of property, of contract, debt and liability. Religious beliefs and practices, while not organized, are surprisingly uniform and still hold full sway over the lives of the tribesmen.

In general the Manobo correspond rather closely to the other tribes of southern and eastern Mindanao. This is particularly true in material culture and religious practices, even the names of many of the deities being held in common.

One portion of the volume is devoted to a discussion of the neighboring tribes and their relationship to the Manobo. Writings of several Spanish missionaries of forty or more years ago are cited but no mention is made of recent ethnological publications which deal with the tribes in question. Reference to these later studies would have made unnecessary the rather vague speculations on physical type and tribal relationships found in the first chapter.

Even though the comparative data is inadequate and the volume is singularly lacking in citing the literature of the field, the study is a distinct contribution to our knowledge of Philippine ethnology. On the purely descriptive side, Mr Garvan has furnished an excellent account of native life, amplified by personal experience and opinions furnished by the people themselves.

FAY-COOPER COLE

UNIVERSITY OF CHICAGO

*Archaeology of Oahu.* J. GILBERT McALLISTER. (Bulletin, Bernice P. Bishop Museum, 104. 201 pp., 67 figs., 12 pls. Honolulu: Bishop Museum, 1933.)

This report is one of a series covering the archaeology of the various islands in the Hawaiian group. Although it deals exclusively with Oahu, even omitting com-

parisons with the rest of the group, it will be of interest to all students of Hawaiian culture. In Hawaii, no clear line can be drawn between archaeology and ethnology. Practically all the implements found in archaeological work are of types still in use at the beginning of the historic period while many of the ruins were actively functioning temples at the time of Cook's visit. In the case of the smaller shrines numerous beliefs and practices are still current. Dr McAllister has covered the literature with unusual thoroughness and has gathered a good deal of new material from local informants, making the book a real addition to our knowledge of both past and present Hawaiian culture. He has also succeeded in making it unusually readable for a work of this sort. Routine descriptions of objects and sites are enlivened by legends and historic incidents and there are occasional flashes of humor which in no way detract from the accuracy and thoroughness of the book as a scientific report.

RALPH LINTON

UNIVERSITY OF WISCONSIN

*Archaeology of Kahoolawe.* J. GILBERT McALLISTER. (Bulletin, Bernice P. Bishop Museum, 115. 61 pp., 5 pls., 25 figs. Honolulu: Bishop Museum, 1933.)

*Stone Remains in the Society Islands.* KENNETH P. EMORY. (Same series, 116. 182 pp., 20 pls., 133 figs., 1933.)

*Tuamotuan Stone Structures.* KENNETH P. EMORY. (Same series, 118. 78 pp., 10 pls., 71 figs., 1934.)

McAllister's report on the smallest of the inhabited Hawaiian islands is based upon data collected by Stokes in 1913, upon a detailed study of materials collected at that time, and upon a week's field survey. Archaeological remains encountered on the island include heiaus, fishing shrines, house foundations, camp sites, burial places, and miscellaneous features associated with these. The sites produced such culture-indicative materials as are consistent with a simple, primitive, fishing village type of life. A particularly illuminating series of specimens illustrating the manufacture of bone fishhooks was discovered, supplying previously unknown details.

All the evidence indicates a small, semi-permanent population, strictly Hawaiian of the time of the discovery. However, the cultural manifestations are limited, lacking many features characteristic for the more densely populated islands, and suggest a fishing industry rather than a well-rounded culture. The author logically postulates that the early inhabitants were transient fishermen, hampered in any broader cultural expression by local arid conditions.

The report exhibits a clear simplicity in the presentation of facts, and a quality of critical judgement in conclusive treatment, that promise well for the future work of the author.

Emory's two reports, on stone structures in the Society and Tuamotuan groups respectively, are based largely upon personally collected data, although the author

takes full advantage of all previously reported facts pertinent to his subject. Three-fourths of the text in these reports are devoted to detailed descriptions of archaeological features, with directly associated historical and theoretical comments. Two hundred and fifty sites in the Society Islands and ninety-three in the Tuamotus were examined.

The outstanding structures in both groups are maraes. In the Leeward Islands of the Society group, these are generally paved courts with a sacred platform (ahu) at one end. In the coastal regions of the Windward Islands the maraes are relatively large and equipped with stepped ahu. Inland in Tahiti there are three types of maraes: (1) simple shrines without ahu; (2) platforms with a type of ahu surmounted by three upright back-rests; (3) enclosures with ahu surmounted by a single back-rest. The Tuamotuan maraes are basically like the Leeward Island structures of the Society group, although generally smaller and tending towards even greater simplicity. A greater number of back-rests characterize the ahu platforms. Exceptional are the maraes of some of the most eastern of the Tuamotus, which are larger and more similar to the archaic maraes of Necker Island.

There is a fundamental cultural difference between the type of stone work dominant in the Windward Islands of the Society group, characterized by small dressed stones and superior stone shaping and fitting, and that most typical for the Leeward Islands and the Tuamotus, where a megalithic type of masonry prevails.

Emory concludes that maraes "probably" came into Polynesia from, or by way of, Micronesia. The Tuamotuan marae is the prototype of the Society Islands forms. Similar structures in the Marquesas were probably introduced from the Society Islands. The marae, so named, is not found in Hawaii proper. It is a sacred place in southeastern Polynesia, whereas in southwestern Polynesia it is a place of general assembly. The sacred marae of Tahiti is probably the result of a merging of the marae, or place of assembly, and the sacred ahu, accomplished after the separation of the Society islanders from the Polynesian migrants to Hawaii and southwestern Polynesia. The Hawaiian heiau, when equipped with a stone enclosure wall, is not unlike the marae of southeastern Polynesia, structurally and functionally, and there is traditional evidence that two of the enclosed heiaus were built under the influence of Tahitian immigrants in about the thirteenth century. The image platforms of Easter Island are basically like the maraes of the easternmost Tuamotus.

Archery platforms, house sites, fortifications, wier fish traps, petroglyphs, and burial mounds are also described and discussed.

These reports not only reflect careful attention to facts encountered in the field, where much time and hard labor must have been required to cover the sites described, but also exhibit a thorough understanding of the problems and a comprehensive knowledge of culturally related provinces in Oceania. These factors lend important value to Emory's work. It is the reviewer's reaction, however, that the author is inclined to see the phenomena of other Polynesian divisions in terms of Society Islands phenomena, and so to over-accent the latter province as a center of one-way diffusion. Data are submitted in support of a number of interesting theories

of diffusion, but the extent to which these data may have been selected for that purpose remains a pertinent question in the reader's mind.

W. C. MCKERN

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### PREHISTORY AND PHYSICAL ANTHROPOLOGY

*Paleolithic Man and the Nile Valley in Nubia and Upper Egypt. A Study of the Region During Pliocene and Pleistocene Times.* K. S. SANDFORD AND W. J. ARKELL. (Prehistoric Survey of Egypt and Western Asia, Vol. 2. [James Henry Breasted, ed.] Quarto, 92 pp., 21 figs., 43 pls., map. \$6.00. Chicago: University of Chicago Press, 1933.)

The first volume of this series (see review, *AMERICAN ANTHROPOLOGIST*, Vol. 32: 677-78) dealt with the Faiyum basin and the adjacent west side of the Nile valley a short distance above Cairo, or more precisely, 125 to 175 miles up the river from the Mediterranean. The present volume is a supplementary report on another segment of the Nile trough, extending from Luxor southward beyond the Egyptian border as far as the third or Semnah cataract in Sudan, or more strictly, Nubia; i.e., between points approximately 425 and 775 miles upstream. As before, the aim is to correlate early human activities with definite geological events, an undertaking to which the specially equipped authors have devoted no less than five field seasons, the present report representing surveys conducted in 1926-7 and 1929-30, supplemented by final checkups in 1930-31.

Fully two thirds of the verbal report is devoted to the geological and topographical history of the Nile valley, the remaining portion being divided between description of selected artifacts found *in situ* and description of typical local petroglyphs. The geologic introduction begins with considerations of the ancient igneous (granitic) and later metamorphic (sandstone, shale, limestone) formations across which the Nile has cut its deep channel. It next demonstrates the existence during Pliocene times of a Mediterranean gulf extending up the Nile trough for a distance of more than 500 miles, as indicated by the presence along the valley sides of breccias and conglomerates ranging up to 180 meters above the present sea level. Beyond the head of this gulf, near Kom Ombo, no Pliocene formations occur, the well-marked fresh-water gravel terraces there present being regarded as of much later origin. The older or upper series of these terraces, considered as of Plio-Pleistocene date, range at levels approximately 300, 200, and 150 feet above the present Nile valley floor; while the younger or lower definitely Pleistocene terraces occur at elevations respectively 100, 50, 30, and 10 feet above the same valley floor, with which all are uniformly parallel for several hundred miles. These extensive gravel terraces are taken to indicate a strong Nile current, or in other words a relatively heavy rainfall, both local and distant. But, following the last or ten foot gravel terrace, a diminution in precipitation—foreshadowing the approach of modern desert conditions—is indicated from below the second cataract northward by the mantling accumulation exclusively of fine silt. This silt phase—still in progress—was at first characterized



by prolonged widespread deposition and later on, as at present, by slow erosion of the relatively old, high lying silts on the one hand and the continuing deposition of new silts on the valley floor on the other. Advancing desert conditions have finally resulted in sand dunes which partially mantle all formations on the west bank of the Nile. All of this highly interesting local bit of earth history is necessarily technical and in some of its details beyond the full comprehension of the reviewer. In the way of foolish questions, however, the mere archaeologist is left puzzled as to the essential mechanism of terrace building. He wonders how Pleistocene and Pliocene terraces are distinguished—by the presence or absence of implements, perhaps? He would like to know why the upper surface limit of the older silt deposit does not parallel the valley floor as do the gravel terraces but instead rises gradually southward, from 18 feet at Luxor to 100 feet at the second cataract. Lastly, he is particularly anxious to learn how deep in the valley floor silts Neolithic artifacts occur; in short, whether or not Breasted's 1919 estimate of 20,000 years still holds.

Turning to the artifacts themselves, the all-important point about them is that their geologic occurrence here in distant Upper Egypt and beyond conforms in all major respects to the earlier Faiyum Divide findings. Protracted search in the Pliocene and Pliocene-Pleistocene formations has yielded no positive *in situ* results. In consequence of this the authors boldly discard Schweinfurth's collection of supposed Pliocene Eoliths as obviously not Pliocene because, for one thing, they were partly true Paleoliths. The oldest of the seven or more lateral gravel platforms to furnish unquestionable artifacts of Prechellean and Chellean coup-de-poing types is the 100 foot terrace, considered as of Pleistocene date. The 50 foot terrace supplies typical Acheulian implements, as well as rolled Chellean forms derived from the terrace above. The 30 foot terrace yields an Early Mousterian technique, while the 10 foot terrace is characterized by a fully developed Mousterian inventory. Finally, the superposed high-level silts inclose and are overlaid by a rather characterless flint industry which changes gradually in both technique and implement forms from the typical Mousterian of the 10 foot terrace to a microlithic industry suggestive of the Late Capsian of North Africa and the Azilian-Tardenoisian of western Europe. This early silt phase culture stage is locally distinguished as the Sebilian industrial period and corresponds chronologically, and to some extent technologically, to the Upper Paleolithic of Europe. Its remains are significantly absent from the high desert surface, but in the valley (besides occurring *in situ*) are found superficially as workshops and even as shellheaps, which latter demonstrate their relative antiquity by the presence of bones of at least two extinct mammals.

The authors have made a studied selection from their artifact discoveries, which has been illustrated in natural size as well as described and discussed in a distinctly penetrating and illuminating manner. One could wish, however, that the generous allowance of plates had been filled with more illustrations, even if of reduced size, so that the reading archaeologist might have had a chance to size up the successive industries for himself. Also it is regrettable that in the case of the two plates devoted to the Middle and Upper Sebilian remains more space is devoted to mere cores and

flakes than to actual implements, provided of course that the latter were present in larger numbers. Lastly, the petroglyphs, separable into several successive groups of which the oldest bears some resemblance to Bushman work, are also described and figured sufficiently, it is to be hoped, to encourage someone to give the subject special attention.

All in all the report strikes the reviewer as a thoroughgoing performance of the highest importance. It has been known for a long time that the high desert surface in certain places on both sides of the Nile is littered with flint implements of European Lower as well as Middle Paleolithic types and which exhibit widely varying degrees of patination. It has been known also, for at least fifteen years, that the degree of patination correlates directly with the supposed typological evolution of implement forms and that therefore Egypt's early flint industries most likely underwent essentially the same changes as those established for western Europe. Now, finally, in the present report we have our theoretic chronology stratigraphically demonstrated and the local antiquity of man at least tentatively limited to the Pleistocene geological period.

N. C. NELSON

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*An Introduction to the Anthropology of the Near East.* C. U. ARIËNS KAPPERS. With a chapter on Near Eastern Bloodgroups by Leland W. Parr. (vi, 200 pp., 94 figs., 2 tables, 1 chart. \$9.00. Amsterdam: N. V. Noord-Hollandische Uitgeversmaatschappij, 1934.)

To term this monograph an "Introduction" is but to reflect the innate modesty of the author. It is far more than an introduction: it is a comprehensive survey of all that is known of the physical (cranio-cephalic) types of the Near East, both historic and modern. Much of the latter data represents the original research of the author and his colleagues, Dr and Mrs H. Krischner, who have measured 2500 and 2700 individuals, respectively.

The study is based almost solely upon three head measurements—length, breadth and height—and their derived indices. The author regards the cephalic index as of paramount importance in tracing racial affinities, defending his thesis by pointing out that on fragmentary material length and breadth measurements are often the only ones available. A further defence is quoted from Morant who in an evaluation of 31 craniometric measurements concluded that six, including length, breadth, height and their indices, were of supreme diagnostic value. Professor Kappers is quick to point out, however, that the comparative study of craniometric distribution must be limited to a definitely circumscribed area. His handling of the data is based upon the use of the frequency curve; this gives not only the average, but frequencies and the range of the distribution. This method elucidates diverse racial elements by means of a bimodal or even polymodal curve.

The early part of the volume is given over to a discussion of the Armenians and others of Armenoid affinities. The Armenians are found to present two peaks in the

cephalic index curve: one at 83-4 and the other at 86-7. This characteristic is dominant and is established as the criterion of relationship. Thus the Khaldeans with peaks at 83 and 86, the Assyrians (Aissori) with peaks at 84 and 87, the Christian "Arabs" with peaks at 84 and 87, and the Lebanese with peaks at 84 and 87 (although there is an extraneous element represented by a peak at 80-1) are all definitely linked up with Armenoids. Similarly the "hyperbrachycephalics among the Turks are of Armenoid . . . descent."

The skeletal material from Alishar (Boghaz Keui, the ancient Hittite capitol Kheta) revealed dolichocephalic and brachycephalic skulls neither of which type was similar to the "Hittites" of the monuments: the hypsicephalic planoccipital type represented by the Armenians. Professor Kappers explains this apparent dilemma by stating that "the so-called Hittite pictures represent the older and numerically larger Subarean component of the Hittite empire of Ramses II's time." The dolichocephalic skulls of Alishar IV are stated to "have an Indo-Aryan type." Some of the brachycephalic skulls of Alishar II-III are considered Armenoid, notwithstanding that the brachycephaly is due to increased breadth rather than decreased length.

Professor Kappers concludes that

the Armenians, Khaldeans, Aissori and some other groups are closely related to Central Asiatic peoples of the Altai-ugrian or Furki stock. As several of these peoples are related to Central Mongolians and may have Mongolian facial features, this conclusion agrees with Dudley Buxton's statement that the Central Mongolians are connected with the Armenoid race. I would propose to group them together as the "*Central Asiatic Race*"

The author then considers the thinning out of hyperbrachycephaly as Palestine and Syria are approached. This is due, probably, to intermixture. The Jews may be cited as an example when it is observed that they "are a mixture of the brachycephalic Subarean and mesocephalic Asiatic mediterranean types." Professor Kappers is quick to recognize, however, that the Jewish head-form will differ with the surrounding physical type.

The antiquity—as well as the ultimate origin—of dolicocephaly in the Near East is clearly hinted when the author identifies as similar Alishar IV, Hissarlik III, Ur, and Punjabi types. These long-heads are the "Caspian people" who "presented an ancient Indo-Aryan stock." In general Indo-European, Indo-Aryan, Caspian, and Mediterranean are used synonymously, although Professor Kappers insists that it is necessary to discriminate between the *ancient* and the *modern* Mediterraneans. The former, with peaks at 72-73 and 75-76, are either "Indo-Aryans or a Veddoid people." The latter are "prevailingly mesocephalic."

Greece and Crete are considered to both represent Egyptian influence; Greece in dynastic times, Crete in predynastic and early dynastic times. In addition a mesocephalic Scythian element is traceable in both populations. A final sub-brachycephalic Caucasian (Slav) element is observed in certain districts of the Peloponnesos.

The racial structure of the Near East is thus reared almost solely upon the cephalic index, i.e., proportions rather than absolute measurements are the criteria of relationship. Professor Kappers does, however, permit a distinction between the short-skull brachycephaly of the Armenians and the broad-skull brachycephaly of other round-headed groups. Insofar as he does this, absolute dimensions receive consideration. Smallness of skull is explained by environmental factors. Thus the small head of the low caste Chuhra is contrasted with the "better situated Rajputs," the very small-headed hyperbrachycephals of South Arabia measured by Thomas are considered as a "degenerative form due to poorer social circumstances." Again, when discussing index-changes it is observed that the "increase of brachycephaly [might] be due to a greater vitality of the higher index race compared to the perhaps somewhat asthenic lower index people, prevailing length-growth being an asthenic feature." With brachycephaly once achieved, its spread is easily explained by Frets' statement of brachycephaly as dominant in a cross.

Professor Kappers has offered us a brilliant demonstration of method; his thesis is ever pursued to its logical conclusion. Though his wealth of detail is at times embarrassing he has done more than introduce us to the anthropology of the Near East: he has lead us along the avenues of race-movement and has explored with us the little-known paths of race-trend; far more he has opened the way for further study, and has challenged the historo-anthropologist to a renewed effort to disentangle racial strains in the Near East.

In his contribution to the serology of the Near East Dr Parr offers conclusions based on blood-typing data from 7823 individuals of Western Asia and North Africa. After an initial warning that "similar possession of blood type racial formulae does not prove relationship but only like possession of isohemagglutination factors" he states that "blood typing work properly carried out and interpreted may materially assist the older methods of Anthropology in the study of race groups."

The blood-grouping carried on by Dr Parr substantiates Professor Kapper's conclusions as to the kinship of the Hittites, Khaldeans, Armenians and Lebanese. The Egyptians are placed in the Hunan group of Snyder and "some of the Persians" are identified with the Adnan Arabs. Of the Jews it is concluded that "there is serological evidence that the Jews are a religion rather than a race."

The Samaritans, who are classed by Professor Kappers as "Hebrew Semites," are "immunologically unlike any of the existing surrounding groups whom they might reasonably be presumed to resemble." This is probably due to very intensive in-breeding, a fact, which caused Dr Parr to conclude that "the value of blood-typing tests in anthropological studies is very little when the group studied is highly isolated."

The serologist offers his services to the anthropologist. Dr Parr exemplifies the caution necessary in the interpretation of the correlation of apparently related data. His conclusions gain the more because of his reservations.

W. M. KROGMAN

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## MISCELLANEOUS

*Property Restrictions as Survivals of Primitive Communism.* D. K. ZELYENIN. (Transactions of the Institute of Anthropology and Ethnography, Vol. 1, No. 1. 75 pp. Roubles 2.25. Leningrad: Academy of Sciences, 1934 [In Russian].)

Professor Zelyenin adheres closely to the present theoretical position of Soviet ethnography. This position is based on Engel's views of the origin of private property, the family and the state, which in turn were strongly influenced by Lewis H. Morgan's evolutionary theories of social development. The current Soviet belief is in an original universal condition of "primitive communism," a pre-class society which developed universally through various definitely characterized stages to feudal and then capitalistic social forms. Hence a large part of ethnographic research is devoted to finding survivals of earlier stages.

In the present work the author quotes numerous instances, mostly from groups in Russian territory, of restrictions on the right of individuals to do as they wish with their property. "Property restrictions" is defined as a "prohibition on giving away one's own possessions for temporary or permanent use to strangers," meaning by strangers anyone outside the "in-group." Professor Zelyenin then indicates that these restrictions are survivals from an earlier form of property ownership when the possessions in question were the common property of the group.

Numerous varieties of property restrictions both material and immaterial are compared. Restrictions on the use of magical power by the individual are particularly stressed as well as those on ceremonial and name privileges. Limitations on the use of the tribal fire are cited from various localities and carried down to the sacred fires of classical times. One section is devoted to traces of matriarchy as revealed in prohibitions on the use of property. The Marxian approach is emphasized in the section on restrictions covering the means of production and especially in the final paragraphs which consider property restrictions as weapons in the class struggle.

Most of the material is taken from Russian ethnographic accounts though there are a few examples cited from such sources as Frazer and Crawley.

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*The Earliest Relations Between the Celts and Germans.* C. S. ELSTON. (London: Methuen and Co., 1934.)

To anyone who is concerned primarily with modern ethnology, linguistic paleontology must seem an adventure among possibilities of past events and past relationships rather than an encounter with cultural data. One of the possibilities which adventurers preceding Dr Elston found was that Celtic overlordship characterized the earliest relations of Celtic speaking and Germanic speaking peoples. Inferences of such an overlordship were based almost exclusively on the peculiar sort of circumstantial evidence which linguistic paleontology supplies. Dr Elston does not eschew such evidence, but uses it cautiously. Even such a generally accepted point

as that \*rīks is borrowed from Celtic is shown to be not entirely certain (p. 166). And less certain borrowings of Celtic words and concepts by Germanic are shown to be definitely wrong. On the whole, the book indulges in consistently negative criticisms in this respect. Germanic culture was not dominated by Celtic culture; the most that can be said is that certain cultural contacts existed between the right and left banks of the Rhine, presumably commercial, possibly turbulent.

Here we have what so often happens when circumstantial evidence is used exclusively. There are imaginable alternatives. One linguist finds reason to expect a Celtic overlordship; another says that nothing more than trade relations can be supposed. And precisely those scholars who are apt to be most interested in the ethnological relationship are often incompetent to make independent decisions in the type of linguistic controversies which follow. It is no wonder that ethnologists generally regard the evidence presented in linguistic paleontology without great enthusiasm.

Dr Elston, however, does something which is rare enough to give the impression of a new methodology. He reviews all the relevant historical and archaeological data which he is able to find. These data constitute direct evidence. They suffer in being few, sporadic, and inconclusive by themselves. But combined with the circumstantial evidence of words which Germanic borrowed—or did not borrow—from Celtic, the thesis of the book is so fortified that alternative possibilities are ruled out.

Negative theses such as Dr Elston's, however clearly demonstrated and valuable, have not the compelling interest of positive descriptions. It is to be hoped that Dr Elston will in future apply his method in a study of, say, the earliest relations between Hittite speaking and Indo-European speaking peoples. Comparable studies in America, adventurous as they may seem to contemplate, might prove crucial in situations where the isolated attack of ethnology, archaeology, or linguistics yields only conjecture.

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*The Ape and the Child: A study of environmental influence upon early behavior.* W. N. AND L. A. KELLOGG. (xiv, 341 pp., illus. \$3.00. New York: Whittlesey House, McGraw-Hill, 1933.)

How would a baby ape behave if she were reared in a family of human beings and treated as if she were one of them rather than an animal pet? To answer this question Professor and Mrs Kellogg kept a baby chimpanzee in their home for nine months, and treated her, as nearly as possible, as they did their only child, a baby boy two and one-half months older than the ape.

They found that the ape, treated as a child, behaved very much as a child. With regard to clothing, bathing, eating, playing, etc., the ape and the child were very much alike. In her social behavior, too, the little chimpanzee was strikingly "human:" she was affectionate, dependent upon friendly and protective companionship, liked to "show off," was jealous of attention paid to others, etc. Indeed, the

authors felt that in many respects the ape had become more "humanized" than the child (p. 315). The ape learned to do many things before the child did. In some cases this was due to her greater physical maturity, although more than two months younger. But in other situations her superiority did not rest upon physical prowess, as, for example, her superior comprehension of words and phrases and in her better mastery of eating with a spoon.

The child, in his turn, was affected by companionship with the ape. He attempted to emulate her feats of climbing, and frequently imitated her "food bark."

The authors emphasize their conviction that it was not merely living in a house in contact with human beings that made the ape's behavior what it was. It was participating in the intimate socio-psychological intercourse of the family that "humanized" her. And it is this characteristic that makes the Kellogg's experiment differ from all others conducted with apes, so far as the reviewer is aware.

A word of caution might be injected by the reviewer concerning the use of the word "human" in connection with the ape's behavior. Although she behaved like the child her behavior cannot, strictly speaking, be called human. Neither can the behavior of the child before speech is acquired. Human behavior may be said to be primate behavior in which symbolizing (articulate speech, or some other form) plays a part. According to this criterion, the behavior of both ape and child was merely primate behavior, not human behavior. Moreover, the experiment showed quite clearly that the ape has no potential capacity for articulate speech (symbolizing, and hence, human behavior). What we have in this experiment, then, is not an instance of "humanization" of an ape, but a demonstration of the fact that speechless, infant primates of different, but morphologically similar, families will behave very much alike in similar physical, social and psychological situations.

Professor and Mrs Kellogg deserve much credit for this patient and painstaking study. Their story is told with a vivid intimacy that is delightful, and withal, a sober scientific weighing of judgments. *The Ape and the Child* is an important contribution to the study of primate psychology and behavior.

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## DISCUSSION AND CORRESPONDENCE

### MAMMOTH OR "STIFF-LEGGED BEAR"

In a recent number of the *AMERICAN ANTHROPOLOGIST*<sup>1</sup> an article by Dr W. D. Strong brought to light some interesting and supposedly significant interpretations of tales which he recorded among the Naskapi Indians of Labrador. The purport of Dr Strong's conservatively phrased presentation was to contribute to a discussion which he, as well as others for over a century, has maintained as concerns the memory in native tradition of the actual existence of the mammalian monster in post-Pleistocene times. Dr Strong quoted me as one of his sources for contributory data obtained during my research among the Penobscot of Maine, which is cause at the present time, I take it, for reverting to the material and commenting upon opinions contained in his article. And in addition there is something more to be said.

The Naskapi tales which he gives are versions of a narrative widely known among the northerners, of which I have taken some examples that should be included in the series as a prelude to the judgment that I feel urged to give on the matter in question. The following tale from the Mistassini band of Naskapi in the interior of the Labrador peninsula is one to add to the list of versions which Dr Strong refers to in his compilation of published and unpublished occurrences. The tale described events attributed to the career of Tsəka'bec, the trickster-transformer of the northern Algonkian, in which a monster beast known as Katci'to'wəck'w appears as a man-eating colossus, elephantine in size and, according to Dr Strong, in its implied zoological characteristics. The term Katci'to'wəck'w is translatable as "Stiff-legged Bear," and is recognized by interpreters of both Naskapi and Montagnais proficiency as being referable to an animal of the Ursidae.<sup>2</sup>

The tale as recorded in text from the late ex-chief of the Mistassini band, Charlie Metowecic, is as follows.<sup>3</sup>

<sup>1</sup> W. D. Strong, *North American Traditions Suggesting a Knowledge of the Mammoth* (*American Anthropologist*, Vol. 36, 1934), p. 81.

<sup>2</sup> The proper name as given extends entirely across the Cree, Montagnais-Naskapi area without much dialectic variation. Its analysis is as follows: tci'towa'o, "he is stiff," referring to an arm or leg;—əck, final position stem substantive for "bear," viz. məck'w, "bear," wapəck'w, "white bear," natəckwa'n, "where they hunt the bear" (Natashkwan, a Montagnais-Naskapi band and place name on north shore of the Gulf of St. Lawrence).

<sup>3</sup> Some rather trivial personal circumstances surrounding the so-called "mammoth" quest among the Montagnais-Naskapi may be interesting enough to insert here. It happened that I was staying with the Indians at Lake St. John in the winter of 1930 engaged in a research task supported by the Faculty Research Fund of the University of Pennsylvania, when Dr Strong released to the newspress (*Boston Sunday Post*, Jan. 12, 1930) his notes and opinions which lead to the public impression that the mammoth was a contemporary of man in North America. A telephone message from the *Boston Post* called me to the trader's store from one of the camps asking for confirmatory evidence. The response, declined at the time, is given now. The message, however, had an effect. It provoked a discussion over the subject with two chiefs of the bands present, bringing forth during the course of the night session the results embodied in these notes.

*Katci-to'wack'<sup>4</sup>, The Stiff-Legged Bear*

Tsəka'bec was living near a mountain with his sister. Every time he started to go hunting he would begin to sing and so she would know he was about to leave. This time she began to fear for him and said "Don't go over that mountain or you will be killed. There is a monster living there who eats people." "Do not say that," he replied. "Every time you tell me not to go somewhere it makes me afraid. Why do you not want me to go over there? What people has the great beast eaten?" "He has killed and eaten our dear parents," said his sister. "Oho! How can he be known, and what is he called?" added Tsəka'bec.

"He is very large, as large as that rock, and he is called Katci-to'wack'w, 'stiff-jointed bear.'<sup>4</sup> Do not go over there because he will eat you too and then I will be alone," and she began to cry. Tsəka'bec, however, took his bow and arrows and went over the height of land to the place where the monster was said to live. When he got to that country, he went along looking for signs of Katci-to'wack'w and singing his hunting song, "Stiff-jointed bear, I am hunting him," over and over. At last he met a great white beast, and prepared to attack him. "Are you Katci-to'wack'w?" he asked him. "No, I am not. I am White Bear." "Oh, then you are not the one I am looking for," said Tsəka'bec. "Where can I find him?" "He lives farther on, and is very dangerous. No arrow can kill him and if you go there he will eat you as he has others." "I am not afraid of him. I am going to attack him," and Tsəka'bec went on singing his hunting song. At last he found Katci-to'wack'w. He was as big as a hill of rock. Said Tsəka'bec, "Are you Katci-to'wack'w?" "Yes," answered the beast, "Why do you come here? Do you not know that you will be eaten?" "No, I will not be eaten, I have come here to kill you. I am Tsəka'bec." "You can not kill me with your arrow. Do you see that tamarack tree there? I am tougher than that, and your arrow will break to pieces when you shoot." "If that is all," said Tsəka'bec, "My arrow will kill you. See that," and he shot an arrow into the tree and the tree shattered to fragments. "Well now," said Katci-to'wack'w, as he began to be afraid, "I am even tougher than that rock." He pointed toward a rock nearby. Then Tsəka'bec said, "I can kill you even so," and he shot his second arrow at the rock and the rock broke into pieces. With this Katci-to'wack'w became frightened and ran away to escape. Then Tsəka'bec quickly went to the rock and pulled his arrow out, and shot in the direction of the running beast without even being able to see him. So good was his aim that it struck the monster and he fell down dead. Tsəka'bec followed after and there he found Katci-to'wack'w shot dead with the arrow. Then he took his knife and cut him open to see if he had eaten their parents. When he cut Katci-to'wack'w open he found in his stomach two bunches of hair, one a man's tied behind with the "hair-tie," the other a woman's tied on each side with the woman's "hair-ties."<sup>5</sup> Then he took the hair-ties and started back to his camp. When he came, his sister was waiting for him. He showed her the hair-ties and she knew by them that Tsəka'bec had killed the monster who had eaten their parents.

The trend of opinion among the informants is that the tale in question relates to a carnivorous monster of the bear kind living in a former age when Tsəka'bec was upon earth. A mythological creature! And with this view I coincide.

<sup>4</sup> The Montagnais living nearer to the settled country, who have seen pictures of animals of the world, give this name to the elephant. I found one hunter who had seen an elephant himself and said it was correct.

<sup>5</sup> The Montagnais and Naskapi women wear their hair fastened on a small block of wood over each ear, bound with a wrapping of braid or woven bead strips.

Indulging in a glance again at the reference to the supposed "mammoth" of Wabanaki mythology to which Dr Strong gave some attention, I would like to order the analysis of the native term denoting the monster as it appears in several tales recorded in Penobscot text. An English version of the legend of White Owl, containing reference to the hairy stiff-legged monsters, was published some years ago and to which I would point attention at this time.<sup>6</sup> The translation is quoted below. Much significance, however, lies in the fact that the beast is called Ktc'i'awa's, "Great Beast." It also happens that in another tale dealing with the career of the Penobscot hero-transformer Gluskabe,<sup>7</sup> the animals of the earth are receiving for the first time their human designations. A "great beast" (ktc'i'awa's), which Gluskabe slays, is brought to his patron grandmother for identification. She declares it to be of the bear kind. This links the generic awa's, "beast," with the ordinary term in the Wabanaki languages for "bear," namely awa's'u's (St. Francis Abenaki), awe's'sus, (Penobscot). (Penobscot replaces St. Francis a in general with e.) Here, it seems we have the coincidence of terms and identities of the mythical behemoth in question with Ursine monsters of mythical narratives—creatures of as fantastic an origin as the episodes of the tales themselves.

The pertinent sections of both the Penobscot and Naskapi (Mistassini) tales alluding to the "Great Beast" are now given as my final contribution to the interesting and no doubt important discussion evoked by Dr Strong's compilation of evidence in story for palaeontological identification.

I may point out that the Penobscot tale is somewhat more explicit as to the physical peculiarities of the "Great Beast" in that its shaggy hair and its habits of leaning against trees for repose, since its legs were stiff-jointed, preventing it from rising again should it ever fall or lie down, are mentioned definitely. In another narrative the beast is described as having teeth long enough to pierce seven hunters, a lip long as "seven paces," and an unconquerable strength until destroyed by the will of Gluskabe "never to be seen by any of my children (the Indians)." Nicolai, the author of the tale, denotes the beast a mammoth, par-sar-do-kep-piart, a term which as yet baffles analysis.<sup>8</sup> In the Montagnais-Naskapi tales these details are wanting, and the informants when asked did not know more than that the creature was "stiff-legged" by tradition.<sup>9</sup> The legend is evidently a very old property of the Algonkian of the northeast on both the Atlantic and St. Lawrence coasts, losing some of its particulars in the course of time as transmitted among separated groups of the people in Maine and in the Labrador peninsula.

<sup>6</sup> F. G. Speck, One of Caesar's Anecdotes among the Indians of Eastern North America (The Alumni Register, University of Pennsylvania, Vol. 19, No. 9, June 1917, pp. 686-90).

<sup>7</sup> F. G. Speck, Penobscot Transformer Texts (International Journal of American Linguistics, Vol. 1, No. 3, 1918), p. 190, line 7.

<sup>8</sup> Joseph Nicolai, The Life and Traditions of the Red Man (Bangor, Me., 1893), pp. 36-38.

<sup>9</sup> As to having long tusks the informants knew only of the walrus (wipitco', "toothed") as such a creature in Naskapi.

*White Owl, His Escape from the Witches and the Great Beasts*<sup>10</sup>

There was a village divided into two parts, one a quiet part and the other a boisterous part, the latter abusive to the former. Here lived a family in which there was a son named Snowy Owl. At last the family moved away to a divide in the mountains and finally the father and mother died. Snowy Owl was instructed by his dying father to send his "spirit helper" to tell his grandmother that he would come to live with him. This was done and the grandmother became the foster parent of Snowy Owl. One day she gave Snowy Owl his grandfather's bow and arrows which consisted of some pieces of ivory which she put together, telling him that whatever he shot at his arrow would not miss. Snowy Owl next started out to find the camps of human beings to get him a wife. He traveled far to the south, and on the way noticed how the lakes and rivers were drying up. Desiring to learn the cause of the water shrinkage, he ascended the valleys and finally reached a place where he saw what he thought were hillocks covered with brown vegetation moving slowly about. Upon closer scrutiny he learned that these masses were really the backs of great animals with long teeth, *animals so huge that when they lay down they could not get up.*<sup>11</sup> He saw that they drank for half a day, thus taking up all the water in the basins of the land. Snowy Owl decided that some day he would have to kill them.

He journeyed on and came to a place where a great witch lived with her seven beautiful daughters. Whenever these women desired food the daughters went abroad and lured suitors to come home to marry them. On the night of the marriage it was their custom to tell the husband-to-be that he should have his hair combed. Whereupon they combed his hair and combed out his brains with a magic comb, eating the brains from it. The pain could not be felt. When at last the victim cried "Pi u!" they sent him out, where he traveled to the north and lived with the great white rabbit. (This creature lives on a high peak, always surrounded by snow where no one can reach him. When he sees anyone coming he causes it to snow until the visitor is overwhelmed. Only those who have had their brains combed out are allowed to come, whereupon they are transformed, like their overchief, into white rabbits and live on grass.) The youngest daughter of this family, who had never yet had a husband, fell in love at once with Snowy Owl, and knowing the fate intended for him, decided to spare him and escape with him. The mother of the girls was a "night witch" who had no power during the day. Now, the girl made caps for her mother and sisters and told them they were to wear them to celebrate her wedding that night. When they put them on they fell asleep. Snowy Owl and the girl at once took flight. They ran all day, followed by the cry of the old witch, harmless during daylight, "You can't escape me, for I'll get you tonight." At last the pair came to a big wigwam and asked the old man to help them in their flight. This was the wigwam of "Great Star," known as the Morning Star. (He is said to sleep so late that he is the last star to retire in the morning.) He took them in and hid them. When night came the old witch started in pursuit and reached "Great Star's" wigwam, asking him about the pair. "All right," said he, "my dog and I will help you. But wait till I put on my moccasins, my coat, my leggings my belt." He was very slow, and took so long a time to put on his things that when he opened the door of his wigwam there stood the Morning Star. Night was over and the old woman became powerless for the day. The pair then fled to the north where the seven Thunder Brothers live. When they arrived at the home of the Thunders, one of them said to another,

<sup>10</sup> Narrated by Newell Lion. The informant mentioned that a similar tale is known to the Passamaquaddy Indians at Eastport, Maine.

<sup>11</sup> Ktci awa's, "Great Beast." This is not the specific name in Penobscot of any existing animal.



"Open your eye only a little to see who is coming." He opened his eye a little and the lightning flashed forth from his lids, but not enough to hurt the lovers. They were taken in and when night fell the old witch came likewise. Whereupon the seven Thunders opened their eyes wide, the lightning flashed and shattered the mother witch to pieces. Now, Snowy Owl and his wife settled down in safety.

*Snowy Owl proceeded then to find the monsters which he had seen before. He went to where the animals had their "yards." He cut certain trees, where he had observed the monsters were accustomed to lean for rest at night, almost through, so that when the monsters would lean on them they would break. When the creatures went to rest at night leaning against the trees, they fell upon the sharpened stumps when the top bent over and broke, and could not get up again, and Snowy Owl shot them all.*

Thereupon the water basins filled up again and water became plentiful. Snowy Owl and his wife returned to the village and the people were glad to welcome him for what he had done, and they made him their chief.

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### ONCE MORE MASCOUTENS

It would be easy to give several additional references showing that the Mascoutens could not possibly be the same as the Potawatomi,<sup>1</sup> but perhaps it as well to restrict these to a letter of Du Chesneau, dated Nov. 13th, 1681, in P. Margry's *Découvertes et établissements des Français dans l'ouest et dans le sud de l'Amérique Septentrionale* (1614-1764) (Paris, 1875-86, Vol. 2), "du costé du Sud, des Sakis, des Poutoutamis, Puants, Oumalominis ou de la Folle-Avoine, Outagamis ou Renards, Maskoutens, Miamis et Islinois;" to an English translation of Duquesne's letter to Machault, Quebec, dated October 13th, 1754, in Documents relating to the colonial history of the state of New York (Vol. 10: 263), "Poutwatamis, Kickapoux, Mascoutins and Scioux of the prairies, have assembled together to go and destroy the Peorias;" and to an English translation of a letter of Father Marest, dated at Kaskaskia, Nov. 9th, 1712, *apud* J. P. Dunn, Jr. (Indiana, Boston and New York, 1905, pp. 36, 37), "Father Mermet was sent to them. This father thought he ought to labor for the conversion of the Mascoutins, who had made a village on the banks of the same stream—this is an Indian nation using the Illinois language." In this connection it may be well to make some additional quotations from Forsyth: "a numerous nation of Indians who called themselves Linneways and called by other Indians, Ninneway (literally, men) this great nation of Indians were divided into different bands . . . Michigamians . . . Cahokians . . . Kaskaskias . . . Tamorois . . . Piankishaws . . . Weahs . . . Miamies . . . Peorias (being another band of the same nation) lived and hunted on Illinois River: also the Masco or Mascotins called by the French Gens des Pirarie lived and hunted in the great Piraries lying between the Illinois River and the Wabash, etc." (see Forsyth, *apud* Blair, *Indian Tribes of the Upper Mississippi and Great Lakes Region*, Cleveland, 1912, Vol. 2:

<sup>1</sup> See *American Anthropologist*, Vol. 36: 226-33, 1934.

200-201). "The Sauk and Fox . . . were attacked by a party of Mascota or Mascotins. . . . The Sauk Indians attacked a small village of the Peorias" (Forsyth, *loc. cit.*, pp. 202, 203). Incidentally I may note that on p. 178 of Margry, Vol. 2, in a letter of La Salle (which letter is of uncertain date, apparently in 1681) we learn of a Mascouten chief "Un Sauvage nommé Kiskirinanso ce que veut dire 'Bœuf sauvage coupé.'" If this personal name is reasonably well recorded (confirmed as far as the vital point is concerned by Kikirinous, given by La Potherie, *apud* Blair, Vol. I: 370; Miss R. Flannery tells me this is not a misprint) we have linguistic proof of the Illinois affiliations of the Mascoutins. The word corresponds to the Fox personal name Kĩ'ckinenu'swa which belongs to the Bear gens.

MA'cktä'ag<sup>ki</sup> (AMERICAN ANTHROPOLOGIST, Vol. 36: 229) is a misprint for MA'ckuta'ag<sup>ki</sup>.

Wallace A. Brice in a footnote on p. 3 of his History of Fort Wayne (Fort Wayne, 1868) says, "The Mascoutens, says Gallatin, dwelling about Lake Michigan, were a branch of the Miamies." Gallatin says nothing of the sort (see p. 61 of his Archaeologia Americana, Amer. Antiq. Soc. Transactions and Collections, Vol. 2, 1836): "The name 'Mascontens' was therefore used to designate 'prairie Indians.' And it appears that they consisted principally of Sauks and Kickapoos, with an occasional mixture of Potawatomes and Miamies. . . ."

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### THE INTERPRETATION OF MELANESIAN DESIGN: A REVIEW

Miss Gladys A. Reichard has given to students of art and to ethnologists a most valuable and sumptuously illustrated memoir on certain aspects of Melanesian decorative art.<sup>1</sup> Instead of elucidating the style of the decorative art of definite localities by a study of all kinds of objects, she has analyzed chosen specimens from a given locality which might be expected to typify the local style; this mode of approach was selected as the problem she set herself was primarily aesthetic rather than ethnological. With the richness of decorative art in Melanesia some selection was necessary and she adopted the following criteria:

1. The objects studied must be beautiful from my own point of view 2. Each sort should have a fairly wide distribution 3. There should be some overlapping in the occurrence of the objects chosen within a given area. 4. The art should be as free as possible from symbolic or religious significance since that may be understood only by questioning the natives

The carved wooden bowls of the Admiralty Islands were chosen primarily for the study of the form; those of the Tami Islands (north of Huon Gulf) were found to be especially interesting on account of the elements and the composition of the

<sup>1</sup> Gladys A. Reichard, *Melanesian Design: A Study of Style in Wood and Tortoiseshell Carving* (I: Text, 172 pp., 75 figs., frontispiece, map; II: 151 pls. containing 600-700 figs. \$10.00 New York: Columbia University Press, 1933).

designs. Widely distributed are the ornaments of turtleshell fretwork attached to a white shell disc: for these the name of kapkap is adopted from one of the languages of New Ireland. Some attention is also given to decorated lime-gourds. The selection is satisfactory for the purpose, but as Miss Reichard observes: "No matter at what point one starts, an intensive study leads into all sorts of byways."

It has long been recognized that it is possible, by its form and decoration, to allocate an unlabelled specimen to a definite area or people. Although two objects are seldom exactly alike, certain traditional conventions limit the artist from straying far afield. The artist works along lines within which he feels at home and which appeal from wont to his fellow countrymen, such for example as the choice of the elements of the design, their disposition with regard to the surface of the object, the use of rhythm, symmetry or asymmetry, symbolism, and so forth. Any of these principles may be worked out in a variety of ways according to the skill or originality of the artist, but always within definite limits. Occasionally the introduction of an alien form or design produces a disturbance in the local style and if permanently adopted creates a new style.

An attitude of sane scepticism is taken by the author in discussing the evolution of designs. It is pointed out that too many factors combine to make valueless the interpretation of forms by Europeans and I am incorrectly cited for an interpretation of a Tami design, though I admit I have in other instances ventured on suggestions. The author even rejects (p. 6) the explanation given by natives and recorded by careful observers, such for example as Dr W. G. Ivens. She says:

these seem to me flights of fancy comparable to metaphor and figure of speech in literature . . . Such poetic fancies seem to controvert the theory that primitives do not make abstractions. They are, however, independent of the forms to which they are attached and comparisons of form and significance should be kept separate

But one of the instances she contemns is a realistic diagram of a waterspout given by Ivens. More respect is paid to the conclusions of Dr H. Stolpe as "he had considerable experience with natives," but so had Dr Ivens and others also. It is possible that Miss Reichard would considerably modify her views if she could do field-work in Melanesia. Any field-worker will acknowledge that apparently contradictory explanations are sometimes given for the same design. This uncertainty may be due to the fact that these informants do not know everything about the subject or that they may be reluctant to tell; at all events it may be admitted that the artist himself generally knew more or less what he was doing. On the other hand it is recognized that in some cases the design has so long been conventional that it has ceased to have a definite meaning and it is possible it may never have had one.

Dr Preuss correctly identified a certain representation as a hanging Pteropus and he allocated to this motif a series of stylized designs. Miss Reichard (p. 16) says: "Many scientists . . . regard the pattern with the two upcurved elements always as the 'flying dog.'" So far as I am aware only Preuss has studied this design—the characteristic feature of which is the downturned wings—and neither he nor anyone else has correlated it with the Admiralty design. She adds: "those ele-

ments may be only *names* and not symbols in any sense of the word, in which case my 'fleur-de-lis' label is as good as some one else's 'flying dog.' " We are dealing here with only one name which has been applied to designs on the north coast of New Guinea. The term "fleur-de-lis" for a quite different design of the Admiralty Islands may be good enough as a label, but it is meaningless so far as Melanesia is concerned. It is probable that these two designs, like very many others, are symbolic, but that does not prevent either from being "also regarded as a decorative motive, the elements of which may be played with and rearranged at will." When discussing the Santa Cruz kapkaps (p. 115), she is characteristically sceptical about the meaning of the design that is generally regarded as a frigate bird. She adds: "it is even probable that there might be none [no name] at all" for the design, which is doubtful. She also says Preuss "calls practically the same design 'the hanging Pteropus,'" but it is not "practically the same design," as the bat is hanging head downwards, and the forked tail of the frigate bird occupies the position of the head of the bat.

Miss Reichard (p. 7) evidently considers it futile to seek for origins. She says:

The rewards of objective analyses are inherent in art itself. They consist of formulating the principles underlying the art style; the definition of elements, their combination into an organic whole; attitudes towards zones and fields, toward filling of spaces; preference for regularity, symmetry or asymmetry, or rhythmic repetition.

This defines her line of approach and she has carried out this new and profitable investigation with great skill. I must however confess that I cannot accept the argument of pp. 13, 14. It would seem that she analyzes the parts of a human or crocodile design into separate geometric elements and thinks the native artist regarded them as distinct art-forms; by playing about with them, they surprisingly became grouped into designs which resemble men or crocodiles. It is her considered opinion (p. 59) that "designs and elements are more formal than representative, and that they can be moved about in any combinations from man to fish to crocodile or lizard at will" and she points out "the possibility—strong probability—of development from geometric to representative rather than the reverse."

Despite her reluctance to deal with objects of a symbolic or religious significance, the author was forced to do so when dealing with the Tami bowls (p. 56). Our knowledge of Melanesian symbolism is very imperfect, but we know enough to believe that it permeates a great deal of their decorative art. Miss Reichard's reticence is praiseworthy, as she does not want to make assumptions that cannot be proved, but most ethnologists will be of the opinion that to a considerable degree the decorative art has for the natives an emotional value which may aptly be termed religious. On the other hand there is no reason to believe that a large number of designs and patterns are otherwise than secular. As she puts it (p. 6): "ornamentation may be an outgrowth of pleasure in control of technique, it may be applied to purely utilitarian objects, or, it may, like sculpture, become social or religious."

The final chapter "General principles of Melanesian art" is a very valuable study which gives a great deal of information concerning the distribution of various motifs.

Naturally there are points upon which opinions may differ: to take but one, I doubt whether there is "adequate proof of influence [between Tami and the Massim] in both directions, if not intimate, then indirect," since the artistic "feeling" of the two areas is so very distinct; but this is not the place to enter into a detailed discussion. I would like to end up this review with recording my sincere congratulations to Miss Reichard on the able manner in which she has opened up a new method of approach to the study of Melanesian art, and incidentally to that of other peoples of analogous culture. The ethnologist has too frequently neglected the purely aesthetic aspect of native handicraft, and studies such as these are needed to rectify this omission.

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## NOTES AND NEWS

THE REPORTS OF THE COMMITTEE ON STATE ARCHAEOLOGICAL SURVEYS, National Research Council, which in earlier years appeared in the AMERICAN ANTHROPOLOGIST, will be issued in the Circular Series of the Council. The full text of the report *Archaeological Field Work in North America, 1933* (Circular Series, No. 18) may now be obtained from the Division of Anthropology and Psychology National Research Council.

"VOYAGEURS, ROBES NOIRES, ET COUREURS DE BOIS: Stories from the French Exploration of North America," edited by Charles Upson Clark, has been brought to attention as a work especially well adapted to the needs of students acquiring a reading knowledge of French. It contains numerous excerpts relating to the Indians from the Jesuit Relations and Margry's "Découvertes," with partially modernized text and explanatory notes. (Published by the Institute of French Studies, Inc., 391 pp., \$2.75; orders may be placed with Professor G. L. Van Roosbroeck, 504 Philosophy Hall, Columbia University, New York City.)

"DOCTORAL DISSERTATIONS ACCEPTED BY AMERICAN UNIVERSITIES, 1933-1934" (Donald B. Gilchrist, ed.), compiled for the National Research Council and the American Council of Learned Societies by the Association of Research Libraries, is a first complete annual list, with adjunct data on printing requirements, library loans, etc. The primary intent is that directors of graduate research be able to keep track of what is going on in the various fields in order to avoid duplication. (Published by the H. W. Wilson Company, 950 University Avenue, New York City; No. 1, xvi, 88 pp., \$1 postpaid.)

DR MARTIN GUSINDE reports (August 29, 1934) that he and Dr Schebesta have been working for four months among the Pygmies of the upper Ituri. Dr Gusinde has also made somatological observations on the Batwa of Ruanda. Before leaving Modling he completed, as a sequel to his Selk'nam (Ona) monograph, the manuscript of a second volume devoted to the Yamana (Yaghan). The series is to close with a third volume on the Halakwulup (Alakaluf) and the somatology of all the Fuegians.

DR MAURICE FISHBERG, chief physician of Montefiore Hospital, whose "Materials for the Physical Anthropology of the Eastern European Jews" was published as Memoirs of the American Anthropological Association, Vol. 1, No. 1, died suddenly on August 20th, aged 62.

WE DEEPLY REGRET TO ANNOUNCE THE DEATH OF DR ROLAND BURREAGE DIXON, Professor of Anthropology, Harvard University, on December 20th, aged 59.

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## HUMAN BLOOD GROUPS AND ANTHROPOLOGY

By LELAND C. WYMAN  
AND WILLIAM C. BOYD

### INTRODUCTION

THE discovery by the Hirszfels<sup>1</sup> on the Macedonian battlefield during the World War that the percentage of persons belonging to each of the four blood groups was different in various racial groups had such obvious anthropological importance that it aroused hopes for the solution of problems of race which have not been fulfilled. It is the purpose of this paper to examine the reason for this failure, and to state what in the authors' opinion is the probable future usefulness of blood grouping in anthropology. Certain recently discovered facts, and certain possible re-interpretations of old ones, seem to call for such an examination.

*Nature of the Blood Groups.* Many of our readers are no doubt already fully familiar with the classical Landsteiner blood groups, and probably with the newer factors M and N discovered in 1927. Such readers are advised to omit this section, and to accept our apologies for inserting familiar material. However, the authors feel that not all those interested in this field are fully informed as to the basic mechanism involved, especially in regard to the newer factors, and since there seems to be no summary of the whole subject intended for those not specializing in it, this section has been prepared.

The Landsteiner blood groups, discovered in 1900<sup>2</sup> depend on the presence or absence in the red blood cells of the individual of two chemical substances, called A and B. These designations are given because their chemical nature is still largely unknown. One of them, A, has been isolated in presumably pure and certainly very active form.<sup>3</sup> It seems to be carbo-

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<sup>1</sup> L. Hirszfeld and H. Hirszfeld, Serological differences between the blood of different races (Lancet, Vol. 180, II, p. 675, London, 1919).

<sup>2</sup> K. Landsteiner, Ueber Agglutinationserscheinungen normalen menschlichen Blutes (Wiener Klinische Wochenschrift, Vol. 14, p. 1132, Vienna, 1901).

<sup>3</sup> B. Brahn, F. Schiff, and F. Weinmann, Über die chemische Natur der Gruppensubstanz A (Klinische Wochenschrift, Vol. 11, p. 1592, Berlin, 1932). See also footnote 1 in F. Schiff, Die allgemeinen Grundlagen der Blutgruppenlehre (Deutsche Medizinische Wochenschrift, p. 199, Leipzig, 1933.)

hydrate in nature. Thus these substances have been shown to be real, not merely hypothetical. The way in which the presence of one, both, or neither of these determines the four blood groups is shown in Table 1.

TABLE 1. THE LANDSTEINER BLOOD GROUPS

<i>Nomenclature of</i>			<i>Substance in cells</i>	<i>Agglutinin in serum</i>
<i>Jansky</i>	<i>Moss</i>	<i>Landsteiner</i>		
I	IV	O	—	anti-A and anti-B
II	II	A	A	anti-B
III	III	B	B	anti-A
IV	I	AB	AB	—

These substances are inherited as Mendelian dominants: the two of them, together with the factor O (absence of either) form a series of triple allelomorphs. Thus the groups themselves are not inherited as entities; but since the substances determining the groups are inherited, the mode of hereditary transmission is easily seen. This has been checked by studies on thousands of families, and no single completely proven exception has ever been observed.

It was later found that there are two kinds of A, called  $A_1$  and  $A_2$ .<sup>4</sup> For our purposes it may be assumed that either of these behaves simply as A. However, there may be anthropological importance in their world distribution. This point will be discussed later.

Much more recently it has been found<sup>5</sup> that there are two other factors (this word is used since we do not yet know them to be substances) in human red blood cells. These are called M and N. They are inherited exactly like A and B, except that one or the other, or both, but in no case neither, must occur in each individual. Thus they form an ordinary allelomorphic pair. Their inheritance is quite independent of that of A and B, and their genes are evidently on different chromosomes. These groups are not really subgroups of the four classical groups, but determine three in-

<sup>4</sup> K. Landsteiner and D. H. Witt, Observations on the human blood groups. Irregular reactions Isoagglutinins in sera of group IV. The factor  $A_1$  (Journal of Immunology, Vol. 11, p. 221, Baltimore, 1926); O. Thomsen, Undersøgelser over Arvelighedsforholdene af de menneskelige Blodtyper ("Blodgrupper") med særlig Henblik paa Muligheden af to nye Typer kaldet  $A'$  og  $A''B$  (Norsk magasin for lægevidenskaben, Vol. 91, p. 369, Oslo, 1930); A. S. Wiener and S. Rothberg, Heredity of the subgroups of group A and group AB (Human Biology, Vol. 5, p. 577, Baltimore, 1933).

<sup>5</sup> K. Landsteiner and P. Levine, On individual differences in human blood (Journal of Experimental Medicine, Vol. 47, p. 757, Baltimore, 1928).

dependent groups; just as all men might be divided into tall and short, and quite independently of that into property-owners and non-owners. A very full elementary discussion of the inheritance of the blood groups is given by one of the authors in a paper to which those interested particularly may turn.<sup>6</sup>

Other properties of the blood still being investigated, such as Landsteiner's "P" and Schiff's "G" may possibly prove to have application to anthropology. Also, though it is not strictly related to our subject, we may mention that the frequency of certain other human gene substitutions such as the ability or inability to taste certain synthetic organic compounds, has been found to vary in different peoples. We may expect other such cases to be found; perhaps Schiff's "secreting" factor may provide one.

*Methods of determining blood groups.* The determination of the Landsteiner blood groups is not difficult, and a simple method applicable in the field will be described here. Those already familiar with this technic are advised to turn at once to the section on applications to anthropology. Determination of the M and N groups demands training, and it is thought that none but those with such training should attempt it. References<sup>7</sup> are given to papers where the technic is described. It is probable that most workers in this field would be glad to give the necessary instruction to anyone interested. Personal instruction is almost essential.

The substances A and B are detected in the red blood cells by the use of agglutinins. These are substances having the power of causing the red cells they react with to stick together in clumps (agglutinate). By injecting cells containing A into a rabbit, an agglutinin capable of agglutinating cells containing A is produced; similarly for B. These artificially produced agglutinins are not usually used for determining the Landsteiner blood groups, since similar agglutinins occur naturally in human serum (see table 1.) Thus, the serum from the blood of a person in group A contains an agglutinin which agglutinates cells of group B, and vice versa. Serum of group O will agglutinate either A or B cells. Serum of group AB will agglutinate no cells. Then in order to determine the group to which a blood belongs it is only necessary to test it with group A and group B

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<sup>6</sup> S. B. Hooker and W. C. Boyd, Blood-grouping as a test of non-paternity (Journal of Criminal Law and Criminology, Vol. 25, p. 187, Chicago, 1934).

<sup>7</sup> A. S. Wiener and M. Vaisberg, Heredity of the agglutinogens M and N of Landsteiner and Levine (Journal of Immunology, Vol. 20, p. 371, Baltimore, 1931). See also footnote 5. Also A. S. Wiener, R. Zinsher, and J. Selkove, The agglutinogens M and N of Landsteiner and Levine (Journal of Immunology, Vol. 27, p. 431, Baltimore, 1934).

sera. If serum of group A agglutinates the unknown cells, they contain the substance B. If B serum agglutinates them, they contain A. Thus the group is determined. Table 2 will make this clear.

TABLE 2. METHOD OF TYPING BLOOD (UNKNOWN CELLS AND KNOWN SERA)

	<i>Agglutination</i>			
	—	+	—	—
Left hand tube (group A serum)	—	—	—	—
Right hand tube (group B serum)	—	+	—	—
Group	O	A	B	AB

The actual technic follows, quoted from Landsteiner:<sup>8</sup>

... set up the tests in small tubes (7 mm. diameter) using one drop each of serum, saline, and bloodcell suspension (equivalent to 2.5 percent normal blood). This dilution of the serum is generally sufficient to prevent pseudo-agglutination. The emulsion can be prepared simply by mixing a few drops of blood with the necessary amount of saline solution; citrated blood may also be used, preferably after washing. The tubes are shaken several times, and a drop of the mixture is taken up by means of a thin glass rod and examined microscopically with low magnification. The reaction occurs generally within a few minutes. In order to detect unusually feeble reactions the negative tests are re-examined after one hour. Control tests with known cells A and B should be included. Special care must be taken to select test sera of known high agglutinating power.

To this the present authors would add that in dry atmospheres the tubes should be stoppered, and that direct examination of the tubes with a hand lens is usually sufficient. The test sera are conveniently preserved as well as very clearly marked by use of the dyes recommended by Rosenthal,<sup>9</sup> but should be kept in the ice box as much of the time as possible. Landsteiner's advice to select strong sera is quite important.

The chief sources of error which should be kept in mind are pseudo-agglutination and weak sera. If the sera to be used are first tested by the worker himself against a few known bloods, and controls are used whenever possible in the field, there is little to fear from either source of error.

As mentioned above, the determination of M and N is not quite so easy, and is not described here, but it is hoped that the impression has not been given that it is too difficult to be learned. The determination of the subgroups of A is just as easy as that of the Landsteiner groups. The

<sup>8</sup> K. Landsteiner, *The human blood groups* (in *Newer Knowledge of Bacteriology and Immunology* [Chicago], 1928, p. 905).

<sup>9</sup> L. Rosenthal, *The staining of blood-grouping sera for preservation and identification* (*Journal of Laboratory and Clinical Medicine*, Vol. 16, p. 1123, St. Louis, 1931)

test sera will however have to be prepared by some one working with blood groups.

It should be mentioned that anthropologists not able to make determinations of blood groups themselves could nevertheless coöperate with some blood group investigator by arranging to take samples of blood in the field, and sending them to him. Samples of blood have been sent successfully from Kansas to New York. Blood may not stand transportation well, however, so unless the laboratory were near, the best plan would probably be to dry a few drops of blood on filter paper and send this. By an indirect but reliable method which will be mentioned later, the groups could be determined from these specimens, though not quite so readily. Dried blood appears to remain usable for many years.

#### APPLICATIONS TO ANTHROPOLOGY

Although the amount of information is still far short of the ideal, we are in possession of the results of blood group determinations on many different peoples. Of what value are these to anthropologists? Before answering this question, it would be well to point out that of course the knowledge of the blood groups to which a group of people belong will tell us no more (and of course no less) than knowledge of any other one fact about them. Snyder warns us:<sup>10</sup>

The idea that races carry distinguishing characters hidden in the blood has laid strong hold on the imagination of medical and other research workers; so much so that the idea has been slightly overdone. Many workers in the flush of the new field of operations have not realized that the blood groups are simply additional anthropological characters which must take their place along with other longer-known criteria in the study of racial relationships.

On the other hand it should be mentioned that the blood groups possess one advantage, which is that their mode of inheritance is completely known. Thus we need not wonder if climate has affected the observed group distribution of a given people, or wonder what the composition of the offspring resulting from the fusion of two racial groups of different blood group frequencies would be. To answer these questions is easy, whereas in the case of other characters it may be more difficult or impossible. Also the impossibility of social selection in the case of the groups is of great significance.<sup>11</sup>

<sup>10</sup> L. H. Snyder, *Blood Grouping in Relation to Clinical and Legal Medicine* (Baltimore), 1929, p. 117.

<sup>11</sup> L. H. Snyder, The "laws" of serologic race-classification. *Studies in human inheritance IV* (Human Biology, Vol. 2, p. 128, Baltimore, 1930).

## A. THE LANDSTEINER BLOOD GROUPS

A general picture of what has been found about blood group distribution may be gained from the maps (figs. 1 and 2). On these the frequencies of the *genes* for A and for B are plotted. We may regard these as measures of the frequencies of the substances A and B.<sup>12</sup> The frequencies of the genes are more significant than those of any single blood group. The numerous "indices" which have been suggested are all open to criticism.<sup>13</sup> The maps are based on one by Haldane,<sup>14</sup> redrawn with the aid of data from Lattes,<sup>15</sup> Bernstein,<sup>16</sup> and later data.

If the map for the gene for B (q) is examined, it will be seen that the high center in Asia near the Punjab, and the way the contour lines surround it, are strongly suggestive of the origin of this factor in the region of northern India, and its subsequent spread into other parts of the world, including Europe. The map for the gene A, on the other hand is not so clear. Snyder has said<sup>17</sup> ". . . mutation A probably occurred in Europe, and mutation B in India or the Orient, from which they were later carried and spread by migration." To the present authors the evidence for the occurrence of the mutation A in Europe (in Europe alone, at least) does not seem so convincing. Gates and Darby have recently expressed a similar opinion.<sup>18</sup> Note on the map the high concentration of A found in Japan, among the Australian natives, in Greenland, and most unexpected of all, among the Blood and the Blackfoot by Matson and Schrader in 1933.<sup>19</sup>

<sup>12</sup> These frequencies are easily calculated from the equations  $r^2 = O$ ,  $p^2 + 2pr = A$ , and  $q^2 + 2qr = B$ , where r, p, and q respectively represent the frequencies of the genes for O, A and B; and O, A, and B represent the observed frequencies of the blood groups. One of the authors has given a nomogram by which all three of these equations are quickly solved at the same operation (W. C. Boyd, Nomogram for rapid computation of the frequency of the blood grouping genes in populations, Human Biology, Vol. 6, p. 558, Baltimore, 1934). See also L. Hirzfeld, Konstitutions-serologie und Blutgruppenforschung (Berlin), 1928.

<sup>13</sup> Snyder, Blood Grouping in Relation to Clinical and Legal Medicine, p. 119. See also a book by A. S. Wiener now in press.

<sup>14</sup> J. B. S. Haldane, Prehistory in the light of genetics (Proceedings, Royal Institution of Great Britain, Vol. 26, p. 355, London, 1931 reprinted in Science and Human Life [New York and London], 1933, p. 63).

<sup>15</sup> L. Lattes, The Individuality of the Blood (London), 1932.

<sup>16</sup> F. Bernstein, Die geographische Verteilung der Blutgruppen und ihre anthropologische Bedeutung (Comitato Italiano per lo Studio dei Problemi della Popolazione, Vol. 9, p. 3, Rome, 1931).

<sup>17</sup> Snyder, Blood Grouping in Relation to Clinical and Legal Medicine, p. 134.

<sup>18</sup> R. R. Gates and G. E. Darby, Blood groups and physiognomy of British Columbia coastal Indians (Journal, Royal Anthropological Institute, Vol. 64, p. 23, London, 1934).

<sup>19</sup> G. A. Matson and H. F. Schrader, Blood grouping among the "Blackfeet" and "Blood" tribes of American Indians (Journal of Immunology, Vol. 25, p. 155, Baltimore, 1933).





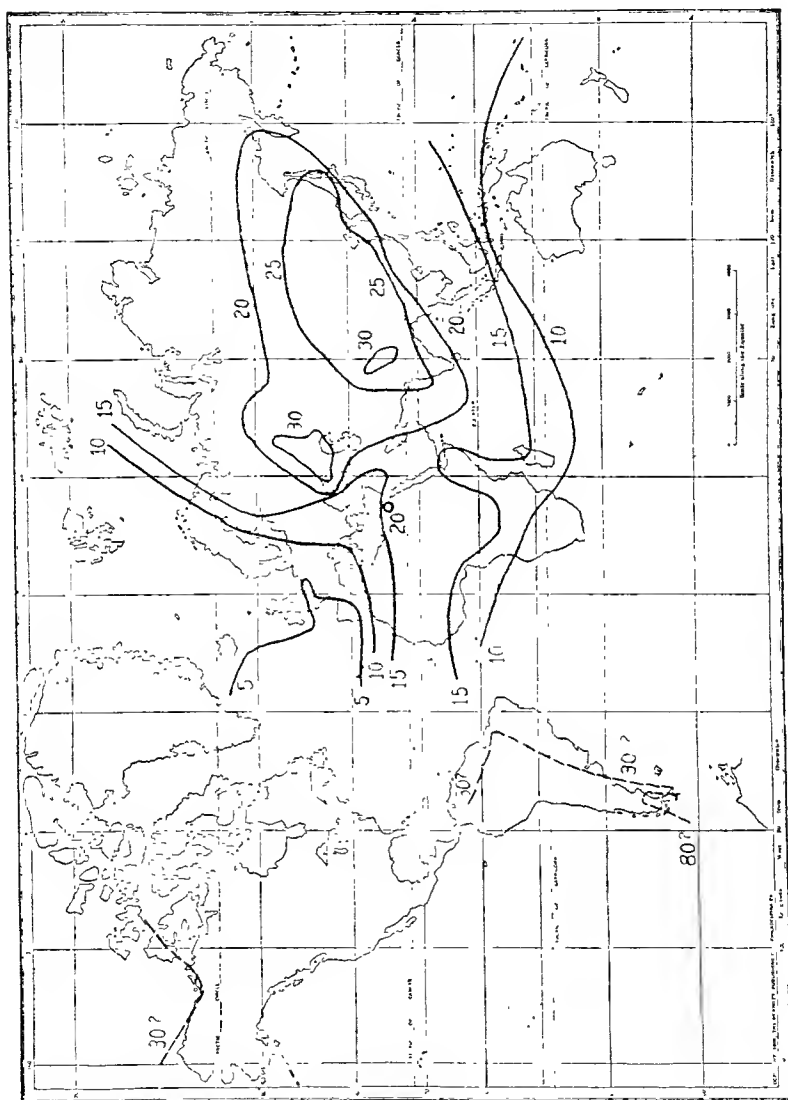


FIG. 2 Distribution of frequencies of the gene for B (q) in percent.

Kroeber has been led by considerations which depend primarily on the peculiar distribution of A to the hypothesis of several independent mutations, in different parts of the world, as the only way of explaining the known facts.<sup>20</sup>

Before choosing between these hypotheses, or elaborating one of our own, let us try to bring out the significance of a fact which is often under-emphasized. This is the fact that there have been found in certain anthropoid apes substances indistinguishable from A and B in man.<sup>21</sup> Unless the evidence to the contrary were strong, the most obvious conclusion from this would be that our anthropoid ancestors possessed the substances A and B, and that the human stock gets them from this common source. What is the evidence to the contrary? Two facts: the distribution of B, which appears to radiate from a center in northern India, and the fact that all tests done prior to 1933 on American Indians were thought to indicate that the Indians before white contact had all belonged to group O (that is, they possessed neither A nor B). Snyder says:<sup>22</sup>

Certain races such as the American Indians and Filipinos and probably the Australian aborigines must on this basis have been isolated from the rest of mankind before the mutations A and B took place, or before there was any wide distribution of these factors. The low frequencies of these factors in the American Indians and Filipinos seem definitely due to intermingling. The frequency of A in the Australian aborigines suggests a definite invasion.

Now it seems to the authors that the findings of Matson and Schrader can not be accounted for on this basis. It will be recalled that the percentage of A found by them in putative full-bloods (76.5) was definitely higher than that in Indians of the same tribes known to be mixed (50.6). The conclusion can hardly be escaped that these Indians, before the advent of the white man, already possessed a remarkably high percent of A. If in the light of this, we reexamine some of the earlier work on Indians, we find that several workers found a percent of A considerably higher than that which should have been found (proportional to the amount of B found) if the presence of A were due to white mixture. In most Europeans

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<sup>20</sup> A. L. Kroeber, Blood group classification (*American Journal of Physical Anthropology*, Vol. 18, p. 377, Philadelphia, 1934).

<sup>21</sup> E. von Dungern and L. Hirschfeld, Ueber gruppenspezifische Strukturen des Blutes (*Zeitschrift für Immunitätsforschung und experimentale Therapie*, Vol. 8, pp. 541, 547, Jena, 1911); K. Landsteiner and C. P. Miller, Serological studies on the blood of primates (*Journal of Experimental Medicine*, Vol. 42, p. 853, Baltimore, 1925), H. Weinert, Neue Blutgruppenuntersuchungen an Affen im Jahre 1932 (*Zeitschrift für Rassenphysiologie*, Vol. 6, p. 75, Munich, 1933).

<sup>22</sup> Snyder, Blood Grouping in Relation to Clinical and Legal Medicine, p. 134.

the proportion of A to B is about 3 or 4 to 1.<sup>23</sup> The ratio from the results of Coca and Deibert on Indians<sup>24</sup> would be 9.6, and from the results of Nigg<sup>25</sup> about 95. From the results of Downs, Jones and Koerber<sup>26</sup> on supposedly pure Indians (only 120 however) a proportion of about 19 would be obtained. Gates<sup>27</sup> found in Indians in Canada 15.5 percent A, but *no* B, and in later work in British Columbia a ratio of A to B of about 21.<sup>28</sup> Similarly F. W. Allen and J. Korber have found in 500 Navajo school children 30 percent A, but *no* B.<sup>29</sup> It would seem that these results indicate the possibility of a source of A other than the white man.

A further remarkable fact difficult to account for on the above theory is the finding by Rahm<sup>30</sup> in the Yahgans of Tierra del Fuego of 91 percent group B (only 33 tested, but according to Rahm the Yahgans now total only 73). Such a high percent of B might be the result of the multiplication of the offspring of one or of a few families—a hypothesis to be considered for North America also<sup>31</sup>—but if the B is supposed to have come from non-Indian sources, the presence of so high a percent ought to imply the presence of high frequencies of other non-Indian genes, which ought to be reflected in physical appearance. Golden<sup>32</sup> found 51 percent B in 61 Caraja Indians in Brazil.

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<sup>23</sup> Of course precise calculations must be based not on the proportion of group A to group B, but on the gene frequencies. Interestingly enough, these show that if a population all in group O are mixed with a European population, the proportion of group A to group B, calculated as above, is *less* for the new mixed population than it was for the Europeans.

<sup>24</sup> A. F. Coca and O. Deibert, A study of the occurrence of the blood groups among the American Indians (Journal of Immunology, Vol. 8, p. 487, Baltimore, 1923).

<sup>25</sup> C. Nigg, A study of the blood groups among American Indians (Journal of Immunology, Vol. 11, p. 319, Baltimore, 1926).

<sup>26</sup> C. M. Downs, H. P. Jones and K. Koerber, Incidence and properties of isohemolysins (Journal of Infectious Diseases, Vol. 44, p. 412, Chicago, 1929).

<sup>27</sup> R. R. Gates, Blood groups of Canadian Indians and Eskimos (American Journal of Physical Anthropology, Vol. 12, p. 475, Philadelphia, 1929).

<sup>28</sup> Gates and Darby, Blood groups and physiognomy of British Columbia coastal Indians.

<sup>29</sup> F. W. Allen and J. Korber (University of New Mexico), personal communication.

<sup>30</sup> G. Rahm, Die Blutgruppen der Araukaner (Mapuches) und der Feuerländer (Forschungen und Fortschritte, Vol. 7, p. 310, 1931; cited by Lattes); Los grupos sanguíneos de los Araucanos (Mapuches) y de los Fueguinos (Investigación y Progreso, Vol. 5, p. 160, 1931; cited by Gates and Darby).

<sup>31</sup> As a simple calculation will show, it would be *possible* for the offspring of a single family to multiply to numbers equivalent to the number of Indians supposedly living in North America in 1492, in a period much shorter than that now supposed to have elapsed since the advent of man in the New World.

<sup>32</sup> G. Golden, Distribution of blood groups in South American Indians (Lancet, Vol. 219, II, p. 278, London, 1930).

It therefore seems that in spite of the undoubted fact that a majority of North American Indians were group O, the other facts brought out above do not justify us in supposing that A originated in Eurasia alone after man came to America. This statement in regard to A probably applies to B also. Furthermore much more data are needed on South American Indians.

Because of the accumulating evidence derived from the finding of human remains and artefacts associated in undisturbed strata with the bones of animals supposed to have been extinct since the Pleistocene, anthropologists are now more inclined to admit the probability of interglacial man in the New World.<sup>33</sup> It is the opinion of some anthropologists that these first and very early migrations consisted of relatively small numbers, as compared with the post-glacial migrations. Is it not possible that the A and B factors came to America with these earlier migrants, thus accounting for their present somewhat scattered distribution in small groups? The remote Yaghans, with their high percentage of B, might be the descendants of one of these small groups (providing Rahm's results meet the test of confirmation). Their position at the tip of the South American continent makes this not improbable. The high percentage of O found generally may then have been derived from groups migrating later.

As the result of our rejection of the most widely held theory to systematize the facts, are we then left with a chaos of strange or meaningless observations? It does not seem so to the authors. Either the blood groups are older than we thought, perhaps having always coexisted with man, or we must examine seriously the theory of independent mutation. According to this view the substances A and B might have originated independently in the anthropoid apes and in man after these stocks were already differentiated, and possibly several times since in man in different parts of the world. Let us first consider the former idea.

If man has always had the blood groups, how could we explain the world distribution? Bernstein<sup>34</sup> postulated three primitive "races," of groups O, A, and B. How these "races" were established is surely a difficult question. It is not impossible that in speculating about it we should go back to the earliest days of man, when his total numbers in the world were very small. If there were such a time, it is not hard to think that small

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<sup>33</sup> M. R. Harrington, Gypsum Cave (Southwest Museum Papers, No. 8, p. 172 ff., Los Angeles, 1933); A. E. Jenks, Minnesota Pleistocene Homo, an interim communication (Proceedings, National Academy of Sciences, Vol. 19, No. 1, Washington, 1933)

<sup>34</sup> F. Bernstein, Zusammenfassende Betrachtungen über die erblichen Blutstrukturen des Menschen (Zeitschrift für induktive Abstammungs- und Vererbungslehre, Vol. 37, p. 237, Leipzig, 1925).

groups, families perhaps, became isolated, and later multiplied to a considerable degree. If the first men had both A and B (and, like the anthropoids, O also), it would be possible that as a matter of pure chance these original families were of different blood groups, and gave rise to certain primitive stocks, each belonging chiefly to one or another of the groups (such a situation could also conceivably result from early mutations independent from the anthropoids).

The maps might suggest that certain early humans having A, and perhaps also O, were driven out of the original homeland, and into the corners of the earth ("refuge areas"). Then the high percentage of A in Australia,<sup>35</sup> among certain Lapps, in parts of Greenland, and perhaps even in the Blood and Blackfoot, might be the results of this original dispersion of the A possessors by the superior strength, skill or numbers of the B possessors. This is similar to Taylor's<sup>36</sup> concept of "zones and strata." Gates and Darby<sup>37</sup> have apparently entertained a similar possibility. Thus the blood groups, and *to a certain extent their geographical distribution* would probably antedate the differentiation of the present races.<sup>38</sup> This seems not improbable in view of the fact that peoples apparently not having most of the other commonly accepted physical criteria in common may have closely similar blood group frequencies. Thus the African tribe in the South Belgian Congo tested by Brynogie and Walravens showed a blood group distribution not extremely different from that found by Barsky in Russians of Kazan (cited by Lattes).<sup>39</sup> Even more remarkable are the similarities in blood group distribution between Greenlanders and Australians, and between the Chinese of Canton and the Congo tribes referred to above. (See Table 3, based partly on Wellisch.<sup>40</sup>)

<sup>35</sup> J. B. Cleland, Further results in blood grouping Central Australian Aborigines (Australian Journal of Experimental Biology and Medical Science, Vol. 7, p. 79, Adelaide, 1930).

<sup>36</sup> G. Taylor, Racial migration zones and their significance (Human Biology, Vol. 2, p. 34, Baltimore, 1930).

<sup>37</sup> Gates and Darby, Blood groups and physiognomy of British Columbia coastal Indians.

<sup>38</sup> When here and hereinafter the much mooted term "race" is used the authors intend it to be understood in the sense defined by Hooton. "a race is a great division of mankind, the members of which, though individually varying, are characterized as a group by a certain combination of morphological and metrical features, principally non-adaptive, which have been derived from their common descent" (E. A. Hooton, Up from the Ape [New York], 1931).

Prof. Verzář said in 1928 of the blood groups that they "weit vor die Differenzierung der heutigen Menschenrassen reichen" (Ukrainisches Zentralblatt für Blutgruppenforschung, Vol. 2, No. 1, p. 25, Kharkov, 1928).

<sup>39</sup> Lattes, The Individuality of the Blood.

<sup>40</sup> S. Wellisch, Betrachtungen über erbbiologische Begriffe (Zeitschrift für Rassenphysiologie, Vol. 5, p. 91, Munich, 1932).

TABLE 3. SIMILAR BLOOD GROUP DISTRIBUTIONS  
IN DIFFERENT PEOPLES

<i>People</i>	<i>Percentage of group</i>				<i>Percentage of gene</i>		
	O	A	B	AB	r	p	q
Greenlanders	54.7	38.5	4.8	2.0	0.740	0.227	0.033
Australians	55.9	38.2	4.6	1.3	.748	.222	.030
Cantonese	45.9	22.8	25.2	6.1	.678	.154	.168
Katangas (S. Belgian Congo)	45.6	22.2	24.2	8.0	.675	.156	.169
Russians (Kazan)	41.9	27.3	23.3	7.5	.648	.194	.169

The present authors would attempt to explain such similarities by means of the hypothetical early dispersions referred to above. They would assume that the present races differentiated, in various parts of the world, largely after this dispersion. If the assumption is made, as above, that these blood grouping genes dispersed from a center, it may readily be imagined that a stock destined to become Eskimos, and another destined to become Australians, might have received similar amounts of A and B, brought to these distant regions from the common center. Such an idea would account for the great difficulties which have been met by authors who tried to designate certain "types" of blood group distribution in such a way that each type contained only related peoples.<sup>41</sup> Likewise the view taken by Young,<sup>42</sup> based on his statistical study seems too pessimistic. Also, it may be imagined that most of the present races differentiated from stocks not possessing exactly the same amounts of A, B, and O, thus accounting for the fact that as a rule races not closely related do not show very similar blood group distributions, and races closely related do.<sup>43</sup> In this sense, then the blood groups may be older than the present races. Those not wishing to assume this antiquity of the blood groups might prefer to explain such puzzling distributions by assuming later independent mutations, as Kroeber evidently does. Let us then examine this latter hypothesis.

<sup>41</sup> W. W. Howells, *Anthropometry and blood types in Fiji and the Solomon Islands* (Anthropological Papers, American Museum of Natural History, Vol. 33, Pt. 4, p. 283, New York, 1933), p. 324.

<sup>42</sup> M. Young, *The problem of the racial significance of the blood groups* (Man, Vol. 28, Nos. 116, 127, London, 1928).

<sup>43</sup> Cf. Snyder, *The "laws" of serologic race-classification*.

The theory of independent mutation can of course account for any distribution of blood groups whatever, if enough mutations are assumed. Before considering the hypothesis, it would be well to know how often a given mutation will occur. Unfortunately, this is a very difficult point to establish. In an article by Hanson<sup>44</sup> figures are given showing that the most frequently observed mutation in *Drosophila* occurs about once in  $10^4$  life cycles, the majority, however, occur about once in  $10^5$  life cycles. It is there stated that Haldane has calculated that in man the gene for hemophilia mutates about once in  $10^5$  life cycles (or, a mutation frequency of  $10^{-5}$ ). So far as the authors know, no other information exists which would aid us in estimating the frequency with which the blood group genes could mutate. If the frequency is similar, we might expect that such a mutation would occur about once in 100,000 life cycles.

R. A. Fisher has treated the whole subject of the contribution of mutations to evolution mathematically.<sup>45</sup> He shows that while a mutation which gives its possessor a certain advantage in natural selection has a finite chance of establishing itself after one occurrence, the chances are that a mutation which has no such selective value, occurring in a single individual only, will die out after comparatively few generations, and that ultimately it is sure to do so. The probability of the survival of such a mutation after different numbers of generations is given in Table 4. Note that the probability is zero at the limit.

TABLE 4 PROBABILITY OF THE SURVIVAL OF A GENE OCCURRING ONCE AS A MUTATION IN A SINGLE INDIVIDUAL, AFTER VARYING NUMBERS OF GENERATIONS (Condensed from Fisher)

<i>Number of Generations</i>	<i>Probability of survival</i>
1	0.6321
3	.3741
15	.1127
127	.0153
limit	.0000

Fischer has also developed a formula, quoted by Gates and Darby,<sup>46</sup> for calculating the spread of a character, similarly with no selective value which mutates repeatedly. This formula is

<sup>44</sup> F. B. Hanson, *Radiation-genetics* (Physiological Reviews, Vol. 13, p. 466, Baltimore, 1933)

<sup>45</sup> R. A. Fisher, *The Genetical Theory of Natural Selection* (Oxford), 1930.

<sup>46</sup> Gates and Darby, *Blood groups and physiognomy of British Columbia coastal Indians*.



$$q = e^{-n\mu}$$

where  $q$  = the proportion of unmutated genes, so that the proportion of mutated genes  $p = 1 - q$ , and  $n$  = the number of generations elapsing,  $\mu$  = the mutation frequency (the fraction of the total number of normal genes mutating each generation), and  $e$ , as usual is the base of the natural system of logarithms. From this Gates calculates that for the mutation to reach a value of about 10 percent, assuming a mutation frequency of  $10^{-5}$ , would require about 250,000 years.

The frequencies of the genes for A and B often surpass 25 percent in Asia at the present time. We may similarly calculate that this would have required some 745,000 years.

From the above it seems clear that the hypothesis of mutation, in order to explain the observed blood group distributions in Asia and America, must be completely discarded, unless later work may demonstrate some selective value for the blood groups, or for factors linked with them. The chances seem enormously against not only the spread, but even the survival of a single mutation occurring long ago. Furthermore there is definitely not enough time for a single mutation occurring in Asia since the arrival of man in America to have spread. To account for an assumed building up of the observed frequencies of A and B in Asia since the more important migrations of man to America (for the sake of liberality, let us say arbitrarily 50,000 years) would require us to postulate a mutation rate of rather more than  $10^{-4}$ . (About  $1.4 \times 10^{-4}$ . This means that in a population of 1,000,000 there should be on the average about 5 or 6 mutations to A and likewise to B each year). Even if we are willing to assume this, there seems no assignable reason why mutation at a similar rate could not have gone on in man in America during the same time. The observed frequencies in Asia could be accounted for by the assumption of a mutation rate of  $10^{-5}$  and the not too improbable time of some 745,000 years. But on this basis the factors A and B would already have been practically as frequent as at present in Asia when man left for America.

Since this was written, Dr Fisher has stated in a letter to one of the authors: "I was thinking of the blood groups in emphasizing that a gene would not be found disseminated among many millions of people without the positive aid of selection, if it had arisen within ten thousand generations or so in only a single mutation, as I think the first speculations about the ethnographic distribution of the blood groups were inclined to assume. If, moreover, not a single mutation, but a definite rate of mutation is postulated the question arises why the mutation rate should be different in different races."

Rubashkin<sup>47</sup> suggested the possible existence of two types of A, which he designated as A<sup>w</sup> (western) and A<sup>e</sup> (ost=eastern), of which A<sup>e</sup> would be found in Australia, etc. He noted that of course no method existed for detecting these differences. Wellisch<sup>48</sup> proposed to identify these with the subgroups of A, that is A<sub>1</sub> and A<sub>2</sub> respectively, suggesting that the ratio A<sub>1</sub>/A<sub>2</sub> would decrease from Greenland to Australia. Table 5, which gives what is known about the distribution of the subgroups of A, lends no special support to this suggestion.

TABLE 5. DISTRIBUTION OF THE SUBGROUPS OF A  
IN DIFFERENT PEOPLES

Author	People	Percent group						
		O	A <sub>1</sub>	A <sub>2</sub>	B	A <sub>1</sub> B	A <sub>2</sub> B	A <sub>2</sub> /A <sub>1</sub>
Wiener and Rothberg*	White Americans	44.6	25.6	12.0	13.6	3.1	1.2	0.48
Nigg†	Hawaiians	36.5	60.8	0	2.2	0.5	0	0.00
Various‡	Danes	43	30	13	11	2	1	0.43

\* A. S. Wiener and S. Rothberg, *Heredity of the Subgroups of Group A and Group AB*.

† C. Nigg, *A Study of the Blood-Group Distribution among Polynesians* (*Journal of Immunology*, Vol. 19, p. 93, Baltimore, 1930).

‡ Calculated from the data of K. Sand, W. Munck and T. G. Knudtson, *Blutgruppenbestimmung in Paternitätsachen die ersten 500 Sachen des Institutes* (*Deutsche Zeitschrift für die gesamte gerichtliche Medizin*, Vol. 15, p. 535, Berlin, 1930), of Thomsen (*op. cit.*), and G. H. M. Waaler, *To nye blodtyper* (*Norsk magasin for lægevidenskaben*, Vol. 91, p. 516, Oslo, 1930).

#### B. THE M AND N GROUPS OF LANDSTEINER AND LEVINE

The M and N groups have not yet been studied enough to enable us to say what their anthropological importance will be, but it has been shown that their distribution varies somewhat in different peoples. This is shown in Table 6, taken chiefly from Wellisch.<sup>49</sup> Since these groups are independent of the Landsteiner groups, a knowledge of their world distribution might provide a valuable check on conclusions based on studies of the older groups. (The Landsteiner groups are older in the sense that we have known of them longer: we are not able thus far to make any guess as to which are older phylogenetically.)

<sup>47</sup> V. Rubashkin, *Графічне відображення серологічних рас* (*Ukrainisches Zentralblatt für Blutgruppenforschung*, Vol. 2, No. 4, p. 36, Kharkov, 1928).

<sup>48</sup> S. Wellisch, *Das vorhandene Untersuchungsmaterial im MN-System* (*Zeitschrift für Rassenphysiologie*, Vol. 6, p. 66, Munich, 1933).

TABLE 6. DISTRIBUTION OF THE M AND N GROUPS  
AMONG DIFFERENT PEOPLES

<i>Author</i>	<i>People</i>	<i>Group percent</i>		
		M	MN	N
Landsteiner and Levine	White Americans	26.1	54.8	19.1
Landsteiner and Levine	American Negroes	27.6	44.3	28.1
Landsteiner and Levine	American Indians	60.0	35.1	4.9
Wiener and Vais- berg	White Americans	30.6	47.6	21.8
Schiff	Germans	29.8	49.5	20.7
Schockaert	Belgians	39.0	33.8	27.2
Dujarric and Kos- sovitch	French	33.0	45.8	21.2
Shigeno	Japanese	30.2	46.0	23.8
Thomsen and Clausen	Danes	30.0	44.5	25.5
Elovuori	Finns	23.4	59.5	17.1
Lattes and Gar- rasi	Italians	27.2	57.4	15.3

To summarize our examination of the conclusions which have been based on previous work, we may say: the blood groups seem to be older than the present races, which would account for the disappointment which has been felt at finding apparently unrelated peoples with practically identical blood group distributions (cf. table 3). It seems unlikely, therefore, that man came to America before the origin of the blood groups. An examination of the idea that recent mutations can account for the observed striking differences in blood group distribution in Asia and in America leads us to discard it. A somewhat different idea to account for the predominance of group O among North American Indians is suggested above, and attention is called to results which cast doubt on the idea that pure bloods must all be of group O. No decision is reached on the question of whether the blood groups have been derived by man from his anthropoid

ancestors, or have arisen by independent mutations, but the authors favor the former point of view. Much more work is needed (including determinations of M and N and the sub-groups of A), especially on all groups of people giving evidence of long isolation and all of the rapidly vanishing primitive tribes.

In the light of more recent data it would seem that Snyder's "four laws of serologic race-classification" require some modification.<sup>49</sup>

Finally, it may be said that if, as concluded here, the blood groups are older than the present races, we should not regard this as a discouraging fact, but rather as an encouraging one. The groups may still have considerable anthropological value, presumably as much as any other one criterion, and by study of them we may possibly hope to get some information about human migrations preceding the differentiation of the present races.

#### C. BLOOD GROUPS AND ARCHAEOLOGY

Blood grouping has suffered from one disadvantage which did not affect certain other criteria, such as skeletal characteristics, namely that blood groups could be determined only on living persons. It seemed impossible ever to know what the earlier blood group distribution of a people had been. Nevertheless, if thought is given to the matter, it is seen not to be completely hopeless.

The chemical substances A and B are remarkable among antigens for their stability. Heat (100°), drying, acids, mercuric chloride, formaldehyde—none of these destroy them.<sup>50</sup> Blood stains have been grouped after having been dried for years.<sup>51</sup> Further, these substances A and B occur in practically all parts of the body, not only in the blood. Thus blood groups can be determined from the dried saliva left on the butt of a smoked cigarette.<sup>52</sup> The flesh of corpses has been shown to contain the substances.<sup>53</sup> These and similar considerations have suggested an attempt to determine the blood groups of ancient, mummified human remains. Such work is now

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<sup>49</sup> Snyder, The "laws" of serologic race-classification.

<sup>50</sup> L. Lattes, Le diagnostic individuel des taches de sang (Rapports présentés au VIII<sup>e</sup> Congrès de Médecine légale de langue française, Paris, 24, 25, 26 mai, 1923).

<sup>51</sup> Hooker and Boyd, *op. cit.*

<sup>52</sup> L. Lattes, Contribution au diagnostic de l'individu par l'examen des traces de salive (Bollettino della Sezione Italiana della Società Internazionale di Microbiologia, Vol. 4, p. 585, Milan, 1932); Hooker and Boyd, *op. cit.*

<sup>53</sup> I. L. Kritschewski and L. A. Schwarzmann, Die gruppenspezifische Differenzierung der menschlichen Organe (Klinische Wochenschrift, Vol. 6, p. 2081, Berlin, 1927).

being carried out, and preliminary reports have already appeared.<sup>54</sup> As the work goes on, it seems more and more encouraging, and a full report will appear soon. In the meantime a report of independent work leading to similar results has been published.<sup>55</sup>

The exact technic used in this work is described in the papers referred to. Only a brief outline will be given here. The method depends upon the fact that the substances A and B, in dried muscle (or other dried human material), though they can not be agglutinated, nevertheless still combine specifically and firmly with the agglutinins anti-A and anti-B. This is true of dried blood also, and the method we use for mummified tissue is not very different from the method previously used by others for blood stains. A mixture of the agglutinins anti-A and anti-B is placed on a small sample of the finely ground tissue. Later the mixture is tested with known A and B red cells to see if both agglutinins are still present. If the A cells, for instance, are not agglutinated by the mixture, it follows that the anti-A agglutinin has been taken out by the mummified tissue, and that in order to do this the tissue must have contained the substance A. Of course careful controls and checks are necessary.

About 200 mummies, mostly American, have now been tested by this method. The results will appear in another paper; but it may be stated here that A and B are believed to have been demonstrated in prehistoric Indian remains, and that B seems to be present in a prehistoric Egyptian dating some centuries before Mena.

If this work is successful, it seems that light may be thrown on other problems, such as checking the identity of certain Egyptian mummies. In case of a dispute as to the lineage of a supposed royal mummy, if the bodies of the putative parents were also available an application of the well known nonpaternity tests<sup>56</sup> might conceivably afford valuable data to Egyptologists.

#### SUMMARY

An introduction is given to the study of the blood groups, including a description of a method of grouping suitable for work in the field. The data

<sup>54</sup> W. C. Boyd and L. G. Boyd, Blood grouping by means of preserved muscle (Science, Vol. 78, p. 578, New York, 1933); An attempt to determine the blood groups of mummies (Proceedings, Society for Experimental Biology and Medicine, Vol. 31, p. 671, New York, 1934), Group specificity of dried muscle and saliva (Journal of Immunology, Vol. 26, p. 489, Baltimore, 1934).

<sup>55</sup> G. A. Matson, A procedure for determining distribution of blood groups in mummies (Proceedings, Society for Experimental Biology and Medicine, Vol. 31, p. 964, New York, 1934).

<sup>56</sup> Hooker and Boyd, *op. cit.*

bearing on the world distribution of the blood groups are summarized and their possible interpretations, including those offered by earlier authors, are discussed. It is concluded that the blood groups are probably older than the present races, but whether of anthropoid or independent origin it is difficult to say. A method by which it may be possible to determine the blood groups of mummies is described briefly.

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SIOUAN TRIBES OF THE CAROLINAS  
AS KNOWN FROM CATAWBA, TUTELO,  
AND DOCUMENTARY SOURCES<sup>1</sup>

B. FRANK G. SPECK

THE hope entertained since 1893 among students of native history and institutions, that the confusion of tribal names mentioned in the early narratives of the Carolinas would sooner or later be cleared up has not as yet been realized. Nor are the prospects very favorable, now that the last remaining persons speaking any of the Siouan languages of the Southeast have dwindled to the number of two of the Catawba. As Mooney points out in summarizing the results of his study of Siouan tribes of the east, the actual identity of only the Tutelo and Catawba languages can be ascertained with certainty, whereas twenty-two other of his Siouan classifications were so determined only through the inference of their political relations with the Catawba. Later Swanton described grounds upon which Woccon, of the extreme eastern North Carolina region, could be linked by lexicon with the Catawba as a Siouan idiom, and subsequently I was encouraged by him to suggest a similar solution for the identity of Duhare, spoken about Winyaw Bay, south of Cape Fear River. Mooney (1893), following Hale (1870) and Gatschet (1881), concluded that historical sources were sufficient to remove doubt as to the Tutelo relationship of Saponi and Occaneechi, finally reducing the totally unattached languages of the Carolinas to some fifteen. No lexical terms from these latter are known to exist for purposes of classification except for three or four chief's names in Cheraw and Santee, and the river and village names from which the tribes themselves had derived their proper names.

No adequate linguistic sources other than Catawba now exist to which we can turn for explanations of these perplexing proper names. Perchance the Tutelo, known as a speakable language until about 1890, had it been more intensively recorded than it was by Hale whose Tutelo vocabulary contains less than 200 words,<sup>2</sup> might have afforded sufficient etymological evidence to have settled the question for some of the unexplainable proper

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<sup>1</sup> Published with the permission of the Bureau of American Ethnology and the American Council of Learned Societies (Committee on Research in American Indian Languages), organizations that contributed to the accomplishment of field work in the Carolinas, the partial results of which are incorporated in this article (correspondence F. Boas, Feb. 18, 1933 and M. W. Stirling, Feb. 21, 1933). Acknowledgement is also due to the Faculty Research Fund of the University of Pennsylvania, Grants No. 50 and 93, for the use of resources in the field which resulted in making additions to the material from native sources in Canada.

<sup>2</sup> Horatio Hale, *The Tutelo Tribe and Language*.

names of North Carolina. This chance passed by. Thus Catawba remained as the last and only source.

With this whole perplexing situation constantly in mind I have grasped every opportunity, while studying Catawba under funds provided by the Bureau of American Ethnology and the American Council of Learned Societies, to learn from the last speakers of the language what they could tell of the few tribal names applied to surrounding peoples. And at the same time I have tested the possibilities of explanation of proper names of the extinct tribes of the Carolinas through their knowledge of the Catawba language in the hope that tribal synonyms might help clear up some of the gaps in our knowledge by proving out meanings for some of the native names of surrounding tribes, rivers, and villages. The success of this attempt has not been phenomenal, yet some contributions to the terminology of the region have inevitably resulted from the memory-dredging process on the part of the four living speakers of Catawba with whom I have worked these past years.<sup>3</sup> These results are now brought together from the notes and presented with certain of my own speculations on the Siouan lineages of the east, inasmuch as there is unfortunately no prospect that henceforth any additional first hand material can be elicited from the Catawba informants.

A question that has engaged attention for some time is that of the former wider distribution of the eastern Siouan peoples farther to the eastward and northward of where they were found by the first explorers. I have still to find convincing proof that the Algonkian populations did not displace some of these Siouan kindreds in their expansion southward and into the Alleghenian region.

The deep-rooted hostility that prevailed between the Powhatan and the Monacan and Manahoac may be attributed to intrusion of the one upon the territory of the other in later times. And in this case it would seem to have been due to the aggression of the Powhatan, supposedly the later arrivals in the Virginia lowlands.<sup>4</sup>

Such an attitude toward cultural history here could be held to account for the allocation of the Powhatan peoples in the lower country to the eastward and the Siouan peoples in the piedmont region, their hostility

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<sup>3</sup> Mrs Samson Owl, Margaret Brown and her son and daughter, Sam Blue and Sally Brown.

<sup>4</sup> In a previous article I gave reasons for my inference regarding the relatively recent migration of the Powhatan peoples into the Virginia tidewater region (*Ethnic Position of the Southeastern Algonkian*). Also Birket-Smith. In a recent study Bushnell also proposes a similar movement (Bushnell, 1934).



toward each other, the survival of the language of the Occaneechi as a trade language of the region and as the language of religious ritual, which facts we learn from Strachey.<sup>5</sup> If accordingly, my inferences for a more easterly habitat of certain Siouan peoples, the Shoccoree and possibly the Eno, are accepted, then we have a trend of evidence hinting at the conclusion that Siouan peoples were earlier residents in eastern Virginia and Carolina and were invaded several centuries before the coming of the Europeans by the Powhatan, and gradually dispossessed of their territories by them.

THE SIOUAN TRIBES OF THE CAROLINAS AND THE MEANINGS OF  
THEIR NAMES IN CATAWBA

The deep mystery surrounding the identity of the small extinct tribes to the east and south of the Catawba receives but little light from an inquiry into tradition among the survivors of the tribe. That many of these peoples on the lower courses of the rivers flowing from the Appalachians into the Atlantic were of southeastern Siouan affinity has been substantially shown by Mooney. And nothing has developed through later inquiry into the question to weaken this supposition. The tribal names and local designations characteristic of the area in question would fall well within the phonetic range that we find in the Catawba language. It is indeed even possible for renderings to be given to many of them in Catawba, yet we can not feel certain that interpretations for the proper names are to be relied upon when the Catawba themselves do not know until we mention them that peoples under these designations ever existed. In view, however, of the avidity with which any possible clue to the perplexing aboriginal history is sought in our process of investigation, I shall offer the results of discussion with the Catawba-speaking informants apropos of the tribal names of the long-extinct groups so painstakingly worked over from various sources by Mooney.

*Catawba.* The proper name Catawba has been the subject of speculation as respects its origin and meaning. No definite source in neighboring Indian languages can be traced to the satisfaction of critical judgement. Both Swanton and Mooney considered the possibility of Catawba (Kata'pa) and Kituhwa (variations Katuha, Kuttawa, etc.) being related forms. And while the latter term is an accepted proper name for the Cherokee in a number of instances, it appears as a synonym for the Catawba as well in some southern tongues, such as Shawnee and evidently Delaware. The interchange of *b*, *p*, and *h*, *w* in the others need cause little embarrassment

<sup>5</sup> Strachey, p. 161. See also Hale, *op. cit.*, p. 12.

for they are phonetically interchangeable. Ostensibly the proper name is an old one referring to the populations of the Carolina mountain and foot-hill region, without specifying the particular tribe or idiom of its bearers as being Catawba, Cherokee, or mixed groups of the two.

My own suggestion regarding the analysis of the mysterious proper name is a brief one. We may begin by taking the form of the name as given by the speakers of Catawba themselves. This form is  $y\epsilon\ k\alpha t'ha'p\alpha$ , "Catawba people." This form is known to the informants without any further consciousness of its literal meaning. Yet in experimenting with various possibilities with a view to its analysis, the following discovery was made. If we accordingly regard the term as a sentence in construction we may divide the elements into syllables with a fairly definite meaning.  $Y\epsilon\ k\alpha$  may be rendered "people the present, or now," the element  $-t'$  would be a subjective case form and  $ha'p\alpha$  "on the edge of or bank of," referring to a river. Hence the construction could and does express the idea of "the People upon the Edge or Bank of River." This possibility has some degree of likelihood in the fact that the Catawba have always been designated as located in proximity to the river that bears their name. The historical proper names of the tribe have been, from the first (Lederer), variations of the term  $i'sw\alpha$ , "river (people)," as Esaw, Issa, Ushery and the like, all too well reviewed in the accounts to need listing here.

In the Catawba language the term  $y\epsilon\ i'sw\alpha$  (here), "People of the River," is also applied to the nation. This is probably the form of the proper name from which the common names in early records were derived, as for instance those just given. And we might go further in constructive speculation concerning all these forms. By combining the elements into a perfectly good idiomatic sentence we have  $y\epsilon\ i'sw\alpha\ k\alpha tha'p\alpha$  which would mean, depending upon the arbitrary literal values of the stems, "People of the River (Catawba) Banks," "People of the River (Catawba) Broken Banks."<sup>6</sup> In connection with this name it should not be forgotten that the

<sup>6</sup> Swanton's suggestion of explanation is also worth considering here (correspondence May 24, 1933):

"Please consider the following which I find among our Catawba [vocabulary] cards:  $ya'p\ k\alpha\ t\alpha pa$ , a tree fork. It also appears that  $k\alpha t$  is the stem of a verb meaning "to break." As the Catawba appear in Lawson's narrative as one of two bands of the tribe of about equal strength, the other being called Esaws, probably from the river, it has occurred to me that the word may be taken from this native term indicating a separate part of the main Catawba nation.

"However, if it is identical with Kituhwa, it must be fairly old. Mooney says this last was originally that of an important settlement on Tuckasegee River. Is it possible that the Catawba were once settled there? And if they were, would not this be an excellent hint for archeological investigation?"

Wateree river, which is nothing but a section of the Catawba lower down, traces its name to a Catawba word meaning "Washed Away Banks" (watəɾə'həre).

*Saponi and Tutelo.* The identification of these two tribes in the historic period with the Monahassanugh and Monasickapanough (Smith, 1607), divisions of the Monacan group, as residents in the Virginia foothills has been undertaken by Mooney and concluded by Bushnell.<sup>7</sup> Their exodus from Virginia, their wanderings southward and then their return to Virginia to settle for a while at Fort Christianna<sup>8</sup> have been succinctly traced by Mooney. As yet, however, we have no mention of their association with the Catawba as allies or as incorporated units. Nevertheless there must have been at one time an association between the northern (Tutelo and associated peoples) and the southern (Catawba, Woccon, and others) divisions of the Siouan tribes of the region.<sup>9</sup> Swanton thinks that the incursion of the Spaniards into the Carolinas in the 16th century resulted in forcing certain of them to the northward.

Catawba tradition is silent in regard to the Tutelo. A single echo of the once important name Saponi possibly comes down to us through Catawba memory in the mention by Margaret Brown of a tribe whose name was remembered as (ye) pa'nə spoken of by her mother. She knew nothing more of the term or its meaning.

Of the proper names denoting the Tutelo (Toteri, Yesang, Nahissan, etc.) there is no hint of cognizance among the Catawba. Treating the village names of the Tutelo and Saponi identified as Monahassanugh and Monasickapanough, from the viewpoint of Catawba stem similarity, the element mona- is valid as the Catawba designation for "land, earth, ground" but this etymology does not apply to forms in the dialects of the northern (Monacan) eastern Siouan area.

Incidental to this brief discussion of Tutelo and Saponi history, I have added a few notes on these tribes secured from informants among

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<sup>7</sup> Mooney, 1894, p. 37; Bushnell, 1919, pp. 13, 17.

<sup>8</sup> Bushnell (*ibid.*, p. 28) locates this fort about ten miles north of Roanoke River, Brunswick Co., Virginia.

<sup>9</sup> The definition of the northern and southern division of eastern Siouan languages was announced first by Swanton, 1928, pp. 34-35. The extant linguistic sources for Tutelo are Horatio Hale, *The Tutelo Tribe and Language*; L. J. Frachtenberg, *Contributions to a Tutelo Vocabulary*; E. Sapir, *A Tutelo Vocabulary*.

A notice of some importance in connection with the Tutelo language is given by Gatschet (*A Grammatic Sketch of the Catawba Language*, p. 52) stating that J. O. Dorsey had found in an old document in Washington a mention that Tutelo and Winnebago could understand each other's language at a treaty taking place at Prophet's Town, Indiana, in 1809.

the Six Nations at Grand River, Ontario, since here these observations will meet the eyes of students interested in them.

The words of Mooney applying to the Saponi seem to be the last that can be said of the tribe. One clue concerning its disappearance from history, in solving the identity, is a family name and family group named Boni', domiciled among the Mohawk of the Bay of Quinté, Ontario. At the Six Nations this information was furnished; the Boni' family, under the name Boni' ha'ga, "Boni' people," was understood to have come from the United States about 1800. The family is classified as being of mixed Mohawk and Cayuga lineage, affiliated with the Mohawk of the Bay of Quinté. If, by remote chance upon deeper inquiry, the tradition of this family should possibly develop an association with the Saponi tribal appellation, Saponi' ha'ga, the question of the fate of the mysterious Siouan tribe from the southeast may be reopened.

Among the Six Nations Iroquois of Ontario a reminiscence of the Tutelo, which has escaped recording by those who have questioned the Iroquois on the subject, was offered me in 1925 by Joe Henry, the oldest Cayuga living at that time. This addition to our knowledge of the Tutelo relates that the name of the last Tutelo chief was Ka'sto'hagu, the term referring to his "Dwelling in Stone."<sup>10</sup> Legend states that he had killed a number of people; that he was the "first Tutelo who came to the Six Nations;" and that he had formerly lived in a cave having a room perpendicular to the entry passage in which recess he lived for protection. The cave was so formed that only one invader at a time could enter and turn the corner. Intrenched in this cavern he had accounted for his enemies. The interesting tale of the old Cayuga is apparently a native version of a tradition recorded in 1733 by Byrd in reference to a cave that he found on an island in Roanoke River (Mecklenburg Co., Virginia) lying above Occaneechi Island, inhabited by the Tutelo before 1701: in which cave the last Tutelo king with only two men had defended himself against a large party of Iroquois and at last forced them to retire.<sup>11</sup> Tradition among the Iroquois at times dies hard!

The only sources now remaining open for the investigation of Tutelo

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<sup>10</sup> We learn (Chadwick, *People of the Long House*, p. 19, and Boyle, *Annual Archaeological Report of Ontario*, p. 55, quoted in article *Tutelo* by Mooney [*Handbook of American Indians*, Bulletin, Bureau Amer. Ethnology, 30, Pt. 2, p. 856]) that John Key, a Tutelo of the Six Nations Reserve, Ontario, one of the last to speak the language, bore the name Gostango, "Below the Rock." He is evidently the person referred to above.

<sup>11</sup> Byrd, *History of the Dividing Line*, etc., Vol. II, pp. 5-8, quoted by Mooney, 1894, p. 38.

customs lie in the traditions of the Cayuga, who were instrumental as sponsors for the adoption of the Tutelo into the League of the Iroquois, and the Onondaga and Seneca with whom the tribe has intermarried in the last century. These sources remain largely untested. The occasion in the winters 1931, '32 '33 for field research among the Six Nations placed an opportunity within reach to carry on questioning and recording of facts in Tutelo traditional history and custom handed down among the Iroquois of the Six Nation's Reserve on Grand River, Ontario. The results are incorporated into this paper. Among the seven families of recognized Tutelo descent among the Onondaga, Seneca, and Cayuga, there are still preserved certain ceremonies of distinct Tutelo origin which, while not conducted through the medium of Tutelo speech, are nevertheless chanted in Tutelo songs. The outlines of one of these ceremonies, the Re-dressing or Adoption Rite, have been recorded and explanations secured of the meanings of the separate ceremonial acts comprising it. Treatment of the Re-dressing ceremony, however, is omitted from this section of Tutelo notes covering the history of the tribe winnowed among the Tutelo descendants. The conduct of its rites, its songs, the equipment required, its symbols, its entire function, are Tutelo in origin and character. They attest the survival of Tutelo culture after the assimilation of the tribe into the body of the Iroquois. This fact is most interesting and significant. It means to the historian and to the ethnologist that Tutelo institutions still remain to be studied after Tutelo blood has been assimilated into the body of the larger Indian nations. I need not add that certain words of the language have been recorded in recent years on the Six Nation's Reserve by at least four investigators. A vocabulary taken down by myself in 1932<sup>12</sup> duplicates the effort and the results. The accompanying notes are selected as being appropriate to include under the title of this paper.<sup>12</sup>

The Cayuga claim to have befriended the Tutelo at the time of their first appearance in the north when the hand of the other tribes was turned against them. The tale to be given shortly, which was narrated by Deskaheh (Alexander J. General), one of the Cayuga chiefs, refers to their first contact with the Tutelo, presumably after the tribe had first left its seats in the Carolinas. The Cayuga designate the Tutelo as *todi'i'ho'nα* or by the shortened form *ti'i'ho'nα*. The Mohawk term applied to the Tutelo is *te'yotoni'ro'nα*. The following narration is given in the words of the informant.

<sup>12</sup> The task was made possible, as forming part of the plan for research in native religious rites and beliefs of tribes dwelling in eastern Canada, through several grants (Nos. 40, 93) allotted to me in 1932 and '33 by the Faculty Research Committee of the University of Pennsylvania, to which source I accordingly express acknowledgment.

## HOW THE TUTELO WERE ADOPTED BY THE CAYUGA

The Tutelo came up from the south. They did not have any settlements and lived in the woods and caves like wild people. They were a very timid people and were afraid of other Indians. The Tutelo scouts who went out to look for the smoke from camp fires (settlements) would transform themselves into mice and travel under the leaves so that they would not be discovered by unfriendly Indians. When they wished to look over the country they would resume their natural form and climb to the tops of trees. The Tutelo scouts were at last seen by the Cayuga who, being a friendly and peaceful tribe, invited them to join their settlement. They accepted and mingled with the Cayuga and learned their language. The Tutelo scouts returned to their people and told them how they had been taken in by the Cayuga. They brought back the other Tutelo and their families to the Cayuga settlement. There they built a camp of logs. When sleeping at night they were arranged like spokes of a wheel, feet to the fire: the children first, then the women, and last, the men to guard the camp. One night the Tutelo overheard the Cayuga talking in council with the Seneca. They could not understand all that was being said, but it sounded to the Tutelo like a plan to eat them. They thought that the Cayuga and Seneca were saying, "The Tutelo are good to eat." It proved to be that the members of the council were talking over the proposed plan for the adoption of the Tutelo.<sup>13</sup>

A reference by Cammerhoff<sup>14</sup> in 1755 alludes to the residence of a detached tribe, neither Iroquoian- nor Algonkian-speaking, on the east branch of the Susquehanna. The reference is deemed worthy of quoting in full as follows.

Here they tell me [referring to Cohontoto] was in early times an Indian town, traces of which are still noticeable, e g., corn pits, etc., inhabited by a distinct nation [neither Aquinoschioni, i.e., Iroquois, nor Delawares] who spoke a peculiar language and were called Tehotitachsae; against these the Five Nations warred and rooted them out. The Cayuga for a time held a number of them, but the nation and their language are now exterminated and extinct.

General John S. Clark,<sup>15</sup> painstaking student of documents referring to former tribes in Pennsylvania, has devoted considerable attention to the identity of the tribes and villages referred to in the passage just quoted. In his correspondence with Professor A. L. Guss relative to the identity of the peoples in question we find that the latter's responses to General Clark are also of a character that throw light on the topic. They are as follows.

<sup>13</sup> By the Indians on the Reserve who use English, the Tutelo are referred to as Tuteli.

<sup>14</sup> J. C. Cammerhoff, *Diary of the Journey of Brother Cammerhoff and David Zeisberger to the Five Nations, 1750*. A special translation is quoted in *Handbook of American Indians*, Part 2, p. 977; also in *Selected Manuscripts of General John S. Clark*, p. 37. Tehotitachsae also mentioned on pp. 3, 7, 8, 38.

<sup>15</sup> See Guss-Clark Correspondence in *Selected Manuscripts of General John S. Clark*, *op. cit.*, pp. 38-39.

The three towns appearing on the Popple map of 1733 must have been those of the same people mentioned at the treaty of 1722 as OSTANGHAES which live upon the Susquehanna river and occupy Ostauwocken as one of their towns, very likely the remnant of the Tehotitachse who retreated via the West Branch where they halted for a time and built the Conestoga fort at the mouth of the Tiatacton and afterwards removed to the vicinity of Lake Erie. The name Tiatacton given by the Moravian missionary Spangenberg in 1745 is precisely the same as the Iroquois name for Pine Creek at the confluence of which was the fortified stronghold where the last stand was made by the Conestogas against the Iroquois, and from which they retired about 1675 Tehotitachse as given by Cammerhoff is another version of the same name.

Otstonwaken was no doubt the town lying on both sides of the mouth of the Loyal Sock (a creek flowing into the West Branch). Otstuagy is only another form of the same name. I have been collecting sentences from books, and names from maps and send you my list. I have a notion that this name originally denoted a Huron-Iroquois nation that lived on the West Branch, called the people of the Demon's Caves because they lived in caves at one time, or more likely because they claimed that they originally came out of caves after the manner of their legends generally. These being exterminated by the Iroquois at an early day, the river and region still had the name clinging to them. And it is probable that a remnant of the old tribe lived at this place as did the Conestogas below the mountains, known as the ancient Susquehannocks or Minquas (See Col. Records, Oct. 11, 1722). This remnant like the Conestogas may have perished and therefore as you once wrote me was overlooked by historians. Otzinachse with terminal variations was applied to the West Branch of the Susquehanna—the same term without the prefix OT, was applied to the river, also to Shamokin or the region of the mouth of the West Branch. All the names on my list with prefix OT are forms having some relation to the name of the river, and were applied to a town on that river and denoted also the people of the region of the Demon's Caves or that originated from the Demon's Caves.<sup>16</sup>

Another correspondent of General Clark's, David Craft,<sup>17</sup> wrote the following in reference to the towns mentioned in the above notes.

In 1745 Zeisberger made his first acquaintance with the County. . . . In 1750 he in company with a Cayuga chieftain paddled up the Susquehanna from Wyoming to Tioga . . . at Mehoopany was an Indian town called ONOCHSAE (below Wyalusing), "opposite to which is a cave." . . . It was at that time nearly abandoned there being only two or three families living there. This was on the right bank of the river and about 60 miles below the State line as the river runs.

Next was Gahontoto, which the Cayuga said had been inhabited by a

<sup>16</sup> A synonymy of twenty-nine equivalents for the term is given in the Clark manuscript, *op. cit.*, p. 39.

<sup>17</sup> Clark, *op. cit.*, pp. 6, 7.

strange tribe of Indians, neither Iroquois nor Delawares, called by the Cayuga Tehotachsae, with whom the Cayuga made war, finally exterminating them. "This was before the Indians had seen rifles when they fought with bows and arrows." Craft places the date at about 1640. In reply to Craft's notes, Clark<sup>18</sup> says, ". . . In your locality I identify Sionassi as another form of Onochsae carrying the idea of a cave. . . . Onondaga Creek in our state was called 'ZINACHSON' from a Demon's Cave."

Upon the occasion of referring these terms to Deskaheh, the Cayuga linguistic informant referred to previously, certain checks upon their interpretation were brought out. The inferences to be made from the etymologies worked over do not coincide in every particular with those offered by Clark and others. It seems apparent that added weight is given to the judgement that the mysterious tribe in question may have been a Siouan speaking unit, to wit, the Tutelo or their affiliates. The repeated reference to the unidentified tribe as "cave dwellers," as "wandering people" or "people without homes;" the concurrence of the proper name of the Tutelo in contemporary Cayuga with the proper names for the mysterious tribe and its villages appearing in the documents, point likewise to establishing identity with the Tutelo. The associations are constant. With this tentative conclusion I leave judgment to the critic.

The explanation of the terms in Cayuga are as follows.<sup>19</sup>

Tehotitachsae-todi'dast, "stopped them"

tehoti'ta'se, "stopped them" (Mohawk)

Gahontoto-kawq do'do, "tree, or post, standing up"

khaudo'do, "small tree standing up"

Otzinachse-o'sana'se, "new name," or "new chief"

The explanation of the term Diad-aklu as being "lost" or "bewildered people," quoted by Clark from the Journal of Conrad Weiser, who passed through the village in 1737, was confirmed by Deskaheh, the Cayuga informant.<sup>20</sup>

Further questioning of John Buck, the present Tutelo chief among the descendants of the tribe in the Six Nations, adds the following valuable facts to our knowledge of this interesting group.<sup>21</sup>

<sup>18</sup> Clark, *op. cit.*, p. 12.

<sup>19</sup> Compiled by Miss G. Tantaquidgeon from the results of questioning Deskaheh in checking over data in her appendix to the Selected Manuscripts of General John S. Clark, *op. cit.*, pp. 138-43.

<sup>20</sup> Clark, *op. cit.*, p. 41.

<sup>21</sup> John Buck has also been consulted by Hewitt and memorialized by him as an informant in several of his reports (Explorations and Field Work of the Smithsonian Institution, 1926,



The Tutelo constituent still maintains the tribal name as an alien element adopted into the Six Nations. The event of adoption is dated 1753 by the records of the Six Nations Council. The political status of the tribe is that of a "prop," or "support between the logs" in the side wall of the Long House of the League of the Iroquois, as the native figure of speech phrases it. This status in the League entitles the adopted tribe to maintain a chief as its representative to sit in the League Council of forty-nine members. The Tutelo chief may speak and act in the Council only in reference to affairs of the Tutelo tribe, not having the privilege of participating in the parleys of the Five Nations in dealing with major affairs of the League. The status of the Tutelo corresponds to that of the Tuscarora and the Nanticoke as "younger brothers" and the Delawares as "nephews." The present chief of the tribe is John Buck, Ga'poga'tadyi' (for which there is no equivalent given in English because the title is a Tutelo term whose meaning has been forgotten). Buck is the descendant of a long line of leading Tutelo persons. He derives his descent in the tribe through his father, contrary to the usual Iroquois custom of tracing descent in the maternal line. He is a life chief holding the name-title as given, handed down from early times. His paternal grandmother was a Tutelo who was born among the Iroquois about 1802. Another Tutelo ancestor who died about sixty years ago was Ku'he, a name like the rest not being translatable. Buck also traces descent from the Tutelo war chief Kasta'-hagu, "Dwells in Stone," mentioned above. This was his paternal grandmother's uncle. Buck is accordingly the political and ceremonial representative or chief of the seven family groups, comprising about sixty individuals, who carry on the Tutelo identity in the League of the Iroquois at their reserve in Ontario.<sup>22</sup>

Still another reference to the Tutelo under their oldest recorded name, Nahissan, comes to us, I believe, in the reference by Bland (1650) to a nation living

above the head of the James River unto the foot of the great mountains, on which River there lived many people upwards being the Occanacheans and the Nes-

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p. 237; *ibid.*, 1929, p. 201) Hewitt refers to the migration legend of the Tutelo from North Carolina leading them to the Cayuga, and his notes are to be looked for with extreme interest.

<sup>22</sup> The family heads and enumeration as given by him, which would be about as correct as it could be estimated now, are as follows. Peter Williams (four children), John Buck (thirteen children); Mrs Sanders (one child), Eliz. Fish (four children); Joe Cranbette (a large family of children); Elisha Williams (four children), Mrs Lucy (Williams) Fishcarrier (eight children) Other Tutelo descendants of mixed lineage might be listed among the Six Nations.

*soneicks* and that where some of the Occanacheans lived, there is an island within the River three days journey about.<sup>23</sup>

This form of the name (Nessoneicks), which I take to be a synonym of Nahissan, with an Algonkian plural ending plus the English plural, appears to have been overlooked by previous authorities.

Another minor point. Under another form of the tribal name, Mohetan, a village of this affinity is indicated on Alvord and Bidgood's map (1673-4), visited by Needham and Arthur, a days journey from the Great Kanawha River, West Virginia.<sup>24</sup> At present we may admit that this reference means an earlier wider extension toward the west in the Alleghenies or a move toward the end of the 17th century in that direction, after which the village may have acquired a name derived from some other tongue.

The association of the Mannahoac with the Monacan brings up another aspect of the problem before us. Both peoples are described as occupants of the piedmont and mountain slopes of Virginia, and they have been regarded as related tribes, by all the authorities who speak about them since Hale and Mooney, both as concerns the characteristics of speech and culture. Since, therefore, we possess a vocabulary from only the one language of the Virginia area, namely Tutelo, it may be profitable to apply the lexical forms of Tutelo to the half dozen terms preserved by Captain Smith as place or tribal names of the Mannahoac confederacy. I would suggest accordingly that the term "Stegarakes," one of those given by Smith as a division of the Mannahoac people, is a derivation from Tutelo *histēk*, *stēk*, "island," in reference to the location of one of its villages. Also that Smith's Whonkentēas might be a corruption of Tutelo *wahtakai*, "Indian," and that Smith's terms Tauxanias, Shackaconias, Ontponeas might contain as terminations (-onea[s]) the Tutelo element *onī* (Hale, p. 34), "at the prairie." Without intending to assume a positive attitude concerning the interpretation of Mannahoac names written in Smith's manuscript three hundred years ago, by a stranger to the Indian tongue, explained through the medium of the small Tutelo vocabulary (spoken by a Siouan tribe about one hundred miles distant from them) of about one hundred fifty words recorded by Hale in 1870, the above suggestions will be accepted merely as such.

*Occaneechi*. The term Occaneechi (with its variants Akenatzzy, Occanacheans, Patshenins) comes down to us as the tribal designation of an early people of the Virginia-Carolina frontier, dwelling (1670) on a large

<sup>23</sup> Bland, *The Discovery of New Brittain*, p. 16.

<sup>24</sup> Map by Lee Bidgood and C. W. Alvord in *The First Trans-Allegheny Explorations by the Virginians, 1650-1674*.

island in Roanoke River just below the confluence of the Staunton and the Dan Rivers, near the site of Clarksburg, Mecklenburg Co., Virginia.<sup>25</sup> It is undoubtedly, as Mooney has shown, the designation of a Siouan-speaking tribe affiliated with the Saponi and Tutelo. Yet we have no linguistic proof of such an affinity beyond the statement that their languages were similar, which is supported by testimony given to Hale by Nikonha, the Tutelo.<sup>26</sup> Nor is it possible to add anything to its meaning or history from Catawba sources. Its connections were, however, definitely with the northern (Saponi, Tutelo, or Nahissan) branch of the eastern Siouan tribes with whom they finally combined. Bland (1650) writes of the Occanacheans and Nessoneicks (which I have already noted is a synonym for Nahissan) as living together on a branch of the Roanoke.<sup>27</sup>

As an advance step in the attempt to explain the tribal names in this area of puzzling terms, I would make bold to suggest a possible explanation of the word Occaneechi, using Tutelo sources for the purpose, since we have accepted the conclusion offered by Hale and by Mooney that Saponi, Tutelo, and Occaneechi were related and reciprocally intelligible tongues. Reverting to the Tutelo vocabulary recorded by Hale we encounter the term yuhkañ, "man," among five other synonyms listed.<sup>28</sup> I suggest, accordingly, that Occaneechi, and its related forms, are derivations from this form (occan=yuhkañ) with a terminal modifier; whence Occaneechi, Occanachee (ans), Akenatzy, denote an equivalent of "people." This possibility leads even farther. In the early form Monacan, denoting, in the 17th century, the Saponi, Tutelo, and probably the Occaneechi assembled, we may have a corruption of Tutelo amāñi, amai, "land," prefixed to the term yuhkañ, whence tentatively develops amāñ(i) (y)uhkañ or Monacan, "people of the land."<sup>29</sup> The sense of this meaning, as being logically applicable to native tribes in America and elsewhere in reference to themselves, is too obvious to be seriously disputed by the ethnologist.

*Cheraw, Sara.* Of the tribal groups of the Carolinas supposed to be of eastern Siouan classification, the name of the Cheraw, or Sara, has engaged much attention. From the earliest mention of the region (DeSoto, 1540) to

<sup>25</sup> Lederer; see article *Occaneechi* by Mooney, *Handbook of American Indians*, Part 2, p. 103.

<sup>26</sup> Hale, p. 10.

<sup>27</sup> Bland, p. 16.

<sup>28</sup> Hale, p. 41.

<sup>29</sup> The Catawba equivalent is mā, or man, or mono, "land, ground, earth." Mooney inclines toward a similar explanation of this element (Mooney, 1894, p. 26), giving also Strachey's (1722) attempt to explain it as derived from Powhatan monohacan or monowhawk, "sword," and Heckewelder's from Delaware "spade" or "digging instrument."

the close of the Indian period of independence there (1838), the name Cheraw has persisted in recognizable form attached to the same general tract of country, namely North and South Carolina east of the Swannanoa Mountains of the Blue Ridge to the Pedee River, following the strip of country along the boundary line between these two states. Cheraw, with its variants Sara, Suala, Xualla, is a name to conjure with in the reconstruction of early Siouan ethnology of the Carolinas. First let me observe, however, that to the recent generations of the Catawba no tribe is remembered under any form of the name, except through its connection with the hypothetical synonym *i'swǎ're*, "river," to be dealt with shortly. So from Catawba sources our information remains purely a matter of linguistic speculation. Gatschet, nevertheless,<sup>30</sup> did suggest an interpretation of the term Sara as "a place of tall grass or weeds." My own opinion would be that the above has value only as a guess, somewhat warped through a supposed connection with the term *sǎra'k*, "grass, weed." Even without taking into account our ignorance of the original accented syllable in Sara, and disregarding the differences in the first vowels, the term lacks the elements denoting locality *mǎ*, *mǎ'nǎ* "land," "place," or the locative *mǎ*, "in," "at," when the meaning given by Gatschet is implied. The name Sara (Cheraw) is worthy of being reexamined for what it may contribute to our meagre knowledge of the region.

Mooney has assembled the synonyms of Sara from available early sources. These forms resolve themselves into modified spellings of Cheraw, Chaula, Xuala (Shua'la), Suala, and most frequently Sara.<sup>31</sup> Among others should be mentioned the name applied to the tribe by the Cherokee, namely Suwali (plural Ani Suwali) according to Mooney's information, to which he adds the note that the name is still familiar to the Cherokee and that it is embraced in the designation for Swannanoa Gap crossing the Blue Ridge east of Asheville, which in Cherokee is "Suwali-nu<sup>n</sup>'ahi, or Suwali trail, that being the pass through which ran the trail from the Cherokee to the Suwali." This is positive and acceptable evidence in corroboration of the name of the tribe as being a variant of Suali, Suala or Saura. Mooney attempts to do more with this interesting appellation.<sup>32</sup>

The term is manifestly not a Cherokee term, but like the Cherokee name for the Catawba (Ani ta'gwa, plural) is a Cherokee attempt to pronounce the word (Ka)ta'pa, and derived from the name used by the

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<sup>30</sup> Referred to by Mooney, 1894, p. 56

<sup>31</sup> Mooney, 1894, p. 56

<sup>32</sup> Mooney, 1894, p. 57.

people themselves. Hence we may look for the explanation of Suala, Suara among the glossaries of the Southeastern Siouan tongues.

A monograph on the Cheraw was written by Alexander Gregg (1867) who indulged in some fallacious speculations on affinities of the tribe, but who discussed and quoted some documents that provide us with information on its history.<sup>33</sup> He quoted the South Carolina Gazette, July 7, 1739, as stating that eleven leading men of the Cheraw and Catawba had been to Charleston to settle a question arising from the murder of a white family in the borders of Virginia, saying that the Indians had put five of the ringleaders to death. In 1744 (Journal of the Council of South Carolina, Vol. 2, p. 133) another entry is quoted in reference to some Pedee Indians who informed the Governor of the murder of seven Catawba by Natchez Indians. These references only show the existence of the Cheraw as a people during that period, and point out the possibility of learning more of them from a careful study of the files of the Gazette. Gregg also describes the characteristics of a Cheraw burial mound in the upper Marlborough district. There is also evidence of the amalgamation of the Cheraw and the Congaree with the Catawba by 1746<sup>34</sup> which corroborates the testimony of Adair for the same period. The Cheraw were then contemplating a withdrawal from the Catawba. That the two continued living together is shown by a statement referring in 1759 (South Carolina Gazette, June 2, 1759) to "45 Cheraws incorporated with the Catawbas," and to the smallpox carrying away about one half of the Catawba, Cheraw, and Wateree.

*Eno.* The name Eno in several cognate forms also occurs as a tribal designation in the region designated by Mooney about 150 miles to the northeast of the Catawba territory, on the headwaters of Tar and Neuse Rivers. Enoree River still bears their name. Mooney<sup>35</sup> traces their subsequent history down to their incorporation with the Catawba, among whom they retained their distinct name and dialect as late as 1743, according to Adair.<sup>36</sup>

Here is likewise a name to experiment with. At this late day no tribal group is known bearing such a title, but the term has a definite meaning in the Catawba language: *i'nare*, "to dislike," whence, "mean," "contemptible," from which *yei'nare*, "people disliked," may serve as a

<sup>33</sup> Gregg, pp. 9, 19, 25. He attempts, for instance, to derive the term Cheraw from Chera-kee (Cherokee).

<sup>34</sup> Gregg, p. 11, quoting Journal of Council of South Carolina, No. 2, pp. 413-14.

<sup>35</sup> Mooney, 1894, pp. 62-64.

<sup>36</sup> Adair, p. 224.

proper name to denote a people whose place in the esteem of the Catawba would be that of a despised nation. Strangely, through the links of testimony preserved for us regarding a population so little known in the documents of the age, we learn from Lederer (1672) that the Eno were of "mean stature and courage, covetous and thievish, industrious to earn a penny and therefore hire themselves out to their neighbors who employ them as carriers or porters."<sup>37</sup>

The identity of these Indians is not certain, as Mooney shows. Even the fact that the name has a definite meaning in the Catawba language does not decide the question of its eastern Siouan affinity, although Mooney tentatively assigns it to this group. The occurrence, indeed, of the plural of the name in Algonkian form—Yardley (1654), Heynokes; Lederer (1672), Oenock<sup>38</sup>—now has to be taken into consideration, although here again an attempt to consider it an Algonkian division on such grounds would be as forced as to class them with the Siouan people for reasons noted above. It should be recalled that Lederer had with him an Indian interpreter from the Virginia Algonkian-speaking peoples and it would have been natural for such a one to give a foreign tribe a plural termination in his own tongue, as frequently occurs in the employment of tribal names.

The possibilities arising out of these sources of information lead in two directions. One is that the Eno or Wino (plural form with varied spellings: Weanoc, Wenoak, Weynokes, Wainoake, Haynokes, Oenock) were an Algonkian-speaking group that drifted away from the Powhatan confederacy about 1650 and wandered slowly southwest, finally became absorbed a little over a century later in the Catawba Nation. The other leads us to consider them to have been an eastern Siouan people of the Virginia area that entered at some time into the Kingdom of Powhatan, and later separated from it, joining its linguistic kin, the Sugeree (Shoccoree), as the disturbances of the colonial period broke upon them, both to lose themselves ultimately among the Catawba.

That Algonkian-speaking groups did join the Catawba Nation has already been shown on the evidence of Adair.<sup>39</sup> And on the other hand that Siouan-speaking groups were associated with the Powhatan peoples as neighbors is shown by Beverley and by Bland in their remarks upon

<sup>37</sup> Lederer, p. 15.

<sup>38</sup> *Wi'na'k'*, *wi'no*, "bearing in abundance," hence sassafras, "tree that bears well" (Delaware), *wi'no wa'kiŋ*, "bearing land," and *wi'no wa'ke'yok*, "people of the bearing land," are acceptable Algonkian equivalents

<sup>39</sup> Adair, pp. 224-25. The Chowan of the coast of North Carolina are referred to.

the location of the Occaneechee and Shoccoree (Sugeree). Thus the final decision of Eno linguistic identity is left open, even though the step has been made toward identifying the Eno of 1743 with the Weanock of 1607 in Virginia.

The name identity of the Eno, however, needs to be reviewed from another angle, one which might bring the tribe into association with the Powhatan Algonkian of the Virginia tidewater. On the Captain John Smith map of 1608 a village, noted as a principal town with 500 inhabitants, and marked at the bend of the James River, north shore, opposite the mouth of Appomatox River, was called Weanoc, and again he writes Weanocks with 100 men.<sup>40</sup> Records show that this unit, due evidently to attacks of the Iroquois, later moved across to the south side of the James. The village spelled Woaneck is marked on N. Visscher's map<sup>41</sup> as being on the south side of the James opposite the old location. To the ethnologist this means the beginning of a shift in location, a phenomenon so characteristic of the populations of the eastern states and especially in the Carolinas that we can infer as much from it in this particular case as we do in others where a single tribe seeking to better its fortunes moved back and forth over an area embracing territory covered within the boundaries of several states.<sup>42</sup> This move brought the tribe closer to the territory occupied by the Tuscarora south of the upper Roanoke, and placed them in direct association with the Nottaway and Meherrin on their respective rivers. It represented a shift of home less than forty miles from Appomatox River at the location where they were first mentioned by Smith. Under the name Wainoake Indians they are several times referred to by Bland (1650)<sup>43</sup> in various connections with the Nottaway and Meherrin references to doings of the explorers and Indians on the frontier of the Tuscarora. And Lawson (1709) notes a Nottaway village by name Winoak, indicating the later association of the group with the Carolina Iroquoians at that time.<sup>44</sup> And in 1654 Yeardley, spelling the name Haynokes, speaks of them as a "great nation" near the Tuscarora, and links their name with the Cacores.<sup>45</sup> Lederer (1672) also met the Oenock near the headwaters of the Neuse

<sup>40</sup> Smith, *A True Relation* [of Virginia] (Tyler edition, 1930), pp. 34, 84.

<sup>41</sup> See bibliography below.

<sup>42</sup> Beverley (1722) stated that their former settlement on the James was then extinct, and that in 1727 they were living on upper Nottaway River and upon a tributary stream they called Wyanoke Creek near the North Carolina frontier (*Handbook of the American Indians* Part 2, p. 926).

<sup>43</sup> Bland, pp. 9-10, 18.

<sup>44</sup> Lawson, p. 383.

<sup>45</sup> Hawks, Vol. 2, p. 19.

River in his journey across North Carolina, and his location places them also adjacent to the Tuscarora.

This location again is roughly not more than 150 miles southwest from where we first learn of their domain in Virginia. And finally their merging with the Catawba in the next century meant a subsequent shift of residence of only 100 miles farther in the same direction.

The earlier dating for the name as denoting a Siouan band of the Carolinas is in accordance with the opinion of Swanton, to whose judgment I concede the point as a more reasonable tentative solution of the dilemma. He regards the Eno to have probably been a Siouan people associated with the Shoccoree and Sissapahaw, forced northward from South Carolina by the Spaniards in the 16th century, and perhaps also by the Yuchi.

*Sugeree*. Only a few miles north of the old Catawba village of the 18th century were the people known as Sugeree, a small group inhabiting the valley of the stream which bore their name, Sugar Creek. In this proper name we have a good Catawba title, *yesh'gri'hare*, "people stingy" or "spoiled," or "of the river whose-water-can-not-be-drunk." The Sugeree were finally incorporated into the Catawba Nation, yet some of the families evidently lingered on in residence on this creek until the establishment of the present Catawba reservation. For we learn that Mrs Owl herself was born off the reservation on the banks of Sugar Creek. The Sugeree Indians were in all probability a local subdivision of the Catawba proper; at least becoming such by the middle of the 18th century.

Shoccoree is another tribal name to be considered as attached to the region northeast of the Catawba, in central North Carolina. Mooney has comprehensively reviewed their known history.<sup>46</sup> Yet the question of its relationship to the tribal name Sugeree has remained undiscussed. Mooney is silent on the point. We may therefore consider it here. In the first place the terms Shoccoree and Sugeree themselves are similar enough to suggest at a glance a common source of derivation.

The location of the Shoccoree, as it was assigned them by Mooney, seems to my mind to be not far enough to the north and east, for the reason that two early authors, Bland (1650) and Yeardley (1654) both refer to these people as being in touch with the Eno, who I have pointed out might possibly have been a Virginia people (Weanoc) residing at an early period (1612) on the shores of the James below the present site of Richmond. Bland speaks of Nottaway and Shockoores "old fields" in the

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<sup>46</sup> Mooney, 1894, pp. 62-64.



vicinity of Nottaway River, between this and a branch of the Chowan, and again of Nottaway and Shockores old fields or clearings adjacent to the "Wainoake" country in a general location similar to the above notice.<sup>47</sup> Yeardley's mention of the Cacores (Shoccores) indicates them as being at war with the Tuscarora, which is not definite as to location, but can be considered to imply a region as far east as the Nottaway River drainage area.<sup>48</sup>

The mention of these localities warrants, I believe, their habitat being indicated as somewhat nearer the Virginia line than Mooney shows it, judging the matter conservatively, and perhaps across it if one dare go further.<sup>49</sup>

By 1672 Lederer found them living south of the Occaneechi about the heads of Tar and Neuse Rivers, which would mean that they had begun a southward movement, like the Eno. Inasmuch as the Shoccoree with the Eno were finally incorporated with the Catawba by 1743, as were also the people designated as Sugeree, the question is raised as to whether or not the two names may have denoted two branches of the same people. And furthermore, while neither of them is known to the Catawba of today as a tribal title, they are both explainable by Catawba informants when urged for etymological analyses.

*Flat Heads (Waxhaw).* Among other tribal names known in Catawba tradition as applying to their neighbors we encounter *yɛ hiskə'petɛ'həre*, "People Head Flat." These Indians, the Flat Heads—as the Catawba themselves were so often called in colonial times—are thought by our informants to have had flattened foreheads and to have lived *iswə'sigri'tak*, which means either "across the salt-water," or "across Sugeree, or spoiled, River." Other connections of their identity are now lost. Sally Brown could make but one more comment: "There were many of them around here, they said, when there were a lot of different Indians mixed in with the Catawba." The story would seem to be a memory of the Waxhaw, described by Lawson (1701) as having flattened heads, who as we know were neighbors of the Catawba within their own ethnic horizon and finally became incorporated with them.

The association in literature between the Waxhaw and Sugeree, which began in Lawson's time, has evidently continued down to the present, as

<sup>47</sup> Bland, p. 18.

<sup>48</sup> Hawks, p. 19.

<sup>49</sup> Bushnell (*Indian Villages East of the Mississippi*, pl. 7) reproduces a map of 1663, showing a Shoccoe Creek upon the site of the city of Richmond, Virginia, and a village of Powite Indians. The creek name is reminiscent of the Shoccoree Indians.

the preceding discussion of names shows. Their Siouan speech affinity with the Catawba proper established by Mooney in 1894 can hardly be doubted. Furthermore the nearness of the two creeks bearing their names (Sugar Creek and Waxhaw Creek) contiguous to the Catawba River, all within a geographical compass of some twenty-five miles, makes it reasonable to consider them even as village units of the old Catawba Nation in its wider sense.

Waxhaw is not a clear term in the Catawba language. The name is not known to the informants. Distorting the form of the term we might relate it to *wąks'*, "opossum," even *wą'sa*, "cane," but these correspondences do not carry conviction. When we come to the term Sugeree the case, as we shall see, is different, since it has a meaning in Catawba that appears more plausible.

*Miscellaneous Tribes and Names.* Some few of the Carolina tribal designations—those that apply to the immediate environs of the known Catawba habitat—have assignable meanings in the language, and these are of great service in defining the ethnogeography of northern South Carolina. Those having a positive meaning and direct application to the country and its physical characteristics are the following, though they are not known to the Catawba as names of tribes.

Congaree, the name of a large river flowing southeast, and west of the Catawba, and also the proper name of a supposedly Siouan tribe located until 1715 on the river of the same name below where Columbia, S.C., now is. Congaree is evidently a corruption of Catawba *i'swą'* *kəŋ'həre*, "(river) deep."<sup>50</sup> In 1746 the Congaree and Santee were met by the Governor of South Carolina at a Congaree village where he named the King and a few headmen of the Santee: namely Yanabe Yalangway, the King, the old leader, Captain Taylor, Nafrebee and some others.<sup>51</sup> These curious names are all that we possess in the language. They are certainly unlike Catawba forms.

Santee, likewise a river and tribe name below the Congaree district, is beyond question derived from *i'swą'ti*, "the river," or "river is there." In 1715 the Santee had two villages, which together with the Congaree, who had one village, numbered 125 souls. Gatschet derives this term from Catawba *sa'nta*, *sq'ta*, "to run."<sup>52</sup>

Sewee is another tribal name affixed to the region about the mouth

<sup>50</sup> Mooney (1897-98, pp. 508, 381) thought the Cherokee proper name *Ani'-Gill'* to be a possible reference to the Congaree.

<sup>51</sup> Gregg, p. 11; quoting *Journal of Council of South Carolina*, No. 11, pp. 413-14.

<sup>52</sup> Gatschet, *Onomatology of the Catawba River Basin*.

of Santee River. In Catawba can be made out the form *yɛ səwi'həre*, "playing people," though the meaning is founded solely on its etymological reconstruction. *Si'wi'* also denotes a flower or blossom. Gatschet thought the term connected with Catawba *sāwe'* "island."<sup>53</sup>

The Catawba River, before it enters the Santee, is for some distance known as the Wateree. Here in early times was located a people known to the colonists as Wateree, but no longer remembered in the traditions of the Catawba. The river name, however, has an assignable meaning in the language, namely (*i'swə'*) *watəɾə'həre*, "(river) banks washed away." The name is indeed an appropriate one.<sup>54</sup> Were we to secure the full proper name of the people of such a river in the Catawba language, it would be *yɛ i'swə'* *watəɾə'həre*, as from *watəɾə'*, "to float on the water,"<sup>55</sup> "people (of the) river of banks washed away." Gatschet gave its derivation.

Pedee is likewise the name of a river of eastern South Carolina, and of an extinct tribe located upon its shores and classified with eastern Siouan speaking peoples by Mooney. While neither river nor tribe are known to the present Catawba, the name may be turned into a meaning in their dialect. By manipulation Pedee comes to sound like *pi'ri*, "something good," or *pi'həre*, "smart," "expert," "capable," whence *yɛ pi'həre*, "people clever." We have some mention of the tribe as living on Uche Island in 1748, and owning slaves, under a King named Billy.<sup>56</sup>

Etiwaw is accepted by Mooney as the name of one of the small tribes of the Cusabo group, last mentioned in 1751.<sup>57</sup> Gatschet's attempt to translate the name as "pine-tree" from the Catawba *itawa* is no more valid than the other equivalents in Catawba for the unknown tribal names of the Carolinas.

Several Muskogean names can be construed into meanings in Catawba without these, however, being in any way responsible for their origin. Among these is *Kusa*, one of the synonyms for the Creeks. *Yɛ kusa'həre*, "man standing, or staying, there," and *Yemasee*, the name of the exterminated tribe that lived on the lower Savannah, equivalent to Catawba *yɛ'musi'*, "old man," or "old people." Yet it should be noted that neither the Creeks nor the Yemasee have been known even by name to the later Catawba informants.

<sup>53</sup> *Ibid.*

<sup>54</sup> For instance Monongahela River (Penna.) is Delaware (Okla.) *man'a'ŋgehəle'*, "caving banks."

<sup>55</sup> Gatschet, *Onomatology of the Catawba River Basin*.

<sup>56</sup> Gregg, pp. 13, 18 (quoting *South Carolina Gazette*, Aug. 30-Sept. 6, 1748).

<sup>57</sup> Mooney, 1894, p. 84.

While investigation of the language was going on the entire list of tribal and place names of the supposedly Siouan area of the Carolinas was examined with all four speakers of Catawba, with the results as given above. Except for the river names in the country adjacent to the Catawba,

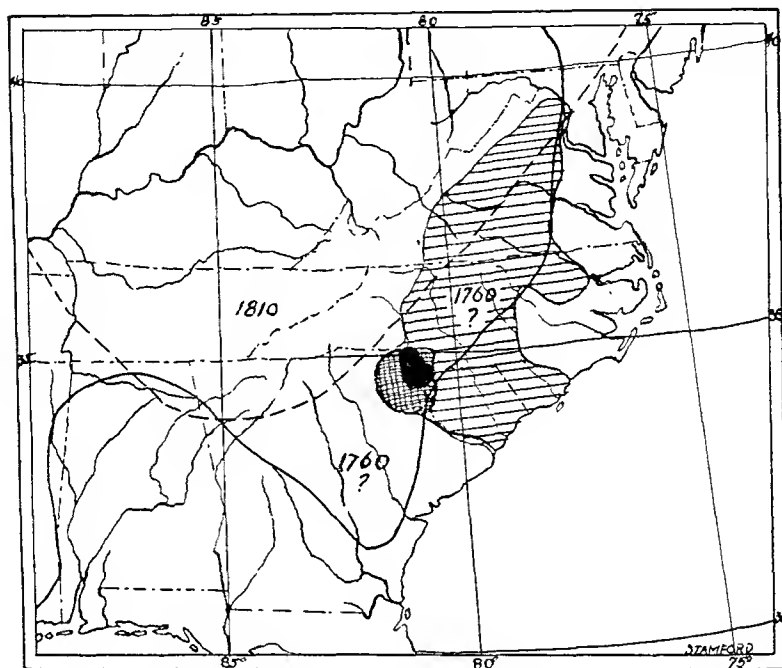


FIG. 1 Distribution of Catawba and related Siouan tribes, and certain life forms in the Southeast functioning in Catawba culture. Hatched area Distribution of southeastern Siouan tribes (after Mooney, 1893); Cross-hatched area Former territory of Catawba from historic tradition and documentary sources, 17th century, Solid area. Habitat of the tribe subsequent to the Revolution, 1780; Broken line. Southern distribution of wapiti (elk) in 1500 (after Thompson-Seton), Solid line: Southern distribution of bison in former times (after Hornaday).

the results show the futility of hoping for light, through a study of the Catawba language, on the history and affinities of the dozen or so mysterious tribes whose titles only remain on the colonial records.<sup>58</sup> We are per-

<sup>58</sup> A chance possibly still remains to shed light on some of these through the Tuscarora, which it is hoped will be attempted soon.

mitted, however, to clinch the point made by Mooney concerning the close affinity with Catawba of the populations known as Waxhaw, Congaree, Wateree, Santee, Sugeree, and Cheraw or Sara. And this is indeed something accomplished. I have, therefore, included the territory ascribed to these tribes by Mooney within the area occupied by the so-called Catawba of the eighteenth century on the revised ethnic map accompanying this study (fig. 1).

I have already referred to several of Gatschet's attempts at explanation of tribal and river designations corresponding to the terms just given. He says, "all these local appellations, probably many more, are terms from the Catawba language."<sup>59</sup> As far as the Catawba etymologies are concerned there can be no objections to the statement, but I must point out that the equivalents he offers for the geographical terms are no more than analytical renderings, derived from linguistic material, not from current traditional memories of the tribes in question among the Catawba informants. It is barely possible that Billy George, who was living at the time of Gatschet's visit to the Catawba, knew more than the present informants do; but I cannot believe that even the conditions of knowledge at that time among these Indians would have justified the acceptance of the meanings given as the actual sources of origin of the long extinct tribal eponyms: "Sewee" from *sāwe'*, "island," and "Kayaways" (Kiawah) from *ka'ia*, "a species of turtle."<sup>60</sup>

For several other names, whose identity will remain a puzzle for some time to come, there is at present little to be said. So with Keyauwee, Yadkin, Sissipahaw, and Woccon no treatment can be offered from Catawba sources that would enlighten the obscurity which surrounds their relationship with other Siouan tribes of the Southeast. Only by forced etymology can these names be fitted into the Catawba lexicon. Swanton has already pointed out the probable affinity of Woccon with Catawba.

A still untested source of information remains among the Quapaw, or Arkansas<sup>61</sup> as they were also styled. This group dwelt in the 18th century in the region connected with some phases of early Catawba history and its survivors should be expected to remember something of the Catawba name and contacts.

<sup>59</sup> Gatschet, *Onomatology of the Catawba River Basin*, p. 53.

<sup>60</sup> Catawba *kaya'* denotes the box turtle (*Cistudo Carolina*).

<sup>61</sup> Among the Delawares (Oklahoma), for instance, the Quapaw are called *Oka'χpa*, which among them is a term of derogation. The Quapaw are by them thought to have been a poor wandering people, whence the appellation *akq'ceoxkaniχke'ti't*, "Brush-house dwellers" (*akq'ce*, "brush").

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## THE SO-CALLED CHACO-SANTIAGUEÑA CIVILIZATION (ARGENTINA)<sup>1</sup>

By ANTONIO SERRANO

THE present Argentine province of Santiago del Estero is situated in the north central portion of the republic. It consists of a large plain, two-thirds of which are covered with woods, with low mountains on the west and south. The great plain is covered in some parts by salt-pits which indent the ground here and there. There are two long rivers, the Dulce and the Salado, which run parallel through the province from northwest to southeast. Between them there is a piece of land of about 100 kilometers in width, to which is sometimes given the name of Mesopotamia Santiagueña.<sup>2</sup> The course of both rivers is variable on account of the plain through which they run. The historical documents of the colonial period frequently speak of these changes in course; changes which took place also during the period of independence.

The river Dulce has its sources on the Aconquija Mountains, in the neighboring province of Tucumán, and after crossing most of Santiago del Estero disappears in the Porongos Lagoon in the northeastern section of the province of Córdoba. The river Salado, much longer than the Dulce, has its origin in the Calchaquies valleys and empties its waters into the great river Paraná after diagonally crossing the whole of the province of Santiago del Estero and a great part of that of Santa Fé.

The "Mesopotamia Santiagueña" is the territory which in the conquerors' language corresponds to the "provincia de los Juries;" the Juries being a nation of sedentary Indians grouped in small villages along the river Dulce especially, nearly from its sources in the province of Tucumán. According to Sotelo Narvaez, 12,000 Indians served Santiago del Estero,<sup>3</sup> but Padre Bárzana puts this number up to 25,000. From this datum we may infer the numerical importance of the Juri nation, since the *tasa* [taxed?] Indians included only adults from 25 to 50 years old.

As has been said, this numerous nation was distributed in small villages, some inhabited by only one family, others by groups of related families. They were farmers who sowed twice a year, in August and in January. The products were maize, pumpkins, and kidney-beans. They gathered

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<sup>1</sup> Translated from the Spanish by Maria Inés Alvarez.

<sup>2</sup> It must not be confused with the so-called "Mesopotamia Argentina," a zone on the eastern side of the republic bounded by the rivers Paraná and Uruguay.

<sup>3</sup> Santiago del Estero, present capital of the province of the same name, was founded by Francisco de Aguirre in the year 1553



some wild fruits, especially carobs (*Prosopis*) and mistoles (*Ziziphus mistol*); they reared guanacos; they also fished, hunted, and gathered wild honey. They prepared fermented drinks with the carob fruit and maize.

The men wore a kind of curtain [kilt?] made of feathers of ñandú (*Rhea americana*) held to the waist, while the women covered their bodies from the waist downward with a stuff made of guanaco hair or vegetable fibres.

Los yndios e yndias desta tierra [we read in Alonso Abad's Information of services] todos en general andaban desnudos con unas plumas de avestruz y las yndias también desnudas y con unas pampañillas cubriendo sus verguenzas solamente.<sup>4</sup>

Besides this loin-cloth, women wore a mantle which they put on their bodies under one arm and tied over the shoulder.

The weapons these Indians had were bows and arrows; these always poisoned. Their dwellings were bohios. The conquerors say nothing about their earthenware industry but we know from these sources that they had clay pots.

The chroniclers and the Jesuit reports speak of another nation, the Tonocotés, which I have studied in a recent work.<sup>5</sup> In this I have shown that the Juries and Tonocotés are two distinct entities, contrary to some interpretations of earlier authors who considered Juries and Tonocotés as terms designating the same nation. They are distinct nations; the former near the river Dulce in Santiago del Estero and the latter near the river Bermejo in the Chaco.

The archaeology of the territory occupied by the Juries was for a long time unknown. There was a feeling that nothing could be found in that region but vestiges of a rudimentary culture. Hence the finds made by the explorers Wagner surprised the lay and scientific worlds holding these mistaken notions and led to the weaving of a legend about a millennial civilization to which has been given the name "Civilización chaco-santiagoueña."

Let us present a bit of history. Beginning in the year 1927 the French brothers Emile and Duncan L. Wagner made a series of archaeological explorations in the zone northwest of the place called Melero (F.C.C.N.A.) in the so-called Chaco-Santiagoueña. Since then they have discovered a

<sup>4</sup> Gobernación del Tucumán. Correspondencia de los Cabildos en el siglo XVI (published under the direction of Roberto Leviller, Madrid, 1918), page 206.

<sup>5</sup> Antonio Serrano, Etnografía antigua de Santiago del Estero, Siglo XVI (Boletín del Instituto de investigaciones históricas de la Facultad de filosofía y letras, Vol. 17, pages 337-74, Buenos Aires, 1934).

great mass of archaeological remains, consisting principally of polychrome earthenware which can be compared for artistic value to the well-known wares of the Diaguitas and Pueblos. Up to the present there has been no publication of the whole of these valuable discoveries; merely journalistic accounts and short notices in reviews. The discoverers published their first signed work,<sup>6</sup> together with excellent pictures, as late as 1932, although earlier they had given their opinions in lectures and interviews.

Let us see how the brothers Wagner summarize their discoveries:

It is not only the considerable number of tumuli which attracts the attention of the investigator. But the different processes used to raise them, their composition, settlement and disposition, and the way they are grouped, either in large villages or in more or less small hamlets, as well as the use to which they were destined, are subjects of great importance demanding a detailed examination.

The tumuli in Santiago del Estero are for the most part made only of the materials taken from the ground where they rest: vegetable soil mixed with sand and clay in proportions which vary according to locality. It is true that in some rare cases, there seems to be noticed in the upper part of the artificial mounds a certain quantity of material different from that of the ground whereon they are raised. But this point has not been yet sufficiently cleared so as to give definite conclusions. It deserves, notwithstanding, the close study that the Misión Arqueológica de la Provincia de Santiago del Estero is now undertaking. It can already be affirmed, taking into consideration the great number of villages and hamlets that have been explored, that these foreign materials have been used only exceptionally.

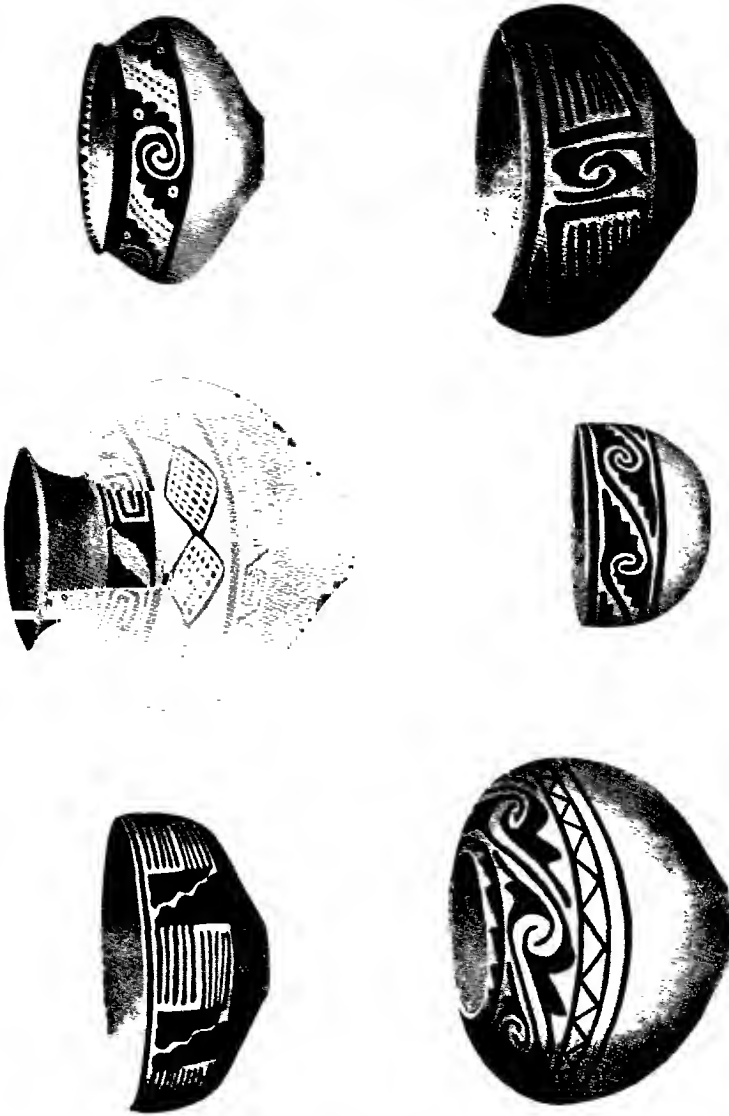
The groups of tumuli are found on the borders of the present courses of the rivers Salado and Dulce or along the old beds of these capricious rivers, which have changed their beds more than once during the historic period as well as during the prehistoric, either veering into the open country or under the dense woods. There exist two quite distinct systems of distribution. One comprises small groups of tumuli, numbering up to ten, set on the borders of the pools improperly called *represas*, whose excavation has provided the soil which composes them. These little groups are generally placed at an arrow's distance one from the other, a disposition which is possibly due to defensive purposes. The tumuli measure up to three meters and a bit more sometimes, and the pools have an equivalent measure in depth.

Such agglomerations may be composed of 400 or more pools with their corresponding tumuli and extend over several hundred hectares.

The other system consists of long avenues, sensibly parallel, formed by tumuli of oval aspect which touch each other or are very near. These avenues, from which soil has been extracted to be carried to the tumuli, formed natural channels or drains of a sort, which carried off the rain water towards large runways and deep ponds,

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<sup>6</sup> Emile C. Wagner and Duncan L. Wagner, *La civilización chacosantiagueña* (Buenos Aires), 1932.



Pottery of the Charco-Santiago culture. The upper left and lower right vessels bear the pettiform decoration called "hands" by the Wagner brothers.



Funerary urns. That on the right is called by the Wagner brothers "anthropo ornithocephalicum with the characteristic tears of the Weeping Divinity" (Lagones).

evidently dug on purpose to receive it: a conclusion drawn from the fact that the borders of the latter, in tumuli form and more or less circular, never show any signs of having been used, as happens with other artificial mounds.

These groupings in avenues, which we shall designate as towns or villages, were frequently very extensive. If we take for example Lljajta Maica (near Melleros, F.C.N.A.) we can make it clear that this grouping, crossed by a great number of parallel avenues, covered a surface calculated as nearly 400 to 500 hectares. Today most of this surface has disappeared under forests which can hardly be penetrated. The land surveyor Carlos Loucheur made a plan of 170 hectares of the whole area: here there are thirty-nine big and deep ponds, to which the workers of the place have given the name *represas* (which means nothing in reality).

The depth of these hollows was calculated so as not to pierce the impermeable clay layer under which there are undrinkable layers of water. Everything makes one think that the object of these ponds—the digging of which demanded a long and costly labor—was to hold rain water. Although the rain waters were soon absorbed by the sandy undersoil, with time they would come to form layers of potable water retained by the impermeable clay bed peculiar to the physical constitution of the earth in these regions. The wells, some of whose traces we have found, were dug in connection with these reserves of drinkable water artificially stored in this way.

The villages and hamlets formed actual chains which extended in every direction and which we have been able to follow up to the several boundaries of the vast territory of the province of Santiago del Estero. Up to the present it has been impossible for us to investigate thoroughly the extension of these villages and hamlets in the neighboring provinces and in the Chaco Nacional. Everything allows us to affirm that those rather extensive plains, where tumuli or earthworks are not found and which are situated between the sites occupied by the prehistoric villages and hamlets, were reserved for the cultivation of cereals and other edible plants, and for the pasture of auchenias cattle, more or less domesticated. These cattle were shut up in big yards of which traces are easily distinguished.

The tumuli in Santiago del Estero seem never to have been burying places: so seldom do we find a grave in one of them that we can apply here the proverb that "the exception proves the rule." Of the hundreds of funerary urns that we have collected up to the present, only three were found buried in the central parts of the tumuli.

Sometimes the urns are buried in the border of the tumuli opposite the pond, but most of them are found buried in the open country or under the woods, isolated, and what must be noted, without any kind of funerary apparel, which is completely out of the ordinary.

On the other hand the tumuli are full of fragments of earthenware, broken or whole pottery, and animal bones mixed with simple kitchen refuse. Spindles, small idols made of stone or baked clay, stone hatchets (generally in fragments), tips of bone arrows, beads made of mother of pearl or worked clay, musical instruments, and small bone implements, are also found in great quantity.

The earthenware, more or less well preserved, but in fragments, abounds to such an extent that where it happens that the tumuli have been compacted by time's influence or by the tread of cattle, the pottery pieces, vessels, and bones form such a thick and homogeneous layer on a level with the ground that the spade can not penetrate.

From all this it is easy to conclude that the tumuli of the Chaco-Santiagoña civilization were never used as burial places, or for cultivation or ceremonial or ritual purposes, in spite of the fact that a certain zone in each tumulus was kept for worship. They have clearly been used as dwellings. On top of them they must have built houses made of perishable materials of which there are now no remains—a fact which must not cause wonder.

We must not suppose that these artificial heights were raised in order to preserve the first inhabitants from floods, because we find them in great numbers in regions where there are no remains of water molluscs, which normally occur in such great abundance in regions subject to inundation.

As regards the ponds, everything makes one think that they were purposely made to receive the rain water in times when the climatic circumstances made it more difficult to get drinkable water; this element being so indispensable to human life.

This short note offers the reader only a brief sketch of the very interesting problem presented by the millions of tumuli spread over so vast a region. By construction and disposition they approach those raised at other pre-Columbian towns, and at the same time they differ from them by several notable peculiarities. This has led us to study this subject with close attention and to give it in our work, as can be seen, a place of much importance. But this does not mean that we feel justified on this account in closing consideration of such a complex and difficult subject.

Enrique Palavecino, in a recent work on cultural areas in Argentine territory constructed according to the principles of the new historic-cultural school (*Kulturhistorische Methode*), has resuméd the elements of this Chaco-Santiagoña civilization in the following words:

The Chaco-Santiagoña culture, discovered by the Wagner brothers, can be analyzed into the following features:

*Economy*—Agriculture, maize growing, artificial irrigation. Llama breeding.

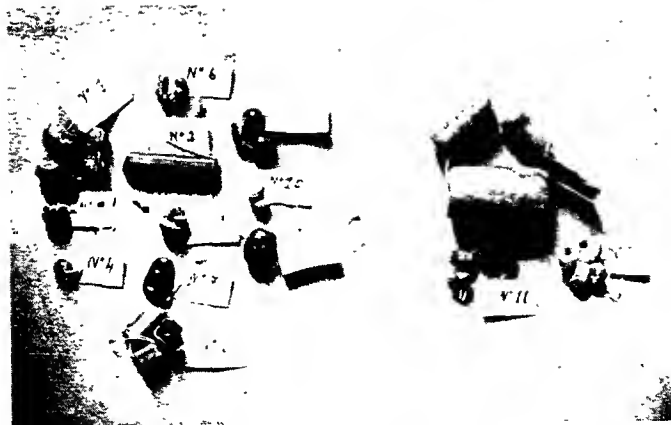
*Dwellings*—Dwellings on top of mounds, natural and artificial, disposed in long streets.

*Clothing*—They knew some articles of knitted clothing.

*Weapons*—Bow and arrow, tips of triangular bones with or without peduncle, lanceolate with small wings and made of long bones with the corners smoothed off.

*Implements*—Punches made of bone, spatulas, etc.

*Industries*—Earthenware: two types, which are found in the same layer but are not mixed: they are in distinct places and regularly alternated. The type called *A* has a painted decoration in various colors and the other, *B*, decoration engraved or



Pottery and other objects of the Chaco-Santiagoueña culture. Above: painted mugs. Below. European beads (nos. 11 and 12). material [?] and manufacture are native. These beads were found in tumuli in a region adjacent to that explored by the Wagner brothers. (Collection, Paraná Museum)







basketry of the "twimev" [twined?] type. Bone-cutting: they made pipes, whistles punches, and bone combs.

From an anthropological point of view the study of the skulls belonging to the creators of this culture leads to very interesting conclusions. They are somatologically an uniform people.

The general morphology of the skull [says Imbelloni<sup>7</sup>] is decidedly hypsicephalous and brachiate. As regards the facial features: face rather short, in relation to its width, and nose, broad rather than thin. General height of the body rather exiguous; somewhat less than 160 cm. in the living body. This human type corresponds in America to various native groups: some from Peru, Araucanos; some from the Diaguita region, Central Mexico, the Shirvis, Zuñi and Moki.

From the exclusively anthropophytic point of view it would be advisable to apply the denomination proposed by Bonarelli, who gave the name of *Puebloandina* to this variety on the rivers Colorado, Gila, Salado, and Rio Grande del Norte, known in ethnography as the Pueblo area.

The skulls of the Chaco-Santiagueña civilization are deformed by the decubitus system: the baby having its head against a plane in the cradle tied by means of ropes that exert compression. This deformation corresponds exactly to that of the Pueblo-Andinos area.<sup>8</sup>

The elements in this culture have been imputed by its discoverers and by those supporting their opinions to a very old "civilization madre" preceding perhaps that of the Tiahuanaco.

There are reasons to think that this Chaco civilization [Wagner has said] could have taken root in American soil at a time at least as remote as the foundation of those other cultures, whose majestic ruins—so much admired by the Spanish people—were already abandoned a long time before the discovery, and which the Mexicans, Peruvians, and Mayas of the epoch of the conquest could not explain because they did not know who had been their builders—not even their own inscriptions. They knew nothing at all in this matter, or if they knew, it was something vague or contradictory.<sup>9</sup>

In my work above mentioned—most of which is an historical discussion—I have tried to demonstrate that the things found by the Wagner brothers correspond to the Juries' cultural possessions:

<sup>7</sup> J. Imbelloni, Los autores de la ceramica de Lljajta Maica. Primeras noticias antropofísicas sobre los antiguos civilizadores del Chaco-santigueño (Actas y Trabajos del XXV Congreso Internacional de Americanistas [La Plata, 1932], Buenos Aires, 1934, Vol. 1, pages 27-37).

<sup>8</sup> The Americanists specialized in craniometry will find in Imbelloni's work above mentioned valuable contributions to the studies of their specialty.

<sup>9</sup> La Razón (Buenos Aires, January 5, 1931)

1. Because they were made in a zone occupied by these Indians in the epoch of the conquest or in neighboring regions;

2. By the similarity of the systems of irrigation and construction of dwellings on top of high places between the Juries and the discoveries made by the Wagner brothers;

3. By the presence of glass beads and European objects in similar layers and near those of the Chaco-Santiagoueña, with earthenware of the same decorative style and with identical cultural elements;

4. Because no other archaeological remains have been found in the region which correspond in cultural characteristics to the sedentary and agricultural life of the Juries.

The discoveries in the Chaco-Santiagoueña have excited the scientific world in the Argentine and without doubt will excite all Americanists. That is why I thought it interesting to illustrate to my fellow workers of the northern continent what has been given the name of the Chaco-Santiagoueña civilization.

PARANÁ

ARGENTINE

## THE ARCHAEOLOGICAL PROBLEM IN CHIRIQUI

By CORNELIUS OSGOOD

### INTRODUCTION

THE purpose of this paper is to present new information concerning the archaeology of the Province of Chiriqui, Panama, and to summarize from the point of view of the field-worker certain phases of previous work in order that data necessary for approaching new problems may be more readily available.

The impetus for the latter undertaking was afforded by the bequest from the late Mr Wm. J. Lampson of a large Chiriqui collection which has now been added to the historic one in the Peabody Museum of Yale University. To classify and exhibit this new material necessitated a careful analysis of the existing voluminous studies on Chiriquian antiquities with the inevitable result that certain information, when reweighed from the field-worker's standpoint and considered in the light of the discoveries of the past decade, took on a new aspect, only obvious after an unnecessarily long study of monographs and collections. Therefore, I shall briefly restate the classification of Chiriqui pottery as established by Holmes and MacCurdy.<sup>1</sup>

### CLASSIFICATION OF CHIRIQUI POTTERY

The following table represents the MacCurdy list, with the Holmes terminology in brackets:

- A. Unpainted ware [Unpainted ware].
  - 1. Armadillo group [Terra Cotta or Biscuit].
  - 2. Salmon colored group.
  - 3. Serpent group [Black incised].
  - 4. Handled group.
- B. Painted ware [Painted ware].
  - 1. Handled group [Handled].
  - 2. Fish group [Tripod].
  - 3. Chocolate incised group.
  - 4. Scarified group [Scarified].
  - 5. Maroon group [Maroon].

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<sup>1</sup> Wm. H. Holmes, *Ancient Art of Chiriqui* (Sixth Annual Report, Bureau of American Ethnology, pp. 1-187, 1888; G. G. MacCurdy, *A Study of Chiriquian Antiquities* (Memoirs, Connecticut Academy of Sciences, Vol. 3, pp. 1-239, New Haven, 1911).

6. Red line group [Red line].
7. White line group [White line].
8. Lost color group [Lost color].
9. Alligator group [Alligator].
10. Polychrome group [Polychrome].

The following facts need emphasis: approximately ninety-five percent of the pottery in Chiriquian collections can be placed in four definable groups of ware which correspond to five of Holmes' original list. It is obvious that these four groups are the basic Chiriquian contribution to ceramics as evidenced by present collections:

1. Armadillo-Terra Cotta ware.
2. Fish-Tripod-Handled ware.
3. Lost color ware.
4. Alligator ware.

Of the remaining six wares in the Holmes list, three appear to represent diffusion of techniques or of the pots themselves. This is also true of the Chocolate Incised ware, distinguished correctly by MacCurdy. As is well known from historical sources, the people of Central America have been particularly remarked as traders traveling considerable distances and it would therefore be reasonable to expect a certain amount of ceramic exchange, if only for the curiosity aroused by the pieces.

a) Polychrome ware—There are only seven pieces in the Yale collection (*ca.* 4000 pieces). The form, technique of manufacture, and decoration link these specimens with the pottery commonly found in provinces to the east, particularly Coclé. Archaeologically speaking, they should not be considered as Chiriqui pottery.

b) Red Line ware—There are only thirty-one pieces. These are definitely related to that group of Costa Rican wares, which when discovered so resembled the specimens from Panama, that they were classified under the same name. One might reasonably account for the presence of such few specimens as have been found by direct borrowing.

c) White Line ware—With not more than seven out of about four thousand specimens from Chiriqui being of this ware, which corresponds to one of the types of White Line ware from Costa Rica, the same conclusion may be drawn as in the case of the Red Line ware; namely, that they do not form an indigenous Chiriqui group.

d) Chocolate Incised ware—Another typical Costa Rican ware to which the seven or eight specimens from Chiriqui can scarcely indicate more than objective borrowing.

The three remaining wares on the Holmes list are not so easily disposed of, and we need more factual information. Certain suggestions are in place, however:

e) *Serpent-Black Incised ware*—These pieces compose less than one-half of one percent of the Yale collection, but all are decidedly similar in almost every way. They must either represent a minor diffusion of some ceramic type center outside of Chiriqui or a specialized development within. In either case, the origin and affiliations of this ware have not been determined.

f) *Maroon ware*—The few specimens in the Yale collection classified under this heading do not appear to me to constitute a class in themselves, but rather in individual instances show similarities to various Costa Rican wares and also in part to Scarified ware.

g) *Scarified ware*—This group of ware, which Holmes considered unmistakable in contrasting it to the principal types of Chiriqui pottery, is the most interesting of all the numerically inferior types and it has been said by local collectors in the Province to be confined to a certain region of the highlands; a view which only can be substantiated satisfactorily by further excavations.

Having disposed of the problem of reweighing the available classification lists of Chiriqui pottery, it will be worth while to attempt a brief working definition of basic Chiriqui types as follows:<sup>2</sup>

1. *Armadillo—Terra Cotta ware.*

Form of pots:

*Bodies:* Tendency toward pointed bottoms.

*Size:* Variable, but tend to be largest of basic types; tripods medium to small.

*Rims:* None or Fragile.

*Handles:* Rare; ribbon shaped on vertical axis when present.

*Tripods:* Short; bulbous; hollow.

Technique:

*Paste:* Thin; porous.

*Slip:* Thin, gritty, and varying from greyish to reddish yellow. Seldom polished. (The yellow gritty slip is the almost unmistakable feature of this ware.)

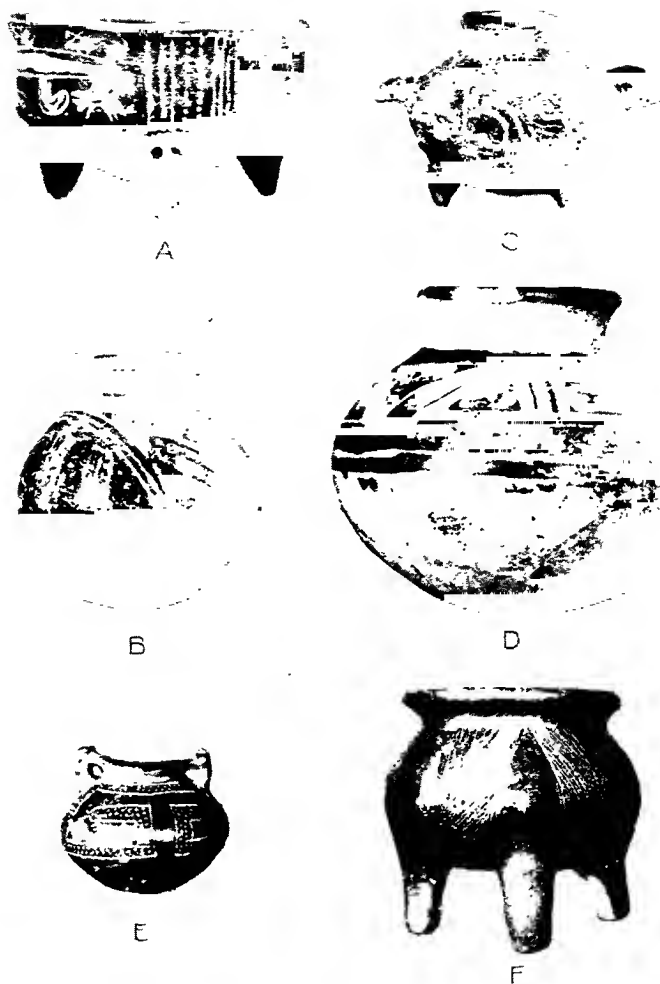
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<sup>2</sup> The names given to these basic groups of ware are merely a combination of the Holmes-MacCurdy lists for the types included. For assistance in determining the defining characteristics of these groups, I am indebted to Mr Irving Rouse.



Chiriqui Pottery. A, B, C, Armadillo-Terra Cotta ware: A illustrates short, bulbous, hollow tripods and modelled adorn; B shows modelled armadillo motive and fragile rim; C demonstrates ribbon-shaped handles on vertical axis and tendency toward pointed bottoms.

D, E, F, Fish-Tripod-Handled ware: D illustrates long, thin, hollow tripods with fish motive and rope handles; E shows modelled decoration, painting and thick rim; F is typical of rude painting, round bottom and thick rim. (2/5)



Chiriqui Pottery. A, B, Lost-Color ware: A shows rare tripod with solid peg-like legs and negative painting; B illustrates typical flask-shape body constructed in two halves and negative painting.

C, D, Alligator ware: C has the solid peg-like legs and the alligator design in positive painting with red and black lines; D shows typical flask-shape body with positive painting in characteristic design.

E, Serpent-Black Incised ware. a typical specimen of this readily distinguished ware.

F, Scarified ware: illustrates typical heavy construction and "scarified" decoration. (2/5)



## Decoration:

*Type:* Modelled and unpainted.

*Design:* Modelled adornos in round or relief, the armadillo motive being predominant.

## Evidences of Use:

Obviously funerary ware; only 5% suggest use, possibly ceremonial.

The salmon-colored sub-ware listed by MacCurdy is included in the above definition.

## 2. Fish-Tripod-Handled ware.

## Form of pots:

*Bodies:* Rounded bottoms.

*Size:* Medium.

*Rims:* Thick and pronounced.

*Handles:* Typical of group and round (generally rope-shaped) in contrast to ribbon handles of the Armadillo-Terra Cotta group.

*Tripods:* Long; thin, hollow.

## Technique:

*Paste:* Thick and coarse.

*Slip:* Usually lacking, light, grey or yellow. Poorly polished, when present.

## Decoration:

*Type:* modelled and generally rudely painted.

*Design:* modelled, with fish motive predominant on legs. Painting often geometric lines or daubs.

## Evidences of Use:

A large percentage are fire-blackened, indicating use as cooking pots.

This ware is a combination of the painted and unpainted Handled ware into which MacCurdy divides Holmes' Handled ware group. Painting or the lack of it in a group otherwise identical does not appear to be, from the problematical point of view, a basic essential for classification. The Fish-Tripod ware of MacCurdy and Holmes is also included. The latter author himself said that the original distinction was based only on the occurrence of tripods, which is not recognized here, since other groups might as well be divided on the same basis.

### 3. Lost Color ware.

#### Form of pots:

*Bodies:* Tendency to be flask-shaped; constructed in two horizontal halves later moulded together.

*Size:* Tendency to be small.

*Rims:* Thick, but less prominent than in the Fish-Tripod-Handled group.

*Handles:* Rare; ribbon-shaped.

*Tripods:* Rare; solid and peg-like.

#### Technique:

*Paste:* Medium.

*Slip:* Highly polished, cream-colored.

#### Decoration:

*Type:* Painted.

*Design:* Negative designs created by the lost color process, i.e., painting the design in wax which was removed after the pot had been resurfaced with black. This is the distinguishing feature of this ware. An aid in recognition is the absence of positive dot designs. Usually the pots were partially or wholly in red before the lost color process was applied.

#### Evidences of Use:

Lack of discoloring may be thought to indicate that the use of vessels was possibly for water bottles or that they were made only as mortuary objects. Some, however, may have had religious significance.

### 4. Alligator ware.

All the definitive characteristics are the same as in the Lost Color ware, except:

*Bodies:* No evidence of bipartite construction.

*Design:* Positive painting in red and black lines; alligator motive typical.

The basic similarities of the Lost Color and Alligator wares show a closer relationship between these two than between them and either of the other wares. The technique of decoration is so unmistakably different, however, that the separation into wares would appear to have been certainly recognized by the makers.

DISTRIBUTION

One of the first problems thought of in connection with Chiriqui pottery is whether the basic pottery groups co-exist in the same cemeteries and the same graves. Returning from an archaeological field trip in South

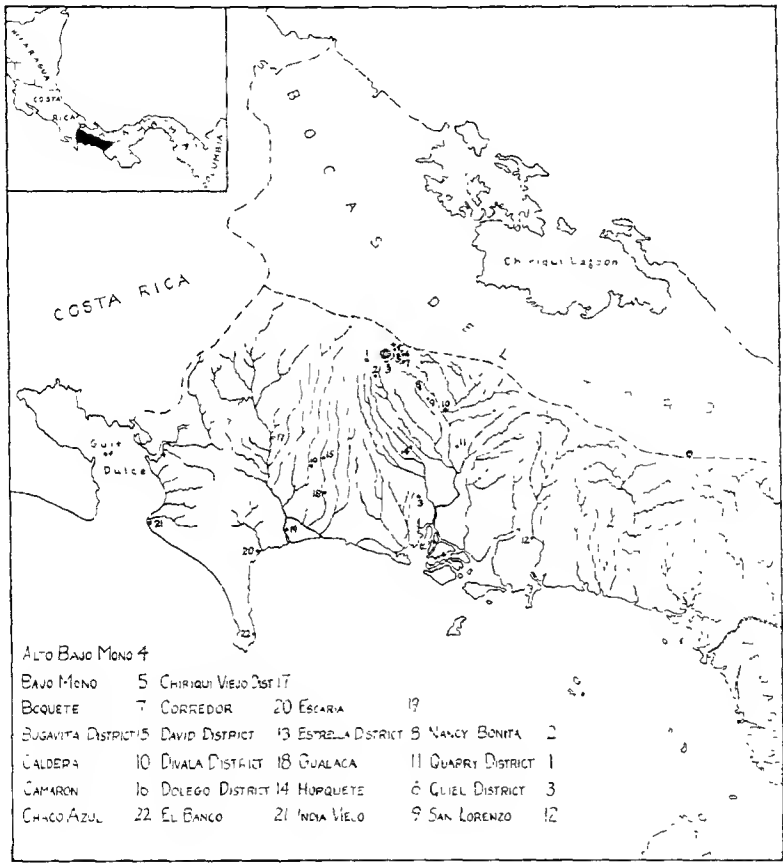


FIG. 1. Archaeological sites of the Province of Chiriqui, Panama.

America, I was glad to avail myself of the opportunity to spend a fortnight in the Province of Chiriqui, where a number of graves were excavated, sites examined, and collectors visited. The rainy season was at its height, which, with the lack of time, allowed no possibility for satisfactorily settling the obvious problems but did serve to gather some contributory data.

From collectors one hears statements that Armadillo-Terra Cotta ware is Highland ware and that Alligator ware is from the Lowlands; that a certain cemetery contains no painted ware; that Armadillo-Terra Cotta ware is lacking from others, but this type of evidence, apart from its inconsistency is insufficient and more exact archaeological work must be done to give the solution.

The accompanying map gives the location of known sites. By analyzing the percentages of basic Chiriqui wares at the three sites from which the majority of pieces come, we do find a notable consistency, however. This is borne out by a similar analysis of 2200 pieces not based on locations. Roughly 50% of all the pottery at both the three known sites and in the collection is Armadillo-Terra Cotta ware, 25% Fish-Tripod-Handled ware, 15% Lost Color ware, and 5% Alligator ware, leaving 5% to include all the non-typical finds. The exact percentages are given below:

	Bugavita		Divala		30 mi. wnw of David		Totals of Localized Pots		Without Re- gard to Sites	
	Pots	%	Pots	%	Pots	%	Pots	%	Pots	%
1. Armadillo	165	57.89	189	50.13	51	45.94	405	52.39	1178	53.54
2. Handled	80	28.07	78	20.68	35	31.53	193	24.96	575	26.13
3. Lost Color	33	11.57	94	24.93	20	18.01	147	19.01	310	14.09
4. Alligator	7	2.45	16	4.24	5	4.50	28	3.62	137	6.22
	285	99.98	377	99.98	111	99.98	773	99.98	2200	99.98

#### ANALYSIS OF PARTIAL INFORMATION ON 117 GRAVES AND THEIR CONTENTS

Through the courtesy of Mr Jack Browne of Boquette, I was afforded the opportunity to analyze the excavation record of 117 Chiriqui graves. This record, although by no means scientifically complete, is the best contribution available at the present time. The graves excavated were in the following eight sites:

1. Bajo Mono (3 graves).

This site is on Browne's Finca on the Caldera River, about three and one-half miles above Boquete. The altitude is approximately 4200 ft.

2. Alto Bajo Mono (40 graves).

This graveyard is in a dense jungle on a high point overlooking the valley of the Caldera River, about five miles above Bajo Mono at the headwaters of the

Caldera River. Altitude *ca.* 4500 ft. The number of stone adzes was unusually great. The covering (tapa) stones were said to be exceptionally large and the graves were arranged in lines. Both graves and lines extended true north and south.

3. Horquete District (12 graves).

This is the valley of Horquete Creek (the next valley northeast of Bajo Mono Valley) which flows into the Caldera River. Altitude *ca.* 4500 ft.

4. Quiel District (16 graves).

This site is on the northwestern heights above Bajo Mono Valley (upper Caldera River) at an altitude between 4500 and 5000 ft.

5. Estrella District (19 graves).

About ten miles south of Boquete and five miles north of India Vieja

6. India Vieja (19 graves).

A graveyard in a deep jungle, only part of which has been cleared for excavation. About fifteen miles south of Boquete.

7. East India Vieja (3 graves).

A site about one and one-half miles east of India Vieja.

8. Caldera (5 graves).

This site, about nineteen miles south of Boquete, is on a small hill overlooking the Chiriqui River to the west. Directly to the east is a high point in the Cordilleras. The burying ground is about half an acre in extent and is near a hot spring.

*Pottery.* As a result of the analysis we find that 45 out of the total 117 graves or 38.46% contained no pots.<sup>3</sup> The average holds with remarkable consistency for all sites considered individually, and there is evidence that the same percentage of round and rectangular graves are without pots. Out of 12 of the 45 graves without pots came stone implements, however.

From the 117 graves came 271 pots or an average of 2.31% pots to the grave excavated. Since these all came from 72 graves, we find that the expectancy is 3.76% pots to the grave containing pots.

Unfortunately, statistical information is not available to give the percentages of types of pottery found in specific graves or sites, but according to my informant the Armadillo-Terra Cotta group is not found at Horquete, Quiel, or Caldera, whereas most of the painted pottery comes from these sites and the low country. To some extent mixed groups of pottery occur in the same grave, but again statistical information is lacking.

*Stonework.* In the excavation of 117 graves 56 stone adzes were found in 28.20% of the graves. The distribution of these finds was very irregular, since from four sites with 50 graves, only 3 adzes were recovered (Bajo Mono, 0; Horquete, 0; Quiel, 1; Estrella, 2). At Alto Bajo Mono, however,

<sup>3</sup> Refers to complete or nearly whole vessels or a group of sherds indicating a pot broken after deposition. Free sherds, however, are not uncommon and it is said that they can be found over the majority of the surface of the Province.

20 adzes were taken from 14 of the 40 graves and in the three other sites with 27 graves, 32 adzes were found in 16 graves (India Vieja, 22; Caldera, 7; East India Vieja, 3). In 18 graves out of 33 more than one adze was found, i.e., in 13 instances 2 were found, and 5 instances 3. Only two round graves contained adzes (3) or 8.69% of the round graves, whereas 31 or 33.69% of the rectangular graves held adzes.

In 117 graves 14 stone metates were found, in almost every instance with an associated grinding stone. Metates occurred only one to a grave and 12 out of the 14 came from Estrella (8) and India Vieja (4); one each coming from Caldera and Alto Bajo Mono. Strangely, perhaps, the most metates came from a site with few adzes. The five graves at India Vieja and Caldera containing metates also contained adzes. Only in one instance was an adze associated with a metate in the other nine graves that produced metates at Estrella and Alto Bajo Mono, a fact which may be explained, however, by the almost complete lack of adzes (2 in 19 graves) at Estrella.

*Construction of Graves.* The analysis of grave types is one of the most desirable bits of information which is lacking. Writers have distinguished from two to six types, but the scientific work is yet to be done. Most excavators distinguish two basic types, the round and the rectangular, each of which admittedly has variations. From this standpoint, out of the 117 graves from which we have a record, 92 or 78.63% are rectangular and 23 or 19.65% are round, the remaining 2 being questionable. In four sites (Bajo Mono, Alto Bajo Mono, Caldera, and East India Vieja) only rectangular graves occur and at Quiel only one round grave was found. At Horquete, however, 9 out of 12 graves are round, at Estrella 9 out of 19, and at India Vieja 4 out of 19. Thus we see that the distribution of grave types with an analysis of their contents may be of considerable importance on future examination.

From limited evidence it appears that most rectangular graves are covered with one, two or three large flat (tapa) stones and that these vary from two to four feet below the surface of the ground. The extreme dimensions of the grave itself can be said to vary from  $4\frac{1}{2}$  to 12 ft. deep by  $4\frac{1}{2}$  to 8 ft. long and  $2\frac{1}{2}$  to 5 ft. wide.

The data on round graves shows them to be the same depth below the surface and from 4 to 10 ft. in depth by 4 to 9 ft. in diameter.<sup>4</sup> The round graves usually have round river stones in place of tapas; often the walls are constructed of these stones, but sometimes of clay. Generally the bottoms of the graves are indistinct, but occasionally they are surfaced with flat stones or gravel.

<sup>4</sup> The information on sizes of graves suffers from lack of precision.

In conclusion to this analysis of graves examined, a few interesting notes may be added. Generally graves are in series forming a single line due north and south; rarely graves will be parallel one with another, but in this case, as in the other, the grave will lie north and south.

Several examples of killed pottery have been found. Most of the graves have the pottery at the north end, but some have it at the south end and some at both.

Pots in some graves appear to have been thrown in, while others seem carefully placed.

Gold, polychrome ware, or open-work stools are said not to have been found during the excavation of several hundred graves.

### CONCLUSION

The only certain conclusion which can be made is that little is known about the archaeology of Chiriqui beyond the information implicit in a collection of antiquities from the region. The summary of what is known should stimulate an interest in discovering the few but important facts which will place Chiriqui on the map of archaeologically explored provinces in America. For the benefit of my friends in Panama who have periodic opportunities for work, I take the liberty of suggesting some of the local problems, the answers to which are needed:

- a) A description of cemeteries in terms of location, environment, possible demarcation stones, etc., and grave positions.
- b) An exact description of the structure of graves.
- c) The distribution and association of established grave types.
- d) Place of origin of stone used in grave construction.
- e) The position of grave objects in graves.
- f) A statistical summary of the association of pottery wares in specific graves and cemeteries.
- g) Distribution of Scarified ware in particular, leading to the proof or disproof of its centering in the graves of the region, including India Vieja, Estrella, and Caldera.
- h) Distribution of stone work, including correlation with specific graves
- i) The association of adzes with rectangular graves.
- j) The percentage of three *versus* four legged metates.
- k) Data on the association of gold with specific grave types.
- l) Indications of successive burials in the same graves.

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# THE POSITION OF WOMEN WITH REGARD TO PROPERTY IN PRIMITIVE SOCIETY<sup>1</sup>

By W. SCHMIDT

## INTRODUCTION

TO everyone occupied with studies of property and ownership in primitive society, as I have been for some years,<sup>2</sup> two principal points offer themselves for elucidation: the nature, quality, and limits of property itself, and the number and quality of the owners. It is with the latter, or more exactly, with an interesting part of the latter that I propose to deal in this lecture.

There is a double point of view which makes property and ownership important for man: the individual and the social. From the former, property realizes in some way an extension and corroboration of the proper self into the exterior world in order to dispose of its objects for its own use. From the social point of view, it is of interest to know to which persons society permits ownership; which persons it credits with the faculty of exercising rights of property. Now, if the subject of my present lecture is "the position of women with regard to property in primitive society" you will understand the double interest that it excites: first, as to what degree women may be able to develop the faculties of their proper selves by independently disposing of the objects of exterior nature as true owners, and secondly, within what limits primitive society considers women capable of fulfilling the right of property. You will see that the answer to be given to these two questions is of a very variant nature, but that this variety is not hopelessly irregular, but depends, as I venture to say triumphantly, on the position of women in my several (beloved) culture-circles (*Kulturkreise*).

If these *Kulturkreise* are known to you a little, you will remember that I distinguish between the more ancient *primitive* cultures and the younger *primary* cultures. The difference consists in that in the former man depends for his subsistence exclusively on what nature offers to him quite spontaneously, while in the latter man begins to work upon nature by developed ("higher") hunting, or by animal breeding, or by horticulture, in order to take the functions of its productive power into his own hands and thus render it more abundant and more certain. You will find it quite natural that the nature and number of objects of property possessed

<sup>1</sup> Lecture delivered at the First International Congress of Anthropological and Ethnological Sciences, London, 1934.

<sup>2</sup> In preparing a somewhat longer work: "Das Eigentum auf den ältesten Stufen der Menschheit."



in these several cultures are very different, but you will see that the number and quality of the subjects possessing objects of property also differ greatly, and with them the position of women with respect to this.

#### I. WOMAN'S PROPERTY IN THE PRIMITIVE CULTURES OF HUNTERS AND FOOD GATHERERS

Let us consider first the earliest economic stage in the primitive cultures of hunters and food gatherers. You know that in this stage any kind of individual ownership of the soil is absent; not only that of the individual person but also of the individual family. Almost everywhere it is the loose *Grossfamilie*, the sib, to which a kind of land ownership is to be ascribed. Such a community consists of those who are able to work—the two parents, father and mother—together with those who are not yet able to work—their children—and also those who are no longer able to work—the parents of the father and in some cases also those of the mother—all living on the animal and vegetable food collected in that part of the territory over which they exercise the exclusive right of ownership. If in these most ancient cultures women have no individual property rights in the soil, men also are not in possession of it; and one may venture to say that here women approximate this right more than men, because the objects of the collecting activity of women, the plants, are in more intimate relation to the soil than those of men's activity, the animals.

While no kind of individual territorial property exists in primitive culture, I have found in nearly all tribes of this cultural type—Asiatic and African Pygmies, North Central Californians, Fuegians, Southeast Australians—individual property in trees of special value (wood, fruits), wild bee hives, termite heaps, and similar natural objects. Here the true nature of individual property manifests itself in the fact that the owner transfers his right to his children or they automatically inherit it from him. Unfortunately, as the general information about this kind of property is still very defective, we are seldom informed of the sex of the owner of these objects and whether women are proprietors. Only in a very few cases have we positive evidence of this: among the Kenta Semang, among the Central and Eastern Pomo, and among the Vedda, women may be proprietors of such objects. But in all three cases there is sufficient reason for suspecting that influences of younger (i.e., later) matriarchal culture are concerned, and it therefore remains doubtful whether women in primitive society may be proprietors of this kind.

Individual property exists in primitive society for all the "movable" objects resulting from individual activity or acquired by gift from an-

other person. This kind of property found in primitive society the greatest extension it ever had in the history of mankind. In the societies representative of the earliest period of humanity the subjects of property are practically unlimited: not only adults, but also children; not only men, but women as well may be owners of such objects. And the rights of property of the children are respected by adults as those of women are respected by men. Thus women may freely dispose of the vegetables, fruits, etc., they have collected. If they give them first to their own families, they do it not only on behalf of their husbands but also of their children and they are compensated by the animal food furnished by their husbands. But with all this, women remain free to distribute presents of food to other families and thus to take part likewise in that "love of giving," which Prof Radcliffe-Brown has reported as quite characteristic of the Andamanese, Father Schebesta of the Semang and African Pygmies, and other observers of other tribes of this primitive culture. In the same manner woman remains the owner of her tools, of her clothes and ornaments, and in many cases also of the hut or tent which it is her duty to erect. We possess information of this kind from Asiatic and African Pygmies, North Central Californians, Interior Salish tribes, Caribou Eskimo, Fuegians, and Southeast Australians.

In nearly all these tribes woman is also in possession of the highest kind of property, i.e., of her own person, to such a degree that she may freely dispose of herself, independent of the will of her parents or other persons; yielding herself to the young man who is equally willing and free to yield himself to her in order that by mating of their free and mutual love, they will furnish the firmest basis for a new family community.

Thus we may venture to say that in the most ancient times woman possessed the most extensive rights of property that she ever had in the history of human culture, and that, if there were limitations, they were identical with those of man: she enjoyed here as well equality of rights with the latter.

## II. WOMAN'S PROPERTY IN THE PRIMARY CULTURE OF ANIMAL BREEDING HERDERS

The situation radically changes when we now enter in some of the primary cultures, first that of animal breeding herders. The change begins to assert itself in the first stage, that of the reindeer breeders in northern Asia; it reaches its highest degree in that of the horse breeders and nomads of central Asia, and continues with some modifications, as it seems, among the cattle breeding herders of east Africa. Within all

three groups ownership of the soil is still more wanting than in primitive culture because even the idea of it is almost absent and in every case exceedingly fluctuating. But whatever kind of territorial property may exist here, not woman but man is the subject of it.

1. The new object of property in the reindeer breeding culture is, of course, the reindeer herds. Now, with all the four reindeer breeding peoples, the Samoyed, the Tungus, the Reindeer Koryak, and the Chukchee, woman may be the owner of reindeer.

Generally she receives reindeer as presents from her father and carries them with her when marrying; at the death of the father not only sons but daughters also inherit reindeer from him. Among the Samoyed she receives one reindeer from her husband at the birth of each of her children; her property is inherited principally by her sons, but some also by her daughters, and if she dies without leaving children it does not devise to her husband but to her own family. Among the Tungus all her clothes and implements, often the whole tent, always the beds, are her property; likewise all she acquires by the work of her hands. Her dowry—consisting of reindeer, horses, cattle, money, implements, clothes—remains as her property and is withdrawn by her in the case of divorce. It seems that among the Koryak and Chukchee also she remains the owner of her dowry.

With all that you may see that woman remains here still a true mistress; that she disposes of her property of her free will; and her faculty of being a mistress is especially recognized in her rights of inheriting even reindeer from her father. Among the Chukchee she may become even the principal heir, and it does not even matter that she is already married. It is quite possible that, as we may infer from other elements, this highest degree of evolution of woman's right is in some respects the result of more recent ("younger") matriarchal influences. But in the main the favorable development of woman's property rights in reindeer breeding culture seems to be inherent in this culture. Equally as in primitive culture this position of woman is crowned by her full independence in giving herself in marriage to whom her love impels her.

2. There is only one point, among the Samoyed, where another development begins to unfold: here the dowry does not remain in the possession of the woman but becomes the property of the husband. Now, just this change invariably takes place among all horse breeding peoples: the Turks, Yakut, Altaians, Kirgiz, Buryat, and Mongols. It is a quite solitary exception to this rule generally reigning among horse breeding peoples that among the Kirgiz and the Yakut, in the rare cases where the dowry is of higher value than the bride price, this surplus, and only this, re-

mains the property of the wife. That is the fateful beginning of quite a new situation for property rights of woman, radically opposed to her former position. This radical change may be summarized very shortly in the words: in horse breeding peoples woman becomes fundamentally incapable of ownership of objects of any sort; not only of horses and cattle, but also of her own implements, clothes and ornaments, and of the fruits of her own work. Likewise she is incapable of bequeathing and inheriting in any way whatsoever. It is an unheard of exception that among the Yakut her husband may bequeath to her the fourth part or even the totality of the property acquired by himself and she may transfer the fourth part of it to her parents or others.

Thus with regard to ownership of anything whatever, woman in horse breeding groups remains always minor and under guardianship: as a daughter under guardianship of her parents or brothers, as a wife under that of her husband. It is only as a widow that she is allowed to administer the fortune of her husband; even then only for the use of her children. If later on she prefers to return to her own family, her children remain with the family of her husband, and she may retain only some of her clothes.

The radical change produced here becomes intelligible if we consider that the whole economic progress characteristic of this culture, animal breeding, reaches its highest development in horse breeding, and has been brought about exclusively by man. Woman has not only contributed nothing to it, but lost moreover the possibility of making contribution by collecting vegetable food—which formed the basis of her property rights in primitive culture—because in the steppes there are no plants of importance to collect. And she is not even admitted as assistant in procuring animal food by cattle breeding, as for instance in milking mares and other animals.

3. If we now turn to the cattle breeding peoples of east Africa, the essential traits of this critical change in the proprietary right of woman remain the same even in cases where some kind of primitive horticulture is added. Some improvement is produced there only where considerable matriarchal influences are at work, as Prof Seligman has shown for such Hamitic tribes as the Beni Amer, the Barea-Kunama and some of the Nilotic or Niloto-Hamitic tribes, as the Bateso, the Bari-Kuku, and the Nandi. With all the other tribes—the Hamitic Galla and Somali, the Nilotic Dinka, Shilluk, Nuer, and Kavirondo, the Niloto-Hamitic Masai, Suk, Turkana and others—we invariably encounter the fixed law that woman is incapable of bequeathing and inheriting any objects whatsoever, even those of their own use and manufacture. We find the same situation

also among those Bantu peoples with strong Hamitic elements, such as the Banyankole, Banyoro, Baganda, and Baziba.

It is easy to understand that among all these peoples, where the right of woman to property is almost totally wiped out, she is no longer in possession of herself in the matter of choosing her partner for life. It is the father or brother, the head of the concentrated patriarchal family who gives the daughter or sister in marriage to whomever he believes useful in the interests of the greater family. The economic necessity of a highly centralized unity of this greater family is the idol to which the right of female ownership is sacrificed. There is little consolation in noting that the property rights of the male members have also been limited, but not to such a high degree, in favor of the patriarchal head of the family.

### III. WOMAN'S PROPERTY IN PRIMITIVE HORTICULTURE

Quite another development to that of the culture of animal breeding herders—almost the opposite, in fact—was realized in the culture of primitive horticulture. Here the rights of property of woman and her consequent social prerogative have reached their highest peak in the well known so-called matriarchy. We may distinguish economic and social sides in this phenomenon. It is the former from which the latter has sprung, but without knowing the latter the true nature of the former cannot be grasped.

1. There is especially one social detail of this phenomenon in which the historical development has found its clearest and therefore simplest expression. I have already given a short exposition of this point in my "*Völker und Kulturen*" (before 1924), when I described what I then called the developmental phases of mother-right. I am not surprised that this exposition has been ironically attacked by some of my benevolent critics, who were completely satisfied to find classical evolutionism, as they called it, in the work of one of the leaders of the historical school. In this sense also my friend Prof Lowie has quite recently taunted me in an article in the *AMERICAN ANTHROPOLOGIST* (Vol. 35, 1933, pp. 390 ff.), in which besides myself Prof Boas and Prof Dr Radcliffe-Brown were also amicably criticised for similar inconsistencies in their theories. I was quite amused to have caused such great consolation to my critics and, charitable as I am, I deeply regret to be obliged to deprive them of this consolation. It was only want of space that prevented me, when I wrote my book "*Völker und Kulturen*," from expounding more in detail these phases of mother-right and adducing all the positive evidence for them at my disposal. During the last year I have chosen as the subject of one of my lectures

delivered at the University of Vienna "Nature, Origin, and Development of Mother-right." It is of my researches on this subject that I undertake to give here a résumé, which, of course, cannot be but extremely brief.

We may consider it as one of the most firmly established results of modern historical ethnology that mother-right did not exist at the beginning of human society, but was preceded by a period of bilateral individual or greater family in which a natural division of economic work reigned: roughly speaking, animal food procured by man, vegetable food by woman, and it was woman who followed man to his residence and family when a new family was constituted. Thus, all sound explanations of the origin of mother-right must necessarily take into consideration and start from the nature of this preceding period. Or, while in the previous epoch there was not, as we have seen, any kind of individual ownership of the soil nor any kind of plant cultivation, we find these two important new elements everywhere among mother-right peoples and always intimately connected there with the woman to such a degree that they seem to constitute the essential parts of mother-right. Only in Australia and northwest America is this connection wanting; but as it is certain that mother-right did not originate in these two regions and spread from there, we may for the moment leave them aside. Now, while in the one primary culture of animal breeding herders the natural equilibrium and harmony of the previous, primitive culture was shifted to the masculine side to such a degree that woman lost all her proprietary rights, in the other primary culture, the plant cultivating culture, quite the contrary was effected. It was man who lost his proprietary rights: these were transferred to woman who, moreover, was crowned with a proprietary right of extreme importance and hitherto totally unknown, namely the individual proprietorship of the soil.

2. If, now, we seek to obtain insight into the way in which this highly astonishing revolution was produced, it is clear to us all that it is impossible to obtain direct positive evidence. But if ethnology is in so many cases forced by the nature of things to trust also to indirect evidence, then here is one of those cases in which reasonable doubt may not be entertained about the only possible way in which mother-right originated. That is, in the previous epoch only woman was entrusted with the task of procuring vegetable food, which, when collected by her, was her individual property: now when not only collected vegetable food, but also such plants as were obtained by cultivating the soil and with the plants the soil itself had become her property, then this change could have been possible only by reason of the circumstance that it was she who for the first time achieved

the marvelous progressive step of taking in her own hands the germination and growth of food-plants by sowing or inserting them into the soil and cultivating them, thus rendering the soil one of her working implements—tools having already been her personal property from the previous period. Thus she became the legitimate mistress of the soil and of all the products which her labor obtained from it. As it was solely woman's task to cultivate and woman's skill which accomplished it, there was no other possibility for perpetuating this marvelous forward step than to leave it to woman who thus became not only the legitimate mistress of the soil and its products, but also the sole competent inheritor of land and consequently of the home erected upon it. The house obtained here a quite new importance because it was the first fixed human habitation and was thus much more solidly constructed than the temporary huts of the primitive times.

It can easily be understood, that, while the very essence of this progressive step must have taken place in a relatively short time, the manifold consequences of it could only have followed step by step. The most interesting series of consequences was the manner in which man took up his position under this change, which was of such tremendous novelty for him. Now, the succession of measures that could be expected he underwent is to such a degree a natural, almost an inevitable one, that it is not aprioristic evolutionism, but quite logical deduction from the very nature of things and men, to arrange them in a certain series of phases of development: this moreover, is fully confirmed historically by the identity of geographical distribution and frequency of the same forms.

3. Thus, first, if in primitive culture woman left her residence and family at marriage and followed man to his home and family, now in the mother-right culture, she could no longer leave her country and tribe because she was bound to it by the ties of her land ownership, the basis of the whole economic structure. But at the same time man, not yet accustomed to the new situation, preserved his old one and remained within his tribe or group: marriage consisted only in more or less frequent visits to his wife in her country and family, a kind of visit-marriage (*Besuchs-Ehe*) as I propose to name it. Here the independence of woman in exercising her property rights is at its peak: her husband is nothing more than a "distinguished foreigner" and may not exercise even the slightest right over the property and faculties of his wife. Even his children do not belong to him but exclusively to his wife and are her only heirs. This kind of marriage is to be found among the Synteng-Khasi, the Dioi in Kueichou (?), the Cham (?), the Cheruman (Malabar), the Kudan (Malayalam).

the Mappila (Malabar), the Nayar (southern India), the Menangkabau of Padang, the Iroquois (North America), and the Seri Indians. As you see, it is only in southeastern Asia and its adjacent insular region that this form of marriage is to be found, except for the two cases in North America to which we shall return later.

As this kind of marriage preserved nothing of the common life of husband and wife of the natural family of the primitive culture, even in this new economic stage it was too unnatural to become a universal institution. With time man learned to recognize the economic superiority of woman and finally yielded to it by following his wife to her tribe and family, i.e., the patrilocal marriage of primitive culture changed into the matrilocal marriage of mother-right culture. Thus the community of family life was reestablished to a certain degree—but not the economic functions of man; the whole economic superiority of the wife was sustained; the husband continued to be a stranger even to his children; the wife remained the unrestrained mistress of the home, the garden and field and their products; and only daughters had the right to inherit from her. Man, when a boy, remained under the guardianship of his mother, and, when a husband, under that of his wife. This economic superiority of woman pervades the whole life and characteristically in religion, where Mother-Goddesses, especially the Mother-Earth, begin their development. Such is the state of things to be found in other tribes of the Khasi and among their neighbors, the Garo, the Lalung (?), the Koch; further among the Stieng, the Moi and the Cham in Farther India, the Idaiyan, Tiyan, Kottai-Vellala in southern India, the Menangkabau Malays of Indragiri in Sumatra, the Malays of Negri Sembilan in Malaya, the Dyak of northwest, central, southeastern and southwestern Borneo, the Tana Ai of Flores, the tribes of southern, central, and northern Celebes, the Melanesians of New Ireland, and southern Solomon Islands (Guadalcanar, Makira, Malaita) and Dobu (Papua). It thus appears that this kind of marriage has its distribution in the same regions as the earlier forms but also beyond them in the adjacent regions of Indonesia; in Melanesia and New Guinea it is to be found only sporadically, and is totally absent in Australia. We find the same kind of distribution in North America: in the Siouan tribes adjacent to the Iroquois, the Kansas, Osage, Mandan, Omaha (?), the Muskogean tribes in the Southeast, the Pueblo tribes and the Navaho in the Southwest, the Haida and some Déné tribes in the Northwest. In Central America it reigned perhaps among the ancient Maya and is to be found among the modern Kekchi and Bribri of Costa Rica. In South America it has its most compact distribution among the Carib and Arawak



tribes of the Antilles and the northwest coast; moreover it appears sporadically among the Pareci, the Caraya, the Bororo, and some Chaco tribes. As Dr Baumann in his valuable study "*Vaterrecht und Mutterrecht in Afrika*"<sup>3</sup> points out, matrilocal marriage

tritt in Afrika selbst in ausgesprochen reinen Verhältnissen selten genug in Erscheinung. Das hängt mit der Tatsache zusammen, dass die afrikanischen Mutterrechtler den Frauen nicht immer Besitz- und Eigentumsrecht zugestehen. Das Mutterrecht ist eben in Afrika entweder sehr stark geschwächt—was am wahrscheinlichsten ist—oder nie recht zur vollen konsequenten Entwicklung gekommen.

One of the most remarkable cases of matrilocal marriage in Africa seems to be that of the Tuareg.<sup>4</sup>

This form of mother-right, together with the earlier form of visit-marriage, represents the classical form and in many cases it reaches a veritable matriarchy. It has its widest and greatest distribution in southeastern Asia and in Indonesia, begins to vanish in Melanesia, and disappears entirely in Australia. It has sufficiently wide distribution in North America, a more feeble one in South America, and, curiously, a still more feeble one in Africa. One quite characteristic element of mother-right took its origin in this phase but disappeared in latter forms: wooing is done not by the youth but by the maiden. We find this curious expression of woman's superiority among the Garo, the Koch, the Cham, the Nayar, the Ulladan (Malayalam), the west Menangkabau Malays of Padang, the Dyak of northwest Borneo. Among the Dyak of southwest and southeast Borneo we encounter the variant that the daughter woos through her parents, and this form is to be found as well among the Carib and Arawak of the Antilles and the northwest coast of South America, while in other parts of South America and in Central and North America female or parental wooing seems to be absent. It is likewise wanting in Melanesia, New Guinea, Australia, and Africa, and belongs thus to the narrower circle of distribution of classical mother-right.

4. Already in some tribes and peoples of classical mother-right an institution begins to creep up which in its further development will bring about the decline of the matriarchal glory and change it with time into the worst female servitude, with the total extinction of woman's proprietary rights. In the classical phase of mother-right, the male element—principally in the person of the husband, but also, though to a less degree, in the person of the uterine brother—was wholly deprived of pro-

<sup>3</sup> *Zeitschrift für Ethnologie*, Vol. 58, 1926, p. 135

<sup>4</sup> *Ibid.*, p. 124.

proprietary rights. The latter (the brother) began to offer his services in the administration of the wealth and certainly there were some duties and tasks which could be better performed by men than by women. But this kind of help developed every day more into the function of guardianship and protection that relieved the sister of one proprietary function after another. This pushed her gradually farther into dangerous inactivity, to the degree that the brother finally controlled not only the whole wealth, but also the children of his sister. Toward these he fulfilled the function of the father, who for his part remained a stranger to them. With time it was no longer the daughters who inherited the fortune of the mother but only her sons. They inherited no longer from her but from their maternal uncle, and in this way mother-right became, so to say, masculinized. This form of mother-right already began to develop in some parts of the territory of classical mother-right, as among some agricultural castes of Southern India (the Bants, Izhava, Paraiyans, Bakuda-Holeya, Billava, and Nayar). But in wider and stronger distribution it is to be found at the margins of the classical mother-right territory and in regions widely separated from it: thus this form of debilitated and masculinized mother-right is characteristic of Melanesia, Micronesia, New Guinea, Australia, excepting New Ireland, southern Solomon Islands, and some tribes of southeast New Guinea. In North America it is to be infrequently found, as among the Choctaw, and the northwestern tribes, Tlingit, Haida and some Déné tribes (Western Déné, Tsetsaut), and in South America it has in every case so little importance that Paul Kirchhoff, in his valuable article "Die Verwandtschaftsorganisationen der Urwaldstämme Südamerikas,"<sup>5</sup> does not mention it. On the contrary, African mother-right is, in the overwhelming majority of cases, of this masculinized kind whose principal characteristic is inheritance by sister's son from the maternal uncle.<sup>6</sup>

This kind of masculinized mother-right bears something abnormal within itself, inasmuch as it deprives woman's soil-cultivating activity (which is the natural foundation of mother-right) of its essential fruit, the proprietary right of soil and earth, and transfers it to man. But this development may be called a natural one in so far as this usurper is the brother, himself a legitimate member of the matrilinear family. We thus have to do here only with the prepotency of the male's more robust gifts and egotism over female debility and modesty. In another way the male element begins to work upon mother-right when the husband, who may not always be a member of a matrilineal, but on the contrary of a patrilinear

<sup>5</sup> *Zeitschrift für Ethnologie*, Vol. 63, 1931, p. 85 ff.

<sup>6</sup> Cf. Baumann, *op. cit.*, pp. 104, 108 ff., 128 ff.

family with quite another ideal and tendency, enters the scene. Thus the struggle between the two opposed forms of marriage and family sets in and produces enormously varied forms of mixture between father-right and mother-right along several lines. There is first the contest between matrilocality and patrilocality when man contrives to diminish more and more the duration of matrilocality till it lasts only for the first year of marriage. An almost infinite number of various forms of "service-marriage," as it may be called on account of the service rendered by the young husband to the parents of his wife, takes its origin from this contest. We find such forms of blending at the frontier zone where patrilinear Indochinese tribes touch matrilinear Austro-asiatic tribes, or patriarchal eastern Dravidian tribes matriarchal southern and western Dravidian tribes, or younger patrilinear Indonesian tribes elder matrilinear Indonesian tribes in north and central Borneo and northeast Celebes. One curious form of blending is that of the Dobu (southeast New Guinea), where the couple takes its habitation one year with the parents of the wife, the other with that of the husband, and so on. Another line which this struggle between father-right and mother-right takes is concerned with the succession of children. Here we have interesting forms of marriage in Celebes and Flores, where the first child follows the mother, the second the father, and so on. A third line regulates the ownership of wealth and soil. There are curious forms of blending in Sumatra, Borneo, and Celebes, where husband and wife retains each the ownership of the wealth with which he entered on marriage; often a third kind of property is formed by the products of common work after marriage.

#### CONCLUSION

The number and quality of all these blended forms of marriage and family must be a puzzle for every one who does not know the original components of this mixture. It would be impossible for him to see the common fundamental elements through this variegated immensity of form and he could arrive at the end of long discussion at the astounding conclusion that matriarchy nowhere exists, merely mother-right, and that this is practically of no importance.<sup>7</sup> In quite the contrary sense we have seen that in the first phases of mother-right woman reached the highest degree in quantity and quality of proprietary rights and social and religious standing. This kind of oldest mother-right thus forms the historical counterbalance to the culture of animal breeding herders, where an ex-

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<sup>7</sup> J. H. Rontaar, *Women in primitive mother-right societies* (Groningen, La Haye, London, 1931), p. 506.

treme patrilinear development had deprived woman of all proprietary rights. Where the two different types of culture touched one another, the one was more or less absorbed by the other or an infinite variety of blending forms originated. On the whole, the matrilinear and matriarchal forms proved to be the more feeble, and very seldom succeeded in producing by themselves higher and highest civilization.

There remains still the task of showing what were the proprietary rights in the culture of the totemistic higher hunters. I am forced to confess that here the doctrine of *Kulturkreise* fails me. This is partly due to the regrettable fact that materials from pure totemic tribes are completely wanting or very obscure and defective, especially in Australia, Oceania, and America. If it is permissible to draw conclusions from the African materials, it would appear that inheritance by the filial brother is its characteristic symptom, because it seems to be warranted that the oldest totemic tribes have father-right, which is not favorable to female proprietary rights. As brother inheritance is ambivalent, insofar as in polygamous marriage it may mean the brother from the same father or from the same mother, it may be that the corroboration of the position of the mother's brother in the later phases of mother-right is at least partly due to the contact of matrilinear tribes with patrilinear totemic tribes having their form of brother-inheritance.

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## GENERIC DESCENT OF THE PAPAGO VILLAGES

By J. W. HOOVER

THE region of southern Arizona and northern Sonora, Mexico, occupied by the Papago Indians, was called Papagueria by the Spaniards. The name is again coming into use as the area is made to stand out in sharper contrast with the lines of settlement and civilization drawing more closely about it. Broadly considered, it is the area south of the Southern Pacific Railway and west of the Santa Cruz River, and north of the Altar Valley, Sonora. Disregarding several considerable outlying villages or groups such as those at San Xavier and in Tucson, the Papago villages are confined to that part of the area which is bounded on the east by the Baboquivari Mountain range and related low mountain groups extending northward. These also roughly bound the Papago Indian Reservation. On the north, white settlement is approaching the reservation boundary line. The western boundary is less definite, as in this direction the desert becomes increasingly inhospitable, merging into great precarious empty waste spaces, too barren to support even Papago.<sup>1</sup>

As part of the Basin Range Province, the Papagueria is characterized by flat detrital plains, interrupted by low fault block mountains which occupy the minor part of the area. The mountains are in an advanced stage of erosion, rugged and almost bare of soil and vegetation, due to aridity and spasmodic rainfall. The products of disintegration have been emptied onto the relatively depressed areas and spread out smoothly.

Both mountains and plains, or so-called valleys, have north-northwest by south-southeast axes, and the Papagueria lies open both to the north and to the south. So, northward the Papago villages blend into the Pima villages of the Gila River, interrupted by the wedge of white settlement extending from Florence on the Gila River westward through Casa Grande. And southward, they blend into the river villages of the Altar Valley, Sonora, now mostly Mexican. The Indians of the latter valley are really Pima as the Spanish padres made the distinction. The settled groups on the river flats wherever found were called Pima, and the somewhat migratory desert Indians were called Papago.

The eastern border land, the Santa Cruz Valley, was formerly occupied by the Sobaipuris, who like the Pima on the Gila and Altar Rivers, irrigated their fields on the flats of the Santa Cruz and San Pedro Rivers.

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<sup>1</sup> Godfrey Sykes, *The Camino del Diablo: with Notes on a Journey in 1925* (Geographical Review, Vol. 17, pp. 62-74, 1927); Eldred D. Wilson, *New Mountains in the Yuma Desert* (Geographical Review, Vol. 21, pp. 22-228, 1931).

It was the Sobaipuris for whom and by whom the Spanish missions were built. The picturesque and well preserved San Xavier Mission, about nine miles south of Tucson, was built at the largest village, Bac. The others were located at Guvavi, about nine miles north of Nogales, and at Tumacacori, midway between.

Fleeing before the ravaging Apache early in the nineteenth century, the Sabaipuris abandoned their villages and the missions, and took refuge among the Papago, with whom the tribe merged and lost its identity. The last one of the known Sobaipuri braves died in Tucson in March, 1932.<sup>2</sup> The present village at San Xavier was settled by Papago at a later date.

#### ECONOMIC ADJUSTMENTS TO THE LAND

Water, or rather its absence, is the critical factor or resource of the Papaguera; limiting in turn habitable sites where water may be had for domestic purposes; limiting the economically useful vegetation to short lived annuals and low scattered xerophytic perennials suited to limited grazing; severely restricting the tillable lands in spite of extensive areas of flat land with deep fertile soils of the lime accumulating type. The rainfall varies from less than five inches in the west to as much as eighteen inches or more in the Baboquivari Mountains.

In recompense, the aridity has afforded a degree of security, for the area has offered little to hostile marauding Indians, or even to the covetous white man. Few white families could make a living on as much of this land as may support an entire Papago village. The very poverty of his land has been the Papago's guarantee, and his peaceful record is in large part due to the fact that he has had comparatively little provocation or interference. A high degree of adaptation through centuries of adjustment, combined with a low standard of living, have made it possible for over 5,000 Papago to live on about 5,000 square miles of this region, desertic both in climate and resources.

The Papago are the premier dry farmers, producing wheat on driest lands where it is known to be successfully grown, with as little as five inches of rainfall or less.<sup>2a</sup> Some years the crop fails entirely, for the rains do not always come. But yields as high as twenty-five bushels to the acre have been grown. A good harvest will furnish wheat enough to last several years, and the Papago have learned the wisdom of keeping a reserve on hand.

<sup>2</sup> The Arizona Republic, March 16, 1932, p. 1.

<sup>2a</sup> Carl Sauer and Donald Brand, *Prehistoric Settlements of Sonora*, with Special Reference to Cerros de Trincheras (University of California Publications in Geography, Vol. 5, No. 3, 1931), espec p. 75.

After a crop failure they prepare for a new crop with as much care as if a harvest were assured.<sup>3</sup>

In the Papago country, dependable sources of water and land which could be tilled were seldom found close together. The Papago adjusted themselves by a compromise between settled village life and nomadism through inter-village migration, commonly between two villages. Those of one type were reserve villages located at the foot of the mountains or in the foothills where there were sources of water that could be relied on. The other villages were out on the open plains adjacent to the fields and were farming communities temporarily occupied. In recent years, with the digging of deep wells near the latter villages, their relative importance has been reversed and the inter-village migrations have lost their seasonal character.

#### THE OLD HEAD OR PARENT VILLAGES

The present day Papago villages may be traced back to about twelve common centers prior to 1860. Each of these represents a tribe which, spreading from the main village or pair of villages, has established new communities. Some of these parent villages no doubt sprang from a few of the very oldest villages as Kaka, Achi (Santa Rosa), Anegam, Tecolote, and Gue Va (Quitovac).<sup>4</sup> The tribal areas coincide fairly well with the larger units of plains or drainage basins, imperfectly set apart by mountain ranges or groups. The parent villages with their descendant villages are as follows:

1. *Tecolote*, on the Tecolote plain, south of the Quijotoa Mountains had as its companion or reserve village, Cobabi, Sonora. A second reserve village developed at Cabota, also south of the boundary. Tecolote was a rallying point against the Apache, but now its people are scattered in a number of small communities or rancherias on the surrounding plain, such as San Rafael or Papala-vakamukik, and San Ignacio, with Molonites on the western front. The old village site is now virtually abandoned. Tecolote Indians settled Vamori, Poso Verde, San Miguel, and Indian Oasis. They also make up in part the villages of Chulik, Topowa, and Kavolik. A part of the Vamori people came from near Caborca in Sonora.

2. *Kupk* (Copeka), with its descendant villages and people, occupies the Quijotoa plain. Its companion reserve village, San Antone and Kupk itself, have shrunk to insignificance. Kupk was parent to Kavolik, Pisinemo, and Haal Muik, with their reserve village, Poso Blanco; and in recent

<sup>3</sup> W. J. Spillman, *Extra-dry Farming* (Farm Journal, March, 1928, p. 22).

<sup>4</sup> See also Carl Lumholtz, *New Trails in Mexico* (New York, 1912), pp. 355-56.

years to the smaller villages, Comevo and Cheweeeton. Southward the plain extends into Sonora and includes San Francisco (Chuo Gûsh), with Hiaspak and Vapk.

3. *Gue-va* (corrupted to Kerwo or Cubo), with its descendant villages occupied the series of valleys with their north-south axes flanking the Quijotoa plain on its west side. Northward the descendant villages extend into the Poso Redonde Valley, westward over the Barajita Valley, and southward across the La Aquituni Valley or plain, with the most important villages bearing the same names. Still farther south, the plain east of Nariz Mountains includes Comoti, Arizona, and Sinunyui Poduk, Sonora, with Tatschak as its reserve village.

4. *Perigua* is the old village of the Perigua Valley. Its descendant villages are Road Runner, Emita, Toapit, with Poso Colorado, and Poso Redondo, in part, as reserve villages. These people are believed to have come originally from Kaka.

5. *Kaba*, with its emergency village, Moivaxia, south of the Sand Tank Mountains, is parent to the large village, Akchin, near Maricopa, and to Sil Mukik, near Gila Bend.

6. *Kuitak*, or Iron Pipe Village, in the lower or southern Santa Rosa Valley, was preceded by Vachk, the "Batqui" of Father Kino,<sup>5</sup> and is now almost extinct. It was the emergency village for Big Fields, to which it is now the most important center. It was also parent to the villages of the Cobabi Mountains and of the South Comobabi Mountains, viz., Cobabi, which is the oldest reserve village, San Luiz, Nolic, Santa Cruz, and Rincon; also to the villages at the north ends of the Baboquivera and Coyote Mountains, viz., Vafkuk (Babakuk), Coyote, Alamo, and San Pedro. Tusconcito and part of the population of Indian Oasis are also of this group. The major part of the Papago village in Tucson is from Big Fields.

7. *Santa Rosa*, whose predecessor was the older village Achi, is still an important center in the Santa Rosa Valley, but at times dwindles to several families. Its most important emergency village is Covered Wells. It is parent to the small communities to the south and east, and to the present large village, Bac, at the San Xavier mission.

8. *Akchin*, just south of Santa Rosa, had Comobabi as its reserve village. Later the other small villages of the North Comobabi Mountains sprung from it, and also Skoskonik.

<sup>5</sup> "Nuestro Senora de la Mersed del Batqui" (Kino's Historical Memoirs of Pimeria Alta, 1683-1711: edited and annotated by Herbert Eugene Bolton, 2 vols., Cleveland, 1919. Vol. 1, p. 208).



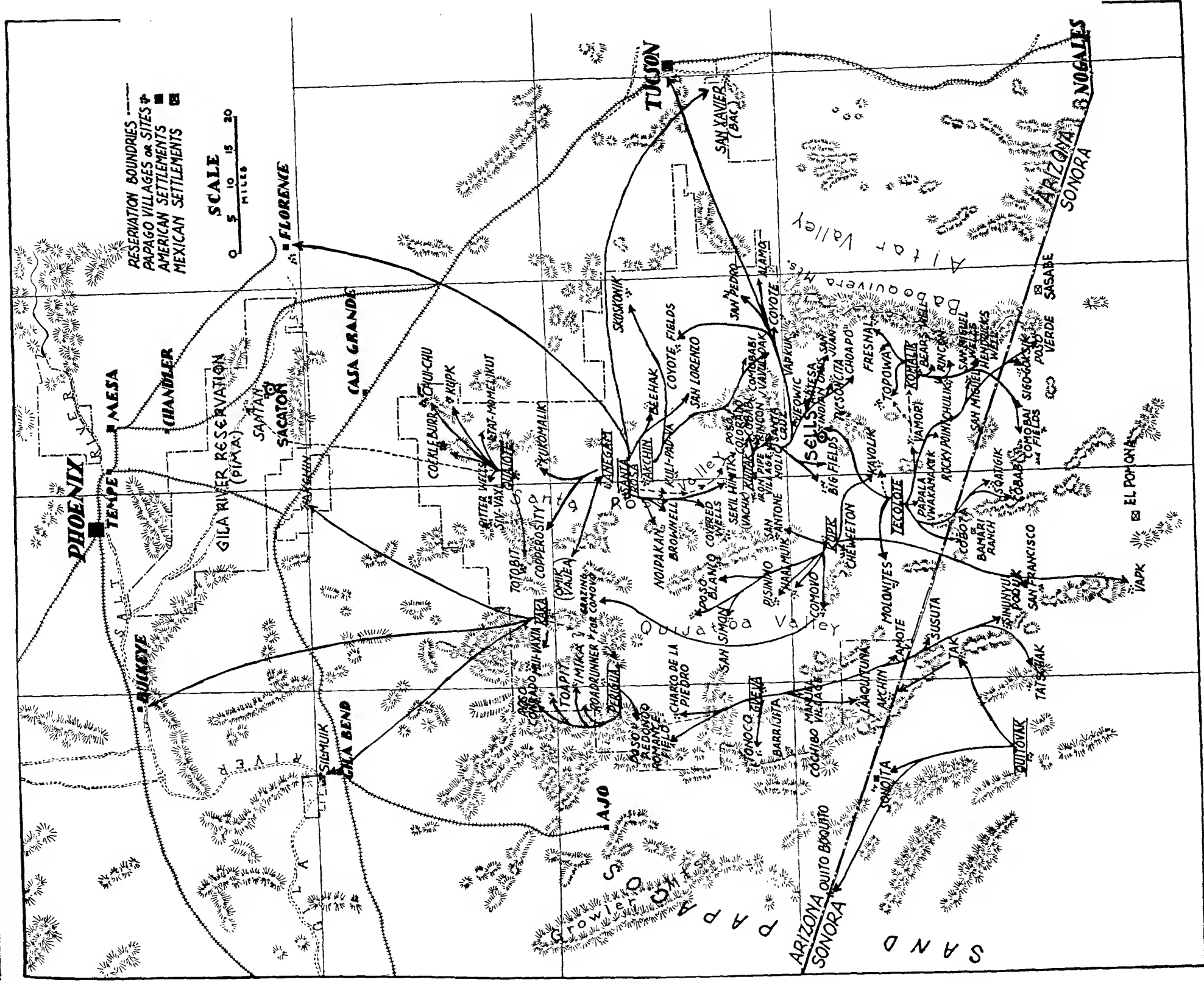


FIG. 1. Location and interrelation of Papago villages of southern Arizona and northern Sonora.



9. *Anegam*, of the upper Santa Rosa and tributary valleys, is the new site of the village Omik-vaxia (Salt Well). The latter was located on "the strip" withheld from the reservation in 1917. The Indians were at that time forced to move their village. With the inclusion of "the strip" into the reservation, some return movement may be expected. Copperosity (Chuopo) was the reserve village. Kukomalik was also settled by these people. Anegam is now one of the largest of all the villages.

10. *Quajote*, on the plain between the Slate and Vekol Mountains, with its reserve village, Bitter Wells, is parent to the villages northward, toward Casa Grande. Some of these people settled at Santan on the Gila River and on the Salt River, and are living among the Pima.

11. *Komalik* is the old Baboquivera Valley village. Its ancient reserve village was Comobabi, Sonora, but nearer reserve villages developed along the Baboquivera Range, the most important of which was Fresnal. They no longer trek to Comobabi, and this village together with Cobabi and the neighboring communities of the piedmont and valleys on the east side of the Sierra de Cobota, Sonora, although of independent origin, have, by virtue of geographical unity, propinquity, and the international boundary, become a distinct unit in Sonora.

12. The *Sand Papagos* have been the most nomadic of the Papago tribes. They occupy the truly arid region west of the Ajo Mountains. Rainfall diminishes toward the Gulf of California, where it averages as little as two and one-half inches a year.<sup>6</sup> The reservation does not extend beyond the Ajo Mountains, and there is no need for it, as the region is and must remain virtually uninhabited by whites except for the former copper mining camp at Ajo. The existence of these people depended upon their knowledge of the places in mountains where there are natural water tanks, such as Tinas Altas,<sup>7</sup> the Tule Tanks, and the Papago Tanks. The latter is a small oasis with an abundance of shade, grass, and a little ribbon of flat fertile valley.<sup>8</sup> For food the Sand Papagos ran down jack rabbits in the loose sand, killed mountain sheep, mule deer, and antelope with bows and arrows, and caught muskrats. They ate lizards, and at certain seasons went to the coast to fish and to obtain salt. Only a single agricultural site has been attributed to these people, southeast of Tinajas de Emilia in Pini-

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<sup>6</sup> Godfrey Sykes, *Rainfall Investigations in Arizona and Sonora by means of Long Period Rain Gauges* (Geographical Review, April, 1931, pp. 229-33).

<sup>7</sup> Kirk Bryan, *The Papago Country, Arizona: Geologic and Hydrologic Reconnaissance with a Guide to Desert Watering Places* (U. S. Geological Survey, Water Supply Paper 499, Washington, pp. 132-34).

<sup>8</sup> W. T. Hornaday, *Camp Fires on Desert and Lava* (New York, 1909), pp. 181-82, 235.

cate. Their vegetable food was largely the camote, an edible root found in the sand dunes, together with the beans of the mesquite and the fruit of the saguaro and pitahaya cacti, for which they came as far as Quitoboquito and the lower Sonoita River.<sup>9</sup> A few of them even now have temporary camps, as of old, south to the Gulf.<sup>10</sup>

*Quitovac*, the old head village for the Sonoita Valley, is Sand Papago in kinship. Descendant therefrom was the Papago village at Sonoita and the now extinct village of Tak and the nearly extinct village, Quitoboquito. Sonoita was an attractive and favored site, as the Sonoita River is here a permanent stream. The present village is composed mostly of Mexicans, numbering about 600, who crowded out the Papago. At present there are five Papago families with about sixty people at the lower end of the village.<sup>11</sup> Quitoboquito is a small oasis with one of the largest springs in the Papago country.<sup>12</sup> It never was but a small village,<sup>13</sup> and the population is now reduced to one half-breed family operating a cattle ranch. The spring is on the American side of the boundary line, but the fields and ranch are in Sonora.

Several small villages of Sand Papago are located around Ajo. They are a nomadic and very poor people, having small houses and a field here and there. The largest remnant of the Sand Papago is located at Dome and Blaisdell, about twenty miles east of Yuma. The population of the two villages totals about two hundred. They have fields along the Gila River and work as laborers on the Southern Pacific Railway.

#### FACTORS INFLUENCING SHIFTS OF PAPAGO VILLAGE SITES OR OF THEIR POPULATIONS

Several factors have contributed toward a disintegration of the older and larger villages. To begin with, a degree of concentration was a necessity for security, especially against Apache raids. As the Apache extended the range of their raiding activities over the Santa Cruz and Altar Valleys, the Papago abandoned all out-lying villages.

At this time the old village of Poso Verde, Sonora, was deserted and its people went to Comobabi, Sonora. The place became frequented by Apache bands. In 1874 the only evidences of habitation were the remains of a few bacquals and of an old adobe fort built by the Papago as a protection for their frontier village and grazing range. The "well" was described as a sort of pit or natural tank with a strong flavor of alkali, cow-manure, dead

<sup>9</sup> Lumholtz, pp. 330-31.

<sup>10</sup> A list of the former camps of the Sand Papagos is given by Lumholtz, pp. 394-97.

<sup>11</sup> A Papago family may include married offspring living under the same roof.

<sup>12</sup> Kirk Bryan, pp. 164-65.

<sup>13</sup> Hornaday, p. 122.

coyotes, and decayed vegetables.<sup>14</sup> Settled again from Tecolote, Poso Verde, situated in a semicircle of foot hills, is now one of the most pleasing of all the Papago villages.

The "trincheras" or walled fortifications on the isolated mesa four miles east of San Miguel were built by the Papago as a retreat in case of Apache raids. They are in this respect distinct from the prehistoric trincheras on the mount just south of Sells, near Poso Verde, and through the Altar Valley, described by Sauer and Brand.<sup>15</sup>

The most important factor in making new and smaller settlements desirable has been the introduction of cattle, dating from the journeys of Father Estubio Kino, 1691-1702, followed by those of Father Garcés, 1768-1775. Along with Christianity, Father Kino introduced cattle, horses, and chickens. Cattle did not become important with the Papago, however, until the latter part of the nineteenth century, and the extensive areas between their limited fields were of little use to them except as game preserves. By hunting and gathering in these areas, they supplemented the produce of their fields, but no inducement was offered to permanent settlement of these areas. In 1849-1850 John Audobon described the Papago as fast passing away, judging from the dilapidation of the towns and the number of empty houses.

The people live on turtles and what game they can get. I have seen small elk and antelope skins dressed and terrapin shells are everywhere. We have bought two terrapins fresh killed, some roots, and the fruit of a plant called the manguey.

Why it is that these Indians settle in such a country, I cannot conceive, for even the lizards, in most places innumerable, are scarce here. The Indians kill them with a light wand, giving them a dextrous tap on the head. They pick up the game, slip the head under a belt or string round their waists, and when sufficient are collected, a little fire is made, and this delicate repast is enjoyed by them as an epicure would relish his brace of woodcock.<sup>16</sup>

Audobon also described a sort of mush made of grasshoppers. The insects were caught and dried, then pounded and mixed with meal or pinole, parched wheat or corn, spiced and pounded.

Pastoralism is better suited to a scattered population, so the tendency has been for the larger villages to break up into more scattered settlements. The passing of the old Tecolote village in favor of smaller communi-

<sup>14</sup> J. Ross Browne, *Adventures in the Apache Country* (New York, 1874), p. 277.

<sup>15</sup> Carl Sauer and Donald Brand, *Prehistoric Settlements of Sonora, with Special Reference to Cerros de Trincheras* (University of California Publications in Geography, Vol. 5, No. 3, 1931), esp. pp. 67-70.

<sup>16</sup> Audobon's *Western Journal*, 1849-50 (Cleveland, 1906), pp. 148-50.

ties and ranches is a good illustration. Grazing has enabled the non-tillable lands to be put to use, and many of the Indians prefer to live on or near their grazing lands.

Other factors responsible for the passing of the old order have been the digging of wells by the government which have scattered more widely the sources of dependable water, the development of mining communities, and increasing contact with the white population.<sup>17</sup>

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<sup>17</sup> There has been considerable variation in the names and spelling of names of Papago villages as given by various writers. Some revision is suggested of the names used on the government maps published in connection with Water Supply Paper 499. The Indian names are given preference in the present study, except where corruptions of them, or Spanish and American names, have become a matter of common usage. The U. S. Geographic Board has as yet taken no action concerning these names.

Special acknowledgment is due Father Bonaventura Oblasser, Superior of Franciscan Missions among Desert Papagos, for the invaluable assistance of his experience among the Papago

THE ISLAND CARIBS  
OF DOMINICA, B.W.I.

By DOUGLAS TAYLOR

THE last remnant of the Island Caribs lives today in a reserved territory usually known as Salybia, extending some nine miles along the windward coast of the island of Dominica. The island forms part of the British Crown Colony of the Leeward Islands, having Antigua for its seat of Government and being governed through an Administrator. The Reserve itself was constituted in its present form by an announcement in the Official Gazette of July 4, 1903 which delimits the boundaries without making any mention of organization, rights, or privileges. Traditionally the Caribs have always had a Chief or Headman (Ubutu) whose duty it is, as leader, spokesman, and delegate, to act and make decisions in the general interest of all Caribs. For a time this office received a certain official recognition with remuneration at the rate of ten shillings per month, but this was withdrawn subsequent to a disturbance in September, 1930 when the police raided the Reserve and shot two Caribs suspected of smuggling. However, the then Chief, Jolly John, still continues to receive the same consideration and respect from his people, who even refused a substitute proposed by the Government.

There are supposed to be something less than five hundred souls at present residing in the Reserve. In my own estimation I do not consider more than one hundred fifty of these to be of reasonably pure Indian blood. It should also be borne in mind that the Carib invaders of these islands mixed freely with the womenfolk of their conquered enemies—in this case Arawaks.

As might be expected, the opening of a school, church and so forth in the last thirty or forty years have brought about the loss of nearly all that remained to the Caribs of traditional characteristics, customs, and language. Things are far, however, from being so irremediably lost as some people seem to believe. The Government Commissioners in their report on "Conditions in the Carib Reserve, and disturbance of 19th. September, 1930, Dominica" (H. M. Stationary Office, 1932), state: "At the present time one searches in vain for any trace of primitive customs or traditions. Not even the oldest inhabitant claims to remember a word of the Carib language (or languages)." How far this may or may not be true will be judged from the following meagre scraps, gleaned by me in the course of about half a dozen visits of from one to five days in the Reserve, made at long intervals between 1930 and 1934.

## CUSTOM, TRADITION, INDUSTRY

It is difficult at the present time to distinguish between those customs which belong to the Créole people in general, and those which are of genuine Carib origin. Most of the old ceremonies and beliefs seem to have come to an end with the Caribs' final conversion to Christianity, though there are signs that this conversion is even more superficial than is the case of the negro population. The marriage ceremony is usually forced upon them by the priest, and only when the girl has become pregnant. Most men—and women, too, for that matter—have several families, mine, thine, and ours!

I have heard vague stories of mankind having originally come out of the navel of Lougwo; of the man in the moon who got sent there because of misbehaving with his sister; and there is a common belief—though I know of no story connected with it—that the fou-fou (humming-bird) is particularly fond of leading people astray. The people who knew these legends have either forgotten half of them, or are afraid of ridicule or betrayal to the priest. Any such confidences as have been made me were always prefaced by "The old people were saying—but you must not tell it, or they will be saying I believe myself such nonsense. . . ." As for *piaïe*, I have never myself heard the word used. As a matter of fact, I believe that *obeah* or *quimbois* are less in use among the Caribs than with the negro population of the island. It is perhaps a coincidence, remarkable nevertheless, that the two words for West Indian sorcery of which one (*obeah*) is supposed to be of African origin, and the other corrupt French from *tiens bois* correspond to the Carib words of the women's and men's languages respectively for "spirit," "knowledge:" *ôpoya* and *akâmboué*.

The Caribs' houses conform more or less today to the general pattern of cabins all over the island; i.e., they are built for one family out of boards and shingles and consist generally of two rooms, the kitchen being in an outhouse. The Caribs' houses are all built upon piles, which is true of only a minority of the negroes' houses in the rest of the island. One distinctive feature is that every house has a more or less big mud courtyard behind it around which, apart from the kitchen, it is not uncommon to find low thatched shelters where the men work or the children sleep. These they call *muinan* (French pronunciation): I do not know where the word comes from. Some few Caribs have nothing else. I know of one old man who lives in such a one far from any other habitation together with his son, daughter, and grandchildren. In answer to my question he told me that the father of the children was his son, that the mother was his daughter. I did not dare ask him whose were the children: he might have told me they were his also!



In the last fifty years the Caribs have ceased to make bows and arrows, rattles, and hammocks. It would be of great help to them if it were possible to re-introduce hammock making. They still make double baskets, hébichets, and matapis of *Luarouman* and lattanier reeds. They seem to make less cassava than in the past, and depend more on roots—yams, tannias, dachine, etc. They no longer make wécou or any other drink, unless it be illicit rum! The children weave a sort of toy matapi, which they call attrape or wife-leaders. The main industry of the men is the making of dug-out canoes from the gommier or çibou tree. These are sold in Roseau for the ridiculous price of three dollars to be retailed in Martinique for from twenty-five to thirty-five dollars. They are made in exactly the same way as of old: hollowed out, the inside burnt, then stretched with cross-pieces of wood known as totes (from taotaca, “to support”). When the boat is ready to be brought from the forest slopes where it has been hewn, the same ceremony is gone through as described by Breton<sup>1</sup> and others: the neighbors are invited to coöperate, and do so to accompaniment of songs, and do not forget to come afterwards to get their reward in the shape of a feast of rum and food. The same thing applies to digging the foundations of a new house, except that this takes place at night by full moon.

A kind of matting used for the children to sleep on is still made by some of the older men from the stem of the balizier leaf.

#### LANGUAGE

It is impossible to deny the fact that as a means of conversation the Caribs' own language (or languages) is dead. Today their mother tongue is the Créole patois of the islands. Nevertheless, if we except the numerous words of undoubted Carib and Arawak origin in current English or patois used in the island of Dominica,<sup>2</sup> there still remains among many of the older men and women of the Reserve an hereditary smattering of the old language, some of which I have been able to record, and give here. I will mention afterwards what I consider to be its particular interest. For although all or at any rate most of the words are to be found in dictionaries, it is always interesting to note the last survivors, and especially their use and pronunciation.

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<sup>1</sup> R. P. Raymond Breton, *Dictionnaire caraïbe-françois meslé de quantité de remarques historiques pour l'esclaircissement de la langue* (Auxerre, 1665).

<sup>2</sup> Some of the many Créole words of Carib origin commonly used in the islands, at least in Dominica are: anoli, canàri, cabouya, mabouya, balaou, coulirou, titiri, pipiri, calalou, siric, carapa, hébichet, tote, etc.

The vowels are here given their French values. The consonants are to be pronounced as in English, with the following exceptions: *ch*, as *ch* in machine; *kh*, as *ch* in the Scottish word *loch*; *ç*, as a sibilant *s* which sometimes approaches but definitely falls short of the *ch* sound. I have apposed an interrogation where I have failed to trace a word in other vocabularies. Accents denote stress.<sup>3</sup>

#### *Persons.*

- carifoùna: Carib (Breton, Callipouna, women's language)  
 mékerou: Negro (? cf. Carib, Mékou and Portuguese, macaco, "monkey")  
 càbourou: Mulatto (? cf. Créole, câbre, ? cf. cabara, "goat")  
 bôcouçili: thy father  
 bôcouçourou: thy mother (both women's language)  
 limétamourou: his father-in-law  
 limétsi: his mother-in-law (Breton gives no *s* after *t*)  
 libàmoui: his brother-in-law (Breton says this means "married cousins") (all three common to both sexes)  
 nàtari: a pretty girl (cannot trace this, unless it has to do with àtaricoua, "to violate")  
 noubouïào-yeni ?: your enemy (my informant always laughed over this word: I do not know whether he invented it or no. Possible connection with noubi, "deformed")

#### *Body.*

- nacou: my eye(s) (women's language)  
 naricae: my ear(s)  
 nîchiri: my nose (both common, or men's language)  
 nrouma: my mouth  
 nougouti: my foot  
 nîti'bouri: my hair (all three women's language)  
 nourâcae: my belly (cf. oule, "edible roots" and acae, "a pot") (common language)  
 boucouthi ?: tooth, but probably wrong (iépa, nari)

#### *Numerals.*

- aban, biama, éroua, biambouri: one, two, three, four

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<sup>3</sup> Mr Taylor writes "As regards the [small] capitals used in transcription of native words, they are meant only to indicate the beginning of the word proper as distinct from the nominal prefix attached, as *trouma*, 'her mouth'—Editor

*Animals.*

- Aoli: dog  
 picouri: agouti (both common language)  
 méçou: cat  
 Bouirikhou: pig (Breton, bouiroucou)  
 Bakhri ?: crab (cannot find this in any vocabulary)  
 yeléou ?: siric, present name for a kind of land-crab (possibly confused  
 with Breton's keléou, "man-devouring fish")  
 oûtou: fish (generic; Breton gives aoto)  
 courouné: red-snapper (kind of fish, locally "grand'-gueule")  
 máwali: vive (kind of fish)  
 ouatànoulou: flying-fish  
 ouàçou ?: crayfish (but Breton gives ichoulou)  
 yàboura: heron (of kind known as crabier) (women's language)  
 katouri: chat-huant (? night-jar)  
 kayou: common hen  
 héhué: tête-chien (kind of snake, but Breton gives it as a generic term)

*Elements.*

- ouàtou: fire (common language)  
 toùnè: water (pronounced with an indefinite sound at end as er in  
 English word water) (common language)  
 hoùya: rain (women's language)  
 nônum: moon (last syllable nasalized like French un) (men's language)  
 kaçi: sun (huéyou) (women's language)

*Various.*

- youri: tobacco (word still commonly known in Salybia)  
 nouni ?: food (but cannot trace this word, unless cf. ouri, "woman's  
 breast")  
 màmba: honey (common language)  
 bakoùkou: bananas (fruit and tree of short fat variety)  
 couaik: choux (meaning tannia; but Breton gives taya and ouàheu)  
 acæ: a pot (a common word applied to many things, a vessel)  
 saoùterou: a cauldron (surely from chaudière)  
 ibitarrou: a line (for fishing, etc.)  
 noucouni: my boat (men's language)

*Qualities.*

- çemêhéenti: (it is) good (as to the senses, I was told)  
 iropônti: (it is) good (as to fitness)

yeheùmeti: (it is) bad  
 bivemeti: (it is) sweet

*Old Place Names in the Reserve.*

Bàraiçi: (now) Bataca  
 wàicimà: a valley sometimes still so known

*Phrases.*

These were difficult to take down as my informant, parrot-like, was usually himself unable to distinguish between one word and the next.

1. Ennaï tàbouă nà(n)kou: I am going to sleep (literally, shut my eyes). The (n) of nàcou indicates a nasal quality in the preceding à.
2. Akàoua niàbou: I am going to bathe
3. Tiàka niàbou: I am going to fish
4. Bàyou-boùka: Go away!
5. Kàïman wai-boùka: Come on! Let's go!
6. Kàïma' alliàgwa: Let us go and copulate
7. (mia?) lamahàtina: I am hungry
8. Mékerou kehééntsi: The negro smells bad
9. En àtakwa: Let's drink
10. Makarahàtina: I am thirsty
11. Roubài takara touna (mi?) àtakwa: Give me some water to drink
12. Màbrika! Yourakhào (your-hào?) kàtou karahi?: Greeting! How are you? (see below)
13. Itènia (?itèlia) karahi: I am well
14. Roubài pàipaté poumianoùti coumoulakha: Give me a pipe, I want to smoke (see below)
15. Itènké karamàti bounouhàri makài: Thank God for having eaten well
16. Bién boéré kapabínou: Give me some rum (see below)

Except for the word ennaï in No. 1 and the word mia in No. 7, the first ten phrases are quite comprehensible to any one who studies an Island Carib vocabulary such as Breton's. Not so the other six. In No. 11 the word takara seems to be related to the word for thirst in No. 10, but just how is not clear. No. 12 is interesting: Breton gives nothing like it. For a like meaning he has: Attouatiéntibou (plural, -tiheu)? with the answer, àttouati; which apparently means "Are you hardy?" The words yourakhào or your-hào and karahi are puzzling. The word katou is used to form a kind of affirmative question, and therefore precludes the possibility of karahi being taken for -enraheù, the second plural interrogative ending,

"are you?" The only other possible meaning that I can find for *karahi* is another word *k-araheù*, "to be prolific, fertile," "to conceive." As for *yourak-hào*, I can only suggest *youlikwa-hào catou*, "are you lacking?" or *youlouca-hào catou*, "are you satisfied, content?" The latter accords better with what I have written down. In either case, if I am right, the question comes to this: "Are you prolific enough?" which is certainly an odd way of inquiring after a person's health! It would be interesting to know if there is any parallel in other Indian dialects. *Itèlia* seems to be the same as *itâlê*, a form of *inâlê*, "it is thus, it is true." No. 4 is interesting: I have never come across the word *païpaté*, which is probably borrowed; *poumianouti* may be a transposition for *pougnoumouti*, if it expresses a wish. No. 15 seems to be corrupt: *itânkê karabàti* might mean "to return thanks" but the other two words are incomprehensible to me unless they be meant for *ibonhàli-lam acàé*. No. 16 is probably meant for *Ibonam boéré cababinê*.

There are certain general differences between the words—mostly of the women's language—as I heard them spoken, and those given by Father Raymond Breton. One is that *r* is often used where Breton gives *l*: *éroua* for *éloua*, *noûracaé* for *noûlacaé*, *youri* for *youli*, *mabrica* for *mabouica*. This is the more remarkable as the general Créole tendency is to suppress the *r* and sometimes to substitute another sound. Moreover Breton states, and it is still true, that the Caribs of the mainland often use *r* where the islanders pronounced *l*. Another sound which Breton turns into *ch*, and which I have written *ç*, is certainly more like a sibilant *s* as it is spoken now, though there is no lack of *ch* sounds in the patois. Another peculiarity is the turning of *o* into *ou* (broad *u*): as *touna* for *tona*, *houya* for *oya*. Again, there is a definite nasalization of some of the vowels which is easy enough to indicate in French but is confusing unless it is clearly indicated: such is the case with *nounum* or *nônum* where the final *um* is merely a nasal form of the short *a*. So also with the word for "die" or "dead" which is still known to many Dominica Caribs although I forgot to write it down—*à(n)whé*. Another point worthy of notice is the guttural *c* as in *koumou-làkha* (which Breton writes *comôlaca*), a sound which exists neither in French nor in the Créole patois. The words are all very strongly stressed, and it is easy to miss an unaccented syllable.

I have taken the above small vocabulary down by ear to the best of my ability and without any subsequent alteration to accord words with those in other vocabularies. Scant as it is, and no doubt corrupt in places, I think it offers some points of interest. My chief informant is a man of about fifty who, like all the others of his age, can neither read nor write.

He has never left the island, and has never been in contact with any outsider who knew anything of the language. Both he and others have told me that in their youth it was not uncommon to find older people who spoke it naturally as their mother tongue. It would probably be possible, with time and patience, to collect a very much larger vocabulary from this man and others, particularly from old women.

Interbreeding and the dying out of the old ways seems to be inevitable; but it might be possible by encouragement and judicious help to stem the tide. Unfortunately, no one in the island takes the slightest interest in these, the last of the first-found American people.

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## THE STATUS OF THE HERMAPHRODITE AND TRANVESTITE IN NAVAHO CULTURE<sup>1</sup>

By W. W. HILL

UNLIKE our own society, many primitive societies recognize in a social sense, and include in their culture pattern a place for those individuals whose psychic or physiological peculiarities set them apart from the normal. The present article is concerned with the hermaphrodite and transvestite among the Navaho; their social recognition and the opportunities given these people to capitalize on an irregularity. To a lesser extent the individual adjustment to these cultural opportunities will be discussed.

The Navaho term for both hermaphrodite and transvestite is *nadle*, which I was told meant "weaver" but according to Dr Edward Sapir can be etymologized as "being transformed." However, they distinguish between the two and between male and female transvestites. The hermaphrodites were called "the real *nadle*." "You can tell them when they are born." The transvestites were called "those who pretend to be *nadle*." "A boy may act like a girl until he is eighteen or twenty-five; then he may turn into a man or he may not. Girls do the same thing." Male and female transvestites were about equal in number. Culturally the status of both hermaphrodite and transvestite is the same and the following description, except where the individual is discussed, applies to both.

During the past year I was told by various informants that there are today at least six *nadle* living on the reservation. The two best known are *kla* at Newcombe (Nava), New Mexico, and *kinipai* at Buck's Store, New Mexico. The former is a well known chanter. He is described as having a voice like a woman and doing a woman's work, but dressing sometimes as a woman and sometimes as a man. His grandfather is also said to have been a *nadle*. *Kinipai*, according to her own and other testimony, is hermaphrodite. She has masculine looking hands and shoulders and a rather masculine face. Her voice is that of a woman. Her hips are well developed but her bust only moderately so. The four other *nadle* noted by informants live respectively in the vicinity of Crown Point, New Mexico, and White Cone, Tuba City, and Polacca, Arizona. According to informants those living at Crown Point and White Cone are transvestites.

The concept of the *nadle* is well formulated and his cultural rôle well substantiated in the mythology. In the tales dealing with the creation and

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<sup>1</sup> The material contained in this article was gathered incidentally to research on Navaho material culture as a Fellow of the National Research Council

emergence of the Navaho the pursuits and activities of the nadle are outlined. They are described as wealthy and as having control of all wealth. In that part of the Emergence Myth which tells of the quarrel between men and women, they play a very prominent part. In this dispute the nadle cast their lot with the men when the sexes separate. Because of the ability of the nadle to perform the functions and duties of women as well as men, they make it possible for the men to overcome the women.

The outlook of Navaho society toward the nadle is very favorable. They are believed to have been given charge of the wealth in the beginning and to control it to the present day. The family which counted a transvestite among its members or had a hermaphrodite child born to them was considered by themselves and everyone else as very fortunate.<sup>2</sup> The success and wealth of such a family was believed to be assured. Special care was taken in the raising of such children and they were afforded favoritism not shown to other children of the family.

As they grew older and assumed the character of nadle, this solicitude and respect increased, not only on the part of their families but from the community as a whole. This feeling is very real. All the older Navaho have a genuine respect for the nadle and only in rare instances do the younger ones scoff at them.<sup>3</sup> They were never made fun of and their abnormalities were never mentioned to them or by themselves. This respect verges almost on reverence in many cases. A few quotations from various informants will serve to make this attitude clearer. One states, "They know everything. They can do both the work of a man and a woman. I think when all the nadle are gone, that it will be the end of the Navaho." Another says, "If there were no nadle, the country would change. They are responsible for all the wealth in the country. If there were no more left, the horses, sheep, and Navaho would all go. They are leaders just like President Roosevelt." A third says, "A nadle around the hogan will bring good luck and riches;" a fourth that, "They have charge of all the riches. It does a great deal for the country if you have a nadle around;" and a fifth, "You must respect a nadle. They are, somehow, sacred and holy."

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<sup>2</sup> This concept is also found in another phase of culture. The genitals of hermaphrodite deer, antelope, mountain sheep, and sheep are rubbed on the ends of the tails of female sheep and goats and on the noses of male sheep and goats. This is believed to cause hermaphrodite sheep and goats to be born. Hermaphrodite sheep and goats were never killed, but were allowed to remain with the flocks. "Then you will have many sheep and grow rich" Rennit from the stomachs of hermaphrodite animals was also rubbed on sheep to make them grow large and to increase their milk.

<sup>3</sup> Albert Sandoval, a Navaho of Lukachukai, who read this manuscript, says that the nadle are not so much respected nowadays. The older attitude is giving way to one of ridicule. Any child showing a tendency to a transformation is discouraged.



The economic rôle of the nadle is dual, their activities overlapping both those of men and women. They are given unusual opportunities for material advancement. They quite generally act as head of the family and are given the control and disposal of all the property.<sup>4</sup> They supervise the work of the women around the hogan and direct the planting and the work in the fields. At large ceremonial gatherings they are placed in charge of preparation and cooking of the food. Beside these general domestic duties, they knit, tan hides, make moccasins, are said to be excellent sheep raisers, and excel as weavers, potters, and basket makers. The last three pursuits contribute substantially to their wealth, as especially are basketry and pottery making restricted technics and they are able to trade these products extensively with their own and other peoples. The only masculine activities from which they are barred are war and hunting.

In the realm of religion and ceremony, the nadle also actively participate and are on a parity with anyone. While all informants agreed that there was nothing about a nadle that made him better qualified to practice curing ceremonies than an ordinary Navaho, the fact remains that most of them excelled in the performance of one or more of these rituals. As mentioned before, the one at Newcombe is a well known chanter. The transvestite at White Cone is one of the few remaining Hail Way chanters. Kinipai at Buck's Store knows the chants for curing insanity resulting from incest, and for curing body sores, and is noted as a midwife. It is also current gossip that she practices witchcraft, of which my interpreter was thoroughly convinced.

The social status of the nadle, while well defined, gives ample opportunity for individual expression. Hermaphrodites usually dress as women and assume the position of women when sitting. Transvestites wear the garb of either sex. Both care for their bodily wants after the manner of men or women depending on the attire they are wearing.

If they dance they assume the woman's rôle for the occasion and joke with the men as women would. In address, the polite person always calls them by the kinship term used for a woman of their relationship and age to the speaker. Their legal status is also that of a woman. The blood payment for the murder of a nadle is the same as that for a woman, which is higher than that required when a man is killed.

Their political power seems limited to an advisory capacity. In case of disagreements between men and women they act as mediators and are also employed as go-betweens in the affairs of the younger people.

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<sup>4</sup> This seems well established. Kinipai sold me two articles which were the personal property of two women in her family. In all other cases I encountered, the right of disposal of personal property has been strictly observed even though the owner was a child.

No stigma is placed on the irregular sex activities of the nadle. The usual tabus placed on abnormal sex relations by normal individuals are lifted in the case of the nadle and their promiscuity is respected rather than censured. "You make fun of a prostitute, but you do not make fun of a nadle, you respect them." Hermaphrodites are said not to marry. Transvestites are known to marry both people of the same and opposite sexes. When they marry, they take the garb of a man and do a man's work. "If they marry men, it is just like two men working together." There is one account of a divorce between a female transvestite and a normal person, and one was known to have borne a child. Transvestites had sex relations both normally and unnaturally with both sexes. Hermaphrodites appear to have sex relations only with men. Sodomy with a nadle is countenanced by the culture and the insanity believed to follow such an act with a normal person does not occur if the relation is with a nadle. Two informants stated that the nadle commonly paid the other person to perform the act. The only limit to these relations is that the clan incest tabu must be observed.

From the preceding sketch of the nadle in Navaho culture it is plain that they enjoy more opportunities for personal and material gratification than the ordinary individual. They are respected and to some extent revered. Their economic advantages are such that it is easy for them to justify the belief that they bring with them and control wealth. In religion and society they enjoy as much, if not more, opportunity and protection than the more normal member of the society. In sex expression they are given the absolute sanction of the culture. In fact every opportunity for personal adjustment to the culture is given. If, however, the individual nadle is studied, there is evidence that in some cases no advantage has been taken of these opportunities.

The writer is primarily interested in the technological aspects of Navaho culture. It was in the course of this research that the hermaphrodite kinipai, who is one of the few remaining potters and basket makers among the Navaho, was used as an informant. As she was well known as a nadle the interpreter was instructed before the work began to be alert for any behavior which differed from the cultural norm. The conditions under which the work was done were favorable. The interpreter's family are friends of the informant, and as had been my previous experience, as soon as the informant was convinced that the interest was in the practical aspects of the culture and not in esoteric ritual, the information was given cheerfully and readily.

Six days were consumed in the questioning. Whenever possible the

questions were put in such a way as to encourage answers and explanations to take the form of a personal narrative, and leads were also given which would bring up the personal element. However, all the informant's answers were impersonal and circumspect, and she refused all leads which might



FIG. 1. Kinipai, Buck's Store, New Mexico.

lead to a personal discussion. After six days without any results, this indirect method was abandoned and the interpreter questioned her directly on the subject of the nadle. The result was that the informant gave instant evidence of acute emotional distress. She was visibly upset, very nervous, kept her eyes on the ground during the whole recital, kept rubbing her hands together, and squirming. She lost her voice completely for a few

moments and when she began to talk, spoke in a whisper, and her accounts and answers were so incoherent that the interpreter had trouble in getting the sense and was forced to question her repeatedly.

The fragmentary account which she gave checked very well with the information given about *nadle* by other informants. She recounted the myth of the separation of men and women, and the part that the *nadle* played in mythical times. She then gave a short sketch of her own life.

A family that has a *nadle* born into it will be brought riches and success by that *nadle*. A person like that will be like a head of the family. Even now I have charge of everything that my family owns. I hope that I will be that way until I die. Riches do not just come to you: you have to pray for what you get. When I was young my father and mother and grandfather took special care of me. I am a hermaphrodite. As I told you, I have had relations with more than a hundred men. My parents always took better care of me than they did of my sisters. I have been happier than my sisters. The family, after I grew up, always gave me the choice of whatever they had.

Besides the anxiety registered at the direct questioning, several other things came up during the course of the work which were outside the norm of Navaho behavior. Kinipai's lack of reticence on questions concerning sex was unusual. She, on three occasions, brought up the subject of her own accord and was cheerfully frank about her own experiences and those of others. She even joked the writer about his experience. This, to my knowledge, is very unusual. While the Navaho have always answered in a matter of fact manner questions on such subjects, neither men nor women have ever joked about the subject or referred to it of their own volition.

Another curious piece of behavior was kinipai's willingness to discuss the subject of witchcraft. Most Navaho are uncomfortable when being quizzed on this subject for fear of being suspected of the practice. However, kinipai, who certainly must have known that she was suspected of practicing witchcraft, gave a fairly full account.

On the other hand, she gave further evidence of maladjustment to the culture by her refusal to discuss or do certain things which would have been done or talked about by an ordinary Navaho without question.

One of these was the subject of dreams. She admitted dreaming but refused to tell her dreams or to discuss dreams in general. The question obviously upset her. This is rather unusual: though the average Navaho may think such a question silly, he is always willing to tell and discuss his own dreams.

In the matter of being photographed, once again her behavior was hardly normal. Most Navaho have no objection to having their pictures

taken, unless it is a monetary one. Kinipai was extremely reluctant to have hers taken, telling the interpreter that she thought that I only wanted the picture to make fun of her. When she did acquiesce she would not be photographed standing, but only in the sitting position of a woman and surrounded by her pottery.

One other inconsistency in behavior was noted. While the informant usually addressed her relatives by the kinship terms used by a woman speaking, she on several occasions, when talking of her sister's daughter, used the male speaker's term of address.

Some insight into her feelings was also obtained from a distant relative of kinipai's, who was used as an informant. He stated that kinipai always addressed him as older brother and that he called her younger sister. "She sure likes that. When her uncles or nephews come over they call her mother. She likes that. Some people call her mother's brother. She does not like that."

As to the personal adjustment of other nadle to their culture, I was able to obtain very little information. One informant stated that "in recent times some of the school boys made fun of the woman's dress of kla at Newcombe, and he put on his pants." I was also told that kla has rationalized the status of the hermaphrodite diety of the Navaho into the position of a supreme god; a concept denied by all other informants. The statement about nadle generally that "you do not mention to a person that they are made nadle because you respect them, and that they do not mention that they are nadle because they respect themselves," would lead one to the inference that this is another rationalization to protect sensitive feelings.

There is no doubt that kinipai has failed to make the personal adjustment which her culture makes possible. This appears also to be true in the case of kla and is probably also true of others.

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MICHABO THE GREAT HARE: A PATRON  
OF THE HOPEWELL MOUND  
SETTLEMENT

By CHARLES C. WILLOUGHBY

MICHABO<sup>1</sup> the Great Hare, the Algonquian culture hero, was the creator of the world and the impersonation of life. He was reputed to possess not only the power to live but also of creating life in others. He was chief of all the animals and in ancient times caused man to be born from the dead bodies of the first of these who died, thus giving rise to the widespread belief among these Indians that they had their being from animals. He impersonated life in an unlimited series of diverse personalities. He was life struggling with many forms of want, misfortune, and death that come to the bodies and beings of nature. In his journeyings over the earth he destroyed many ferocious monsters of land and water whose continued existence would have placed in jeopardy the fate of the people. One of these monsters was the Great Horned Serpent. The fossil bones of extinct animals, occasionally brought to light, are said to be the remains of monsters destroyed by the Great Hare.

He placed beneficent humanized beings (the four winds?) one at each of the four cardinal points or world-quarters to aid in promoting the welfare of the human race. The one at the east supplies light and starts the sun on its daily journey; the one on the south supplies warmth, heat, and dews causing the growth of corn, beans, and squashes, also herbs and shrubs which bear fruit; the one at the west sends cooling and life-giving showers; and the one at the north supplies snow and ice, enabling the tracking and successful pursuit of wild animals. Under the care of the man-being of the south lesser beings were placed, dominantly bird-like in form.<sup>2</sup>

Throughout the entire Algonquian area these Indians never tired of gathering around their camp-fires and repeating the stories of Michabo. He was the highest divinity recognized by them, powerful and beneficent beyond all others. Traditions relating to him seem to have been universal among the Indians of this stock. Some of the stories are also found among certain tribes of neighboring stocks who doubtless obtained them from the Algonquians. He was sometimes called the Great Light, the Spirit of Light,

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<sup>1</sup> Michabo, Nanabozho, Manibojou, Messou, Moshop, etc., all refer to the same being.

<sup>2</sup> The above is taken largely from the article by J. N. B. Hewitt, in *Bulletin, Bureau of American Ethnology*, 30, Pt. 2, pp. 19-23, and from Daniel G. Brinton's *Myths of the New World*.

or the Great White One. He was the loyal friend and patron of the human race. It is significant that in the Peyote ceremony of the later Arapaho, introduced to the Winnebago, the Great Hare is identified with Christ.<sup>3</sup> There are many scores of stories current among the Algonquians in which Michabo took a leading part. As one would expect in such a mass of material these stories are often illogical or contradictory.

In studying the archaeology of the Algonquian region one is surprised to find so few effigies or other representations which can be positively identified as portraying this deity. Dr Brinton, writing of the Delaware, says these Indians rarely attempt to set forth the divinity in image. The rude representation of the human head cut in wood, small enough to be carried on the person, or life size on a post was their only idol. These small wooden effigy heads mentioned by Brinton are probably analogous to the little stone heads not uncommon in the Northeast, which are usually furnished with a groove or a perforation for suspension and were apparently worn as gorgets. These may personify Michabo or some of the spirits of his pantheon. None of them, however, that I have seen has a distinguishing mark which seems to identify it with any physical characteristic of the Hare. Dr Speck mentions twelve carved face images which hung in the interior of the ceremonial house of the Delaware Indians, those on the center pole being symbols of the supreme power. There were also three on the upright posts of the north wall and three on the south wall which represented the manitos or spirits of the north and south; those on the eastern and western doorposts symbolized the manitos of the east and west. Similar carved faces are figured by Harrington<sup>4</sup> in his description of the ceremonial house of these Indians.

I have thus far been able to identify but two representations of Michabo which show him in his traditional character of the Great Hare, both of which are from the large mound of the Hopewell Group, Ross County, Ohio. The first of these is the beautifully executed design surrounding a bone tube made from a section of a human femur. This was found by Dr Moorehead beside the skull of a skeleton during his exploration of this mound in 1891. A pair of curious ear pendants lay beside the neck. They were made from the lips of large helmet or conch shells, and were like one in the lobe of the ear of an ivory effigy of a human figure which was taken from an altar of this mound during the above exploration.

The tube itself is shown in Figure 1*a*, and the developed design in *b*.

<sup>3</sup> Paul Radin, in *Thirty-seventh Annual Report, Bureau of American Ethnology*, p. 376.

<sup>4</sup> *Religion and Ceremonies of the Lenape* (Indian Notes and Monographs, Museum of the American Indian, No. 19).

The principal units making up the composite picture appear in *c-g*. The most prominent single feature shows the hare's head with its conspicuous ears, *c*. The personified portion of the design appears in *d*. This represents a human head wearing a headdress of deer antlers which is an indication of rank. Among the Algonquians antler headdresses were worn by certain priests and perhaps by other notables. Combined with the mature antlers in the drawing is a pair of immature or sprouting horns which are indicated more clearly in *e*. It is interesting that two headdresses made of native

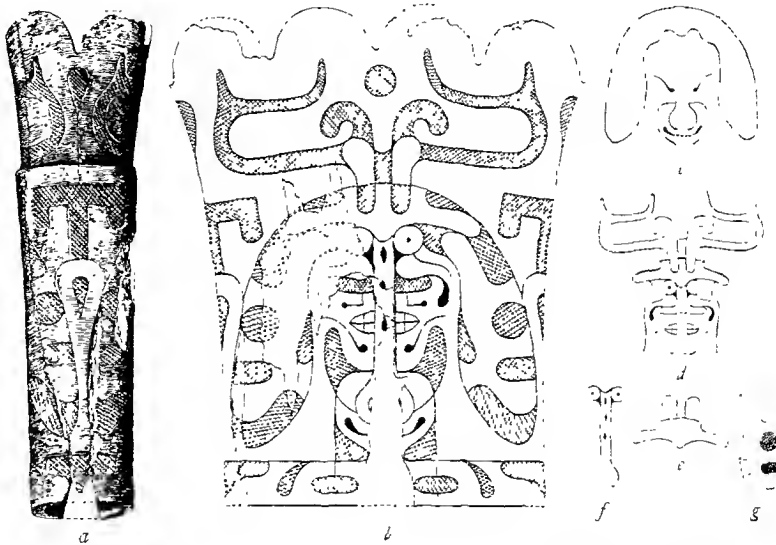


FIG. 1. *a*, Tube made from section of a human femur (from beside head of skeleton, Grave 278, Great Mound, No. 25, Hopewell Group, Ohio); *b*, incised design surrounding tube; *c-g*, principal units making up the design (3/5).

copper, one with sprouting horns, the other with mature branched antlers accompanied two burials in this mound, and a single mature antler cut from thin copper was taken from the large deposit of sheet copper objects found in this mound not far from these burials.<sup>5</sup> This was nearly the same form as the mature antlers in *d*.

Portions of other life designs appearing in the drawing are the profiles of grotesque heads upon the ears, *b*, *g*, and the beak of a bird, *f*, probably that of a spoonbill, as two or three undoubted sculptures of the beak of this bird or of the bird itself were found in this mound.

<sup>5</sup> For drawings of these headdresses see Willoughby, *The Art of the Great Earthwork Builders of Ohio* (Annual Report, Smithsonian Institution, 1916), pl. 4.



The more conspicuous other markings are the T-shaped figure on the back of the tube in *a*, and the sun-like disc above the antlers in *b*. Several smaller markings the purport of which is not clear make up the design. It seems probable that in this excellent delineation, Michabo is shown in perhaps more than one of his personalities.

During the completion of the exploration of the Hopewell Group of mounds by the Ohio State Archaeological Society, which took place thirty years after Dr Moorehead's work in this group, a burial of exceptional interest was discovered in the Great Mound, lying about twenty-five feet westerly from the grave which contained the bone tube described above. It consisted of the remains of a middle aged individual which rested on a raised earthen platform covered with fine gravel to the depth of an inch. Impressions in the earth showed that the grave was originally enclosed on each of its four sides by two logs, one placed over the other, and held in position by corner posts and large stones. Nothing remained of the logs and posts. Near the left arm of the skeleton (fig. 2) was a drinking cup made of a large helmet shell (*Cassis*). Two copper plates lay at the right of the skeleton. At each ear lay a copper spool-shaped ear plug, and another pair in the left hand. The usual pearl beads and cut bear incisors were found, together with eight large bone awls.

The most important feature, however, was the elaborate headdress which Mr Shetrone describes as follows:

At the top of the skull lay an elaborate headdress which consisted primarily of a rectangular curved head-plate, with large oval copper wings on either side. This bird-like headdress had been assembled on a saucer-shaped wooden base, parts of which were fairly well preserved. Along the margins of the wings lay, in place, small designs cut from mica, which evidently had been fastened to them for ornamentation. The remains of a bonnet-like appendage of woven fabric indicated the original form of the complete head-dress, and to the fabric had been sewed large pearl beads, bear claws, bird feathers and the head of a small raptorial, presumably a hawk.<sup>6</sup>

During the recent examination by the writer of this headdress in the Museum at Columbus, its significance became clear.

This was doubtless the grave of a priest who in his lifetime personified the Great Hare or whose functions were intimately connected with this noted culture hero.

In Figure 3 the principal parts of the headdress are assembled in their

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<sup>6</sup> H. C. Shetrone, *Exploration of the Hopewell Group. Certain Mounds and Village Sites in Ohio* (Publication, Ohio Archaeological and Historical Society, Vol. 4, Pt. 4), pp. 149, 150.

relative positions. These are wrought from native copper and consist of a head-plate of the usual form which crossed the crown from the forehead to the nape of the neck. This measures nearly twelve inches in length, is four inches wide in front, and tapers to about three and a half at the opposite

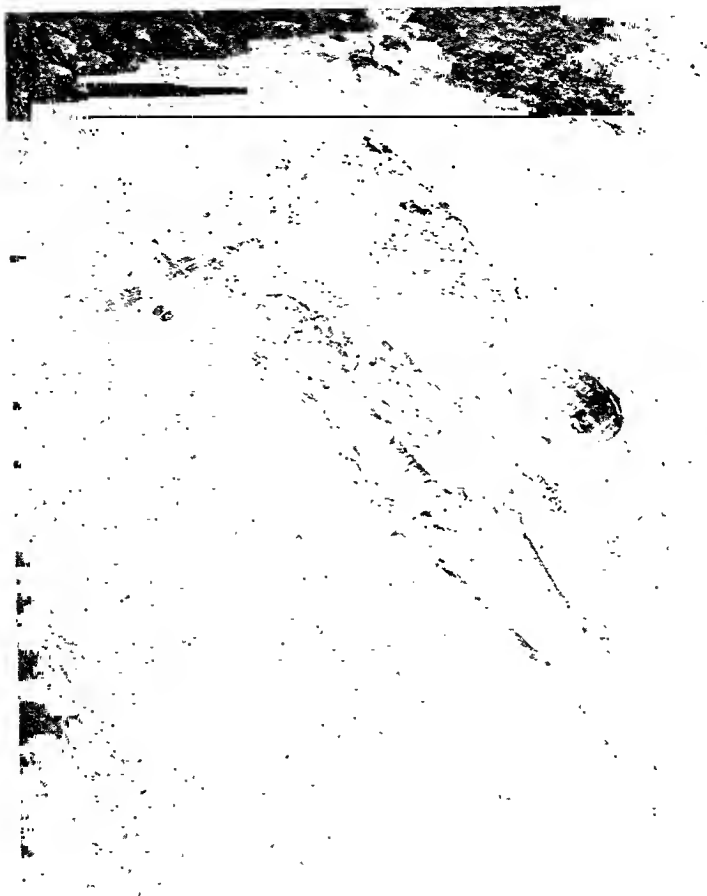


FIG. 2. Grave 11, Great Mound (No. 25), Hopewell Group, showing copper headdress symbolizing Michabo, the Great Hare. (Courtesy of the Ohio State Archaeological and Historical Society.)

extremity. Both the front and the back edges are neatly rounded. From this head-plate projected two large concavo-convex ears undoubtedly representing those of the hare. These were each approximately nine inches in length and a little over four inches across their widest part.

The narrow portion of each ear where it joined the head was bent so as to form a sort of socket into which a wooden plug had been inserted. These were for supporting the ears in their proper position and may have been attached to the saucer-shaped wooden foundation, fragments of which were reported by Mr Shetrone. These wood fragments were, of course, preserved through contact with the copper. There were several designs cut from mica which had evidently been used as decorations for the headdress, about a dozen in all, one being triangular, another diamond-shaped, and the others of the form of the two shown at either side of the central head-plate in Figure 2. These were about one and a half inches in length with

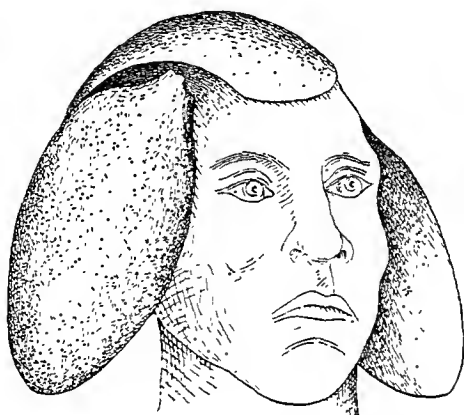


FIG. 3. Principal units of the copper headdress shown in Figure 2 assembled in their relative positions (1/5).

one end rounded. Large pearl beads, bear claws, feathers, and the head of a small hawk-like bird had, apparently, been used in ornamenting the woven fabric which, according to Mr Shetrone, had apparently formed a part of the head-piece.

Most of the skeletal remains from the burial mounds of the Hopewell culture have not as yet been systematically studied. The small though important series from the Turner Group, however, have been carefully studied and tabulated by Dr Hooton. In comparing these remains with those of the Madisonville people, a later and very different culture group, he says:

The affinities of the Turner Group people are rather with the Eastern dolichocephals, although there is present a brachycephalic element such as is often found among the Eastern Indians. The Madisonville people were mostly brachycephalic

and deformed, while the Turner Group people were mostly dolichocephalic, and without cranial deformation.<sup>7</sup>

I have elsewhere endeavored to show that the great earthwork builders of Ohio probably belonged to the Algonquian linguistic stock. Principally in the restricted area of the central and southern parts of this state, this people attained a degree of culture unsurpassed and probably unequalled by any of our native groups north of Mexico. This section had become, so to speak, the metropolitan district of the region lying mostly south of the Great Lakes and the St. Lawrence River, which extended roughly from about central Wisconsin eastward to central New England, and southward to about central Kentucky. Over this extensive area we find a homogeneous group of artifacts apparently of old Algonquian origin, but in the Ohio region many of these forms became elaborated and acquired a finish and symmetry rarely attained in other parts of the area.

Are not the copper headdress and the incised design upon the bone tube above described, both of which doubtless relate to the Great Hare, additional indications bearing out the hypothesis of the connection of the Hopewell people with the Algonquian linguistic group; and may not the presence of this beneficent and peace loving being as personified in one or more priests, account in a measure for the remarkable material and commercial attainments of the people of this ancient settlement?

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<sup>7</sup> Earnest A. Hooton, Papers, Peabody Museum, Vol. 8, No. 3, p. 132.

## THE DISTRIBUTION OF POTTERY TYPES IN NORTHWEST MEXICO

By DONALD D. BRAND

THE term "Northwest Mexico," as used in this paper, comprises only the states of Sonora and Chihuahua. The peninsula of Baja California is as yet little known archaeologically. Parties from the University of California, California Academy of Sciences, San Diego Museum and a few other institutions have carried on varied scientific investigations which have incidentally indicated the presence of kitchen middens throughout most of the coastlands, but the actual archaeology is still unworked.<sup>1</sup> Coahuila is also terra incognita for the archaeologist, as little more is known than that there are mummy caves in the northern portion, especially in the Sierra de Oballos, to the north of Monclova. Mummy caves are known to exist also in the Sierra San Lorenzo, east of Torreon. Sites with painted pottery have been reported, but these reports have never been verified.<sup>2</sup>

The prehistoric or archaeologic cultures of Northwest Mexico are definitely Southwestern in affiliation. Legged metates, tripod ware, complex ceramic forms, cloisonné ware, metal working, obsidian knives, pyramids—to mention only a few of the items typical of Central Mexican culture—were lacking in the Sonoran and Chihuahuan cultures.<sup>3</sup> The prevailing trough or scoop type of legless metate seems to have a distribution from Arizona to Michoacan, as does the large and rather crude three quarter grooved axe; but these are the only important types of artifacts common to Northwest Mexico and the so-called Toltec-Tarascan cultures which extended from Sinaloa and Durango to Michoacan and Guerrero. Legged metates are erroneously attributed to Chihuahua, mainly on the basis of a sketch by Bartlett, who served as the authority for Bandelier,

<sup>1</sup> See Proceedings, California Academy of Sciences, Series 4, Vol. 16. Also, the writings of Paul Rivet and Leon Diguët; and Herman F. C. ten Kate, *Reizen en onderzoekingen in Noord-Amerika* (Leiden), 1885. Since writing the above the author made a brief reconnaissance of Baja California, as far south as Mulegé, but found only kitchen middens with a scant representation of stone artifacts and no pottery.

<sup>2</sup> The locations of the mummy caves were given the writer by an educated native of Monclova. H. H. Bancroft (*The Native Races of the Pacific States*, Vol. 4: Antiquities, San Francisco, 1882, pp. 597–600) summarized most of the literature on Coahuilan archaeology. V. Alessio Robles (*Bibliografía de Coahuila, histórica y geográfica*, Mexico, 1927) gives additional references.

<sup>3</sup> A. L. Kroeber (*Native Culture of the Southwest*, University of California Publications in American Archaeology and Ethnology, Vol. 23, No. 9, 1928) gives a concise comparison of Southwestern and Central Mexican cultures

Lumholtz and other authors who perpetuated this misinformation.<sup>4</sup> Simple olla and bowl forms, typical of the Southwest, predominate in Northwest Mexico, although effigy ware reaches a high frequency in Chihuahua. However, the Chihuahuan hooded effigy jar has no counterpart in Central Mexico and should be regarded as a local development, just like the pictorial designs so characteristic of the Mimbres culture.<sup>5</sup>

Not a single shard of southern provenience has yet been found in the prehistoric sites of Sonora and Chihuahua. Not a piece of tripod ware or cloisonné ware has been turned up, and pottery from even nearby Durango and Sinaloa is nonexistent. This accords with the findings of reconnaissance parties from the University of California in 1929-1931. It was determined that an archaeologic hiatus existed between Central Mexican and Southwestern cultures, in a zone between the Yaqui and Sinaloa Rivers and extending eastward from the Gulf of California into southern Chihuahua.<sup>6</sup> The only items that predicate southern contacts, in the period after the postulated spread of agriculture from a Mexican center, are a few spindle whorls, metal trinkets, and terra cotta figurines. Four double cone spindle whorls of the Mexican type, but lacking characteristic decoration, have been found in the Papagueria. Also, a few undecorated hemispherical spindle whorls are known from Chihuahua.<sup>7</sup> Several copper bells have been reported from Chihuahuan sites, as well as several animal effigies in copper and silver.<sup>8</sup> At Nogales, on the American side, several figurines were unearthed, a few years ago, that closely resemble the "monos" from prehistoric sites in Sinaloa.<sup>9</sup> That concludes the list of items of possible southern provenience, with the exception of sea shells from the Gulf of Mexico, which were probably traded by nomadic Texas and Coahuila tribes.

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<sup>4</sup> J. R. Bartlett, *Personal Narrative* (New York), 1856, plate on p. 362, A. F. Bandelier, *Final Report*, Pt. 2 (Cambridge), 1892, p. 553, C. Lumholtz, *Unknown Mexico* (New York), 1902, Vol. 1, p. 88.

<sup>5</sup> G. C. Vaillant, *Some Resemblances in the Ceramics of Central and North America* (Globe), 1932, pp. 16, 33-37 illustrates the similarities and differences.

<sup>6</sup> C. Sauer and D. Brand, *Aztatlán* (Ibero-Americana 1, 1932), pp. 1-6, 16, 30-38, 49, 61; C. Sauer and D. Brand, *Prehistoric Settlements of Sonora* (University of California Publications in Geography, Vol. 5, No. 3, 1931), pp. 73, 106, 107, 115-17.

<sup>7</sup> C. Sauer and D. Brand, *Prehistoric Settlements of Sonora*, pp. 111-12, C. Lumholtz, *New Trails in Mexico* (New York), 1912, p. 142. Mexican type spindle whorls have also been found by Fewkes at Hidon Pueblo and by Kidder at Pecos.

<sup>8</sup> B. de Obregon, *Historia*, edited by Cuevas (Mexico), 1924, p. 185, E. Guillemin Tarrayre, *Exploration Minéralogique des Régions Mexicaines* (Paris), 1869, p. 176.

<sup>9</sup> C. Sauer and D. Brand, *Prehistoric Settlements of Sonora*, p. 79. Similar figurines have been found in the Gila valley of Arizona.

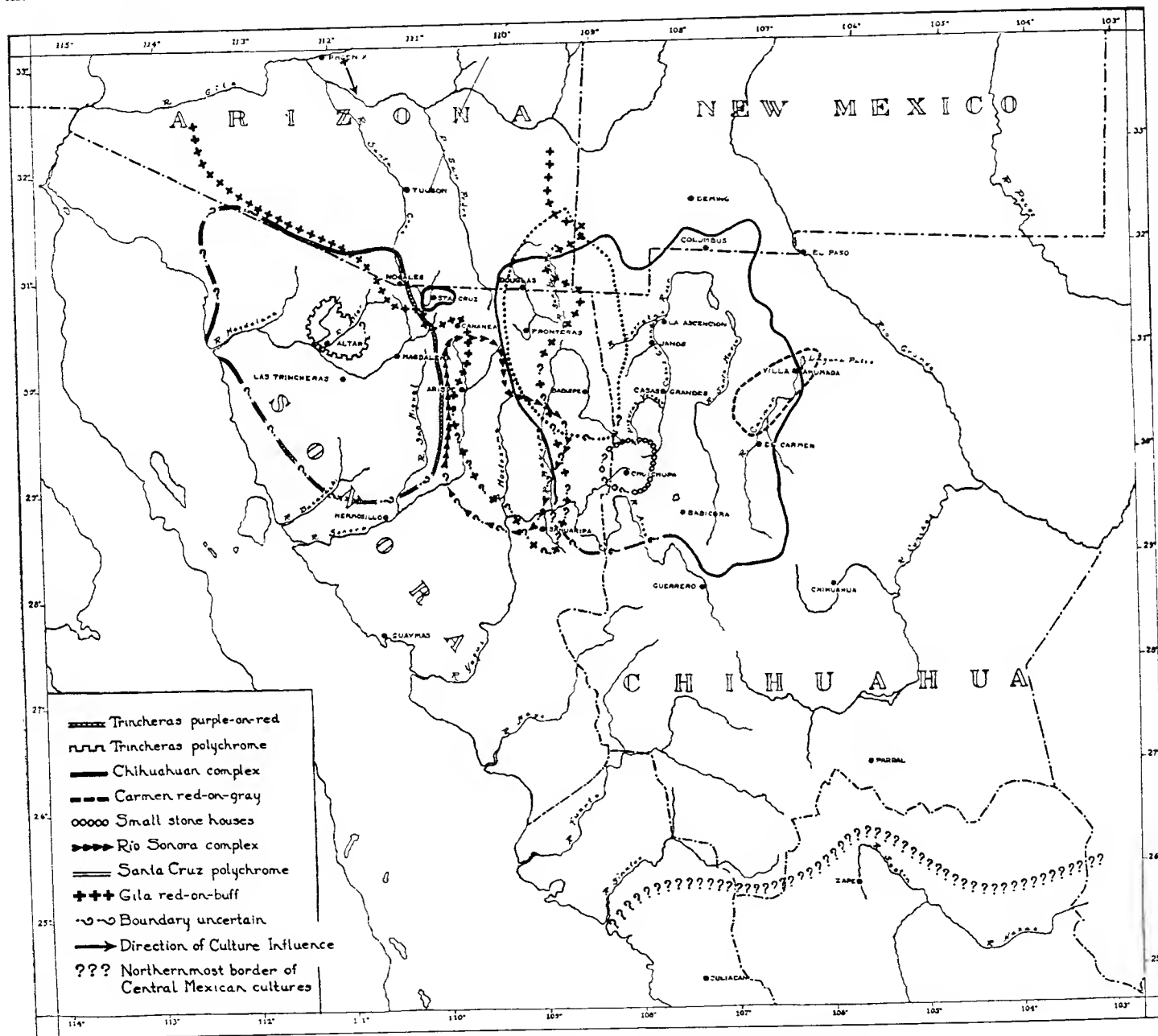


FIG. 1. Map showing the distribution of pottery types in Northwest Mexico.





Northwest Mexico, with the exception of Las Trincheras in Sonora and the Casas Grandes of Chihuahua, has consistently remained an unworked archaeologic region until quite recently. Soldiers, missionaries and travelers, from Ybarra in 1565 to the present, have described the few outstanding ruins, with perhaps a passing mention of pottery; but no archaeologic investigations were made until the reconnaissances of McGee (1894-95), Lumholtz (1909-10) and Huntington (1910) in the Papagueria, Bandelier (1885) in northeastern Sonora, and Bartlett (1852), Guillemin Tarayre (1865), Bandelier (1885), Lumholtz (1891), Blackiston (1904) and Hewett (1906) in northwestern Chihuahua. Hewett, as a Fellow of the American Archaeological Institute, carried on Bandelier's good work and outlined, in 1908, the bounds of Chihuahuan culture so well that only minor revisions are necessary. No similar work has yet been done for the Sonoran cultures, although a few boundaries have been established by Amsden in 1927, by parties sent out by the Gila Pueblo since 1928, and by Dr Sauer and the writer in the period 1928-30.<sup>10</sup>

<sup>10</sup> The Trincheras of Sonora are briefly mentioned in Bancroft, *op. cit.*, on the basis of some newspaper reports in the 1860's. In 1894 and 1895 W. J. McGee and W. Johnson made two short trips into northwestern Sonora for the Bureau of American Ethnology. Brief notes on various trincheras and ruins are given in Sixteenth Annual Report, Bureau of American Ethnology, pp. xxx, xxxiii-xxxv, xxxvi, lxii-lxv, lxviii; Seventeenth Annual Report, Part 1, pp. xl-xliv, 41; Science, n.s., Vol. 3 (1896), pp. 497-98; American Anthropologist, Vol. 8, pp. 372-73. Some ruins were heard of, on one of the Gulf of California islands, that sounded to McGee like "ruins of an ancient culture apparently akin to that of Southern Mexico"! Aviators have recently reported sighting trincheras on San Lorenzo Island. McGee's material is housed in the National Museum, but has not been reported upon. Carl Lumholtz, *op. cit.*, made a good study of the northwest Mexican Papagueria in 1909-10. In 1910 E. Huntington (The Climatic Factor, Papers, Carnegie Institution of Washington, 192, Washington, 1914), investigated the trincheras culture of the Rio Altar region to find material backing his desiccation theory.

Bandelier, *op. cit.*, in 1885 laboriously—on foot and by horse—examined northeastern Sonora and northwestern Chihuahua, via the San Pedro, Sonora, Bavispe and Casas Grandes drainages. Archaeologic notes on Chihuahua, prior to Bandelier—the first professional archaeologist—are to be found in Obregon, *op. cit.*; P. de Rivera, *Diario y Derrotero* (Guatemala), 1736; R. W. H. Hardy, *Travels in the Interior of Mexico* (London), 1829; J. Escudero, *Noticias Estadísticas del Estado de Chihuahua* (Mexico), 1834; P. Garcia-Conde, *Ensayo Estadístico sobre el Estado de Chihuahua* (Chihuahua), 1842; F. Garcia-Conde, in *El Album Mexicana* (Mexico), 1849, Vol. 1; J. R. Bartlett, *op. cit.*; E. Guillemin Tarayre, *op. cit.*; and J. G. Bourke, *An Apache Campaign in the Sierra Madre* (New York), 1886. More recent investigations are reported in F. Schwatka, *In the Land of Cave and Cliff Dwellers* (New York), 1893; C. Lumholtz, *Unknown Mexico* (New York), 1902, and various magazine articles; A. H. Blackiston, in *Records of the Past* (Washington), 1905-09, Vols. 4, 5, 7, 8, and *American Anthropologist*, Vol. 8, 1906; E. L. Hewett, *Les Communautés Anciennes dans le Désert Américain* (Geneva), 1908; articles by E. L. Hewett and K. M. Chapman in *Art*

Archaeologic Northwest Mexico has two distinct major cultures: that of the Trincheras in northwestern Sonora, and that of the Casas Grandes in northwestern Chihuahua. The Trincheras culture scarcely passes the international border, but the Chihuahua culture area extends into New Mexico and Arizona (see fig. 1). In addition to the several pottery types belonging to these two major cultures, there are five minor but probably independent and indigenous wares that will be discussed later.

From the north, tongues of Gila red-on-buff (Lower Gila red-on-gray of Kidder, Lower Salt red-on-yellow of Schmidt), Middle Gila polychrome (Salado ware of Gladwin), Mimbres black-on-white, Chupadero black-on-white, and El Paso polychrome invade the border region. In addition to these cultures that colonized into Northwest Mexico, the ubiquitous Little Colorado polychrome, Three Rivers red-on-terra cotta, Lincoln black-on-red, and Tularosa black-on-white occur as trade wares.<sup>11</sup> Since excavations and stratigraphic work in Northwest Mexico are practically lacking, the surficial finds of trade wares afford the principal means for dating the Mexican cultures in relationship to those of the American Southwest. On the basis of Carey's excavations in Chihuahua, crossfinds throughout the Southwest, and some personal observations made in pothunter excavations, the following tentative arrangement of ceramic cultures is made:

1. Gila red-on-buff, Three Rivers red-on-terra cotta, Carmen red-on-gray, and Mimbres black-on-white, were coexistent in Middle Pueblo III.

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and Archaeology, 1922, 1923; A. V. Kidder, *An Introduction to Southwestern Archaeology* (New Haven), 1924, E. Noguera, *Ruinas Arqueologicas de Casas Grandes* (Mexico), 1926, M. Amsden, *Archaeologic Reconnaissance in Sonora* (Papers, Southwest Museum 1, 1928), *Estado Actual de los Principales Edificios Arqueologicos de Mexico* (Mexico), 1928; I. Marquina, *Estudio Arquitectonico Comparativo de los Monumentos Arqueologicos de Mexico* (Mexico), 1928, C. Robles, *La Region Arqueologica de Casas Grandes* (Mexico), 1929, E. L. Hewett, *Ancient Life in the American Southwest* (Indianapolis), 1930, C. Sauer and D. Brand, *Pueblo Sites in Southeastern Arizona* (University of California Publications in Geography, Vol. 3, No. 7, 1930), C. Sauer and D. Brand, *Prehistoric Settlements of Sonora, op. cit.*; and H. A. Carey, *An Analysis of the Northwestern Chihuahua Culture* (American Anthropologist, Vol. 33, 1931, pp. 325-74).

In addition to C. Sauer and D. Brand, and M. Amsden, cited above, several papers of the Medallion Press (Gila Pueblo, Globe, Arizona) outline the extension of red-on-buff cultures into Sonora.

<sup>11</sup> A study of potsherd distributions has led to the conclusion, somewhat arbitrarily arrived at, that a dominant culture should be represented by seventy or more percent of the pottery complex at any one site. The first five above-mentioned wares are found dominant at sites within the bounds outlined on the maps (figs. 1 and 2). Therefore, the cultures represented are considered to have colonized southward into Mexico. The four mentioned trade wares constitute only minor percentages in north Mexican sites.

2. Chupadero black-on-white, El Paso polychrome, Tularosa black-on-white, Babicora polychrome, and Chihuahua black-on-red overlapped Late Mimbres and continued with increasing strength into Pueblo IV (dated 1300-1540 A.D.).

3. Middle Gila polychrome, Little Colorado polychrome, Lincoln black-on-red and the Early Rio Grande glazed wares, Huerigos polychrome, Trincheras wares, Villa Ahumada polychrome, and Casas Grandes polychrome flourished from around the beginning of Pueblo IV until perhaps 1450 A.D.

4. Historic wares: Pueblo, Pima, Papago, Opata, Seri, Tarahumara, Concho.

The pottery of northwestern Chihuahua—so rich in exotic forms and fine clay wares skillfully painted in warm colors—is well known through numerous museum collections, and the reproductions and descriptions by Lumholtz, Hewett, Kidder, Chapman and others.<sup>12</sup> However, the effigy and polychrome wares have been collected and stressed to the neglect of the other wares constituting the Chihuahuan ceramic complex. This complex consists of painted wares, redware, blackware, and plain buffware, which can be classified, according to decorative technique, into some ten pottery types.<sup>13</sup>

Painted wares constitute from fifty to seventy percent of the shards and whole vessels collected, excluding plain buffware. The clay used is commonly of light color and good quality, with only a small amount of tempering material. Usually a slip was not used. The color of unslipped vessels varies from a chalky white through buff to orange. The most common color is a grayish cream or buff. When slipped the color is whitish. This slip is soft and weathers easily, taking with it the design painted upon it. Much of the white-slipped ware and orange colored ware is composed of a somewhat coarse paste. The range in color and in quality of paste is extreme.

The olla or jar with high gently sloping upper body, rounded shoulder, and full round bottom is the most common form. The generalized olla form

<sup>12</sup> The best illustrations and descriptions are to be found in A. V. Kidder, *The Pottery of the Casas Grandes District, Chihuahua* (Holmes Anniversary Volume, Washington, 1916, pp. 253-68); K. M. Chapman, *Casas Grandes Pottery* (Art and Archaeology, Vol. 16, 1923, pp. 25-34); C. G. Harcum, *Indian Pottery from the Casas Grandes Region* (Bulletin, Royal Ontario Museum, Vol. 2, 1923, pp. 4-11); A. V. Kidder, *op. cit.* (1924); C. Lumholtz, *op. cit.* (1902); E. L. Hewett, *op. cit.* (1908).

<sup>13</sup> The ceramic complex of Chihuahua and its areal differentiation has been treated in detail by the writer in "The Historical Geography of Northwestern Chihuahua," filed in the University of California as a Ph.D. thesis in 1933.

comprises nearly sixty percent of all Chihuahua wares, which sets the Chihuahua culture quite apart from the Gila red-on-buff, Carmen red-on-

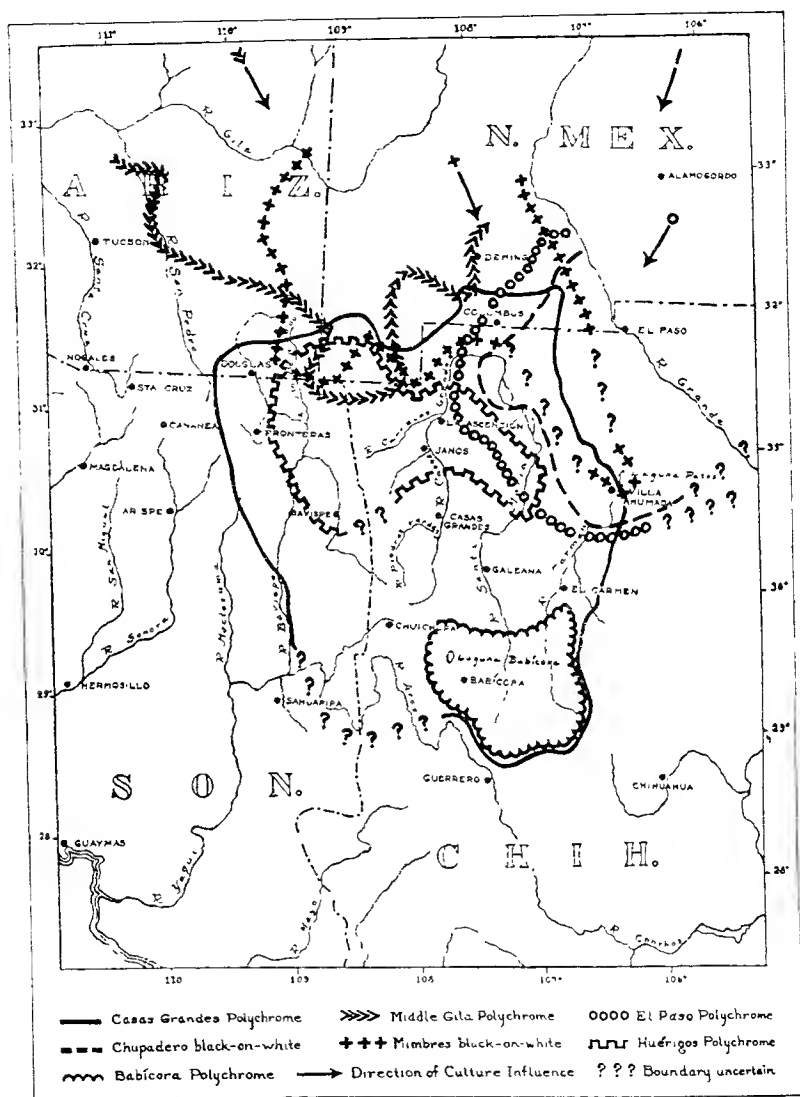


FIG. 2. Map showing the distribution of pottery types in Northwest Mexico.

gray, Mimbres black-on-white, Three Rivers red-on-terra cotta, Lincoln black-on-red, Middle Gila polychrome, and Little Colorado polychrome

cultures where bowls are quite dominant. In the Tularosa and Chupadero wares the bowl and the handled olla or jug are most common. Only in the Trincheras and El Paso wares, of the nearby cultures, are ollas the dominant form. Effigy vases, including the examples where plastic features are added to the sides of standard ollas, make up about fifteen percent of the wares. In this regard Chihuahuan culture is outstanding, as no other portion of the archaeologic Southwest has such a wealth of modeled forms. The nearest, in this respect, is the Gila red-on-buff culture. Bowls are generally small and deep, differing thus from the large shallow food bowl so common to the north. Bowls comprise about fifteen percent of all the Chihuahua wares. Even on the bowls, decoration of the Chihuahuan wares is normally on the exterior. The one exception is the Huerigos polychrome, to be discussed later. Chihuahuan designs have been so well described and analyzed that it is unnecessary to outline them here.<sup>14</sup>

The decoration of painted ware is usually with polychrome designs of black and red figures on a light background. Many pieces, however, lack one or the other color; these examples commonly being decorated in black on gray or buff. There is also a black on polished slipped red or brown ware. The polychrome wares have been divided into four types, on the basis of general decorative technique, paste, areal distribution and stratigraphic relationship (to a minor extent).

Most of the painted ware belongs to the fine clay, unslipped, well decorated type which has been termed Casas Grandes polychrome. Only this type contains life designs such as plumed (horned ?) serpents, parrot heads, quail, etc. Most of the effigies belong to this polychrome ware. It is found throughout the Chihuahua culture area (fig. 2), but most abundantly in the middle basin of the Casas Grandes River: from Cave Valley on the Piedras Verdes and San Diego on the San Miguel, through the Casas Grandes-Ramos-Corralitos Valley, to Janos and La Ascencion, with an important ramification to the southwest up the Janos and Carretas Rivers and over into the upper Bavispe drainage. This distribution coincides with the areas of greatest population density, as indicated by number of ruins and quantity of artifacts. On the basis of stratigraphic work in Chihuahua and crossfinds in the Southwest, the Casas Grandes polychrome is considered to be the most recently developed of the Chihuahua wares, with the possible exception of Huerigos polychrome.

Casas Grandes polychrome was probably manufactured, over a lozenge

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<sup>14</sup> A. V. Kidder, *op. cit.* (1916), and H. A. Carey, *op. cit.*, contain the best analyses of designs.

shaped area, from San Jose Babicora in the south to 76 Draw on Palomas Arroyo in Luna County, New Mexico, and from Villa Ahumada in the east to the Sulfur Springs Valley of Arizona and Fronteras in Sonora on the west. The southwestern boundaries, in the Rio Aros country, are still undetermined. Casas Grandes polychrome sites are known from the middle Aros, and the Satachic (Castillo) and Nacori tributaries of the Aros. It is possible that the cliff houses reported from the barrancas of the upper Mayo and Fuerte Rivers in southwestern Chihuahua may turn out to be the most southern outposts of Chihuahuan culture. The region from the Sestin to the Nazas River in northern Durango is known to have pottery and other artefacts, but the cultural affiliations seem to be with the Toltec-Tarascan type of culture known from the environs of Durango City and southward. The region from the plains of Babicora through southern Chihuahua to the Sestin country is practically lacking in ruins and artefacts, outside of a few fortified hills (such as that near Bachimba) and the excavations at the dam of Boquillas where stone axes and an obsidian drill were discovered.

As a trade ware Casas Grandes polychrome was perhaps even more popular in the Southwest than the Little Colorado (St. Johns) polychrome. Shards of this ware have been found near Arivechi on the Sahuaripa River in eastern Sonora, and westward to Las Trincheras on the Altar River in northwestern Sonora. The peripheral distribution of this tradeware continues north and east across southern Arizona, being found at such sites as Los Guanacos (near Tempe), Togetzoge (between Superior and Miami), and Bead Mountain House (on Pinal Creek). Whole vessels of Casas Grandes polychrome have been unearthed throughout the Mimbres, middle Rio Grande and Tularosa basin regions of New Mexico, usually in association with Middle Gila polychrome, Chupadero black-on-white, and El Paso polychrome. Shards have been found as far north as the Gobernadora Canyon in Rio Arriba County, New Mexico, and as far east as beyond the Pecos River in Eddy County, New Mexico, in camp sites associated with El Paso and Chupadero wares.<sup>15</sup> Although very common as tradeware in the Hueco-Rio Grande basin around El Paso, Casas Grandes polychrome and the other Chihuahuan wares fade out rapidly downstream and to the east.

There is a crude form of polychrome ware which has a comparatively poor clay, a general decorative technique below the Casas Grandes standard, and a virtual absence of life designs. This ware has been termed

<sup>15</sup> Dr H. P. Mera, archaeologist of the Laboratory of Anthropology at Santa Fé, New Mexico, orally provided the writer with the distribution in eastern New Mexico.

Babicora polychrome, which is a fitting designation as it is dominant in the Babicora region, where Carey's excavations have demonstrated a sequence of black-on-red, Babicora polychrome, and Casas Grandes polychrome.<sup>16</sup> Babicora polychrome is found throughout the Chihuahua culture area, but seems most abundant in such peripheral regions as the Rio Carmen, upper Santa Maria, Babicora plains, and the upper Bavispe (fig. 2). Probably it should be regarded as the old basic polychrome ware which maintained itself in the peripheral regions, perhaps through the life of the Chihuahua culture.

The black-and-red-on-white slip ware has been termed Villa Ahumada polychrome because of the great bulk of this ware at the Loma de Montezuma and other sites near that villa on the Rio Carmen. Generally the paste is inferior to that of Casas Grandes polychrome, and the decorations are less involved and not so well executed. It probably represents an attempt to obtain a good background, despite the use of inferior clay. An influence from the Middle Gila may be postulated, as the use of white slip and the general appearance are highly reminiscent of Middle Gila polychrome. The distribution of this ware is nearly coextensive with that of Casas Grandes polychrome.

Huerigos polychrome is quite distinct from Casas Grandes polychrome in its normally coarse paste, bright orange background, and crudely executed designs in broad lines of rough black pigment or black to green glaze. Interior decoration is an uncommon feature in Chihuahua wares, but it occurs in nearly half of the Huerigos vessels, which comprise a high percentage of bowls. About a quarter of the Huerigos ware has a white slipped interior upon which broad line decorations are executed in green glaze. The exterior is nearly always decorated with fire roughened and raised black lines upon a decided orange base. This is the same ware that Amsden<sup>17</sup> termed Peripheral Casas Grandes, from some shards obtained in the Bavispe Valley. It is possible that Huerigos polychrome represents an influence from the Little Colorado region, as Little Colorado wares are encountered in greater numbers in the region of Huerigos ware than elsewhere in the Chihuahua area. This Huerigos polychrome ware is localized in the northwestern portion of the Chihuahua culture area (fig. 2), with its apparent center and best development being at Huerigos, on a northern tributary of the Carretas River. It is abundant in the Bavispe, Animas, Huerigos, Carretas, and Ascencion Valleys, but is rarely encountered east of the Santa Maria River and in the Sierra Madre Occidental.

<sup>16</sup> Carey, *op. cit.*, spent most of his time, in Chihuahua, in the Babicora country.

<sup>17</sup> M. Amsden, *op. cit.* (1928), pp. 45-46.

Redware constitutes about fifteen percent of Chihuahuan vessels. It is quite variable in paste, surface decoration and form. It may be polished or unpolished, slipped or plain. The rich red slipped ware is usually polished, although all combinations of polish and slip may be present on one vessel. A great deal of manual texturing is employed—corrugations, incisions, gouges, indentations, etc. These may occur separately or in various combinations. There is a great variety of forms, and effigies are quite common. Shards and vessels of untextured redware—both polished and unpolished—are most numerous. Perhaps a quarter of the redware has a band of vertical thumbnail indentations around the neck, just below the rim. This decoration occurs less frequently in black and buff vessels. As this form of decoration does not appear among any of the neighboring cultures, it is a most reliable indicator of Chihuahua culture. The distribution of redware as a whole is quite general throughout the culture area.

Black on polished red or brown ware is a variation of the plain polished redware, which often assumes a brown or mahogany color. Black line decorations and solid elements are applied in patterns similar to those of the duochrome variants of the painted wares. It is the least common of the redwares, with major centers of manufacture in the Babicora country, lower Santa Maria, and middle Casas Grandes. Chihuahua black-on-red ware seems to have been developed prior to any of the polychromes, but continued in use to the end.

Blackware may be polished or plain, and varies in color from dark brown to lustrous jet black. The plain blackware is usually made of dark brown paste, coarsely tempered, and having a rough surface. Olla forms predominate. Although normally a plain utility ware, at times vessels of unpolished blackware were decorated with plastic forms and other relief features. The polished blackware is similar to the redware in paste, but has been carbonized on the surface by a smothered fire process. This surface is highly polished. The principal forms are an olla with slightly flaring lip, and a bowl with incurved rim. Eccentric shapes exist, but are rare. Blackware is quite common, occurring throughout the area in proportions as high as fifteen to twenty percent. Museum collections quite often exaggerate the proportional representation of this ware.

Most of the incised ware is redware, both polished and unpolished, although some of it is black or buff ware. The incisions are usually parallel bands formed by the shallow removal of undried clay with a blunt stick or wooden comb. Incised ware is found throughout the culture area.

Corrugated ware is textured on the surface with the partially smoothed coils used in building up the vessel. Many of the redware vessels have



corrugations only on the upper third. Rarely are whole ollas corrugated. The corrugations vary in amount of smoothing or wiping, and often are cross incised, indented and otherwise textured. The distribution of this ware is also quite general.

There are five minor but probably independent pottery types on the peripheries of the Chihuahuan culture area. In the east is the red-on-gray ware found almost exclusively in the Rio Carmen basin. This Carmen red-on-gray ware has a medium coarse paste, tempered with sand, and often showing a mica content. The ware is somewhat thick, with surfaces floated or sometimes slipped. Decorations are red lines upon a gray background. No whole vessels are known. This ware is dominant in several sites from Hacienda El Carmen to the Laguna Patos, and is represented in the Santo Domingo country to the west of the lower Carmen (fig. 1). It is always associated with Mimbres and El Paso wares, while the Chihuahua polychromes are lacking or barely represented. This may indicate a development and life during only Pueblo III.

In the "top of the mountain" country of the Sierra Madre, on the pine-clad plateau where the Bavispe, San Miguel and Piedras Verdes have their sources, there are numerous small stone houses, frequently surrounded by a protective wall. No painted ware occurs in these sites, which are quite obscure, normally appearing as weathered heaps of lava rock with a few pine trees growing out of the debris. The associated potsherds are mainly of a plain yellowish porous ware, with some shards of raked gray and brown (Sonora) and indented rim red ware (Chihuahua). The distribution of this type of ruin seems to be from Garcia to Chuichupa, and from the headwaters of the San Miguel westward to the Sierra de Nacori (fig. 1). Lumholtz, in 1891, discovered several of these sites.<sup>18</sup>

Centering in the area around the mutual corners of Sonora, Chihuahua, New Mexico, and Arizona (fig. 1) is a polished redware, sometimes with a black interior, that has the appearance of a surface gouged with a triangular tool. This pitted effect may have been obtained by nearly obliterating or smoothing out an indented corrugated surface.<sup>19</sup> Although found as trade ware well into the Chihuahua area to the south and east, the principal distribution of gouged redware is in the upper Animas and San Luis Valleys of southwestern New Mexico, westward into the San Bernardino and Agua Prieta drainage basins in Cochise County, Arizona, and southward in the Fronteras, San Bernardino and middle Bavispe regions

<sup>18</sup> C. Lumholtz, *op. cit.* (1902), pp. 42-49.

<sup>19</sup> Suggested by M. Amsden, *op. cit.* (1928), p. 46, and by A. V. Kidder in a letter to the writer.

of northeastern Sonora. Random test pits in Cochise County indicate that this ware was the last to appear in the area. It is therefore under suspicion as non-Chihuahuan, perhaps a ware of the first Opata invaders of this region.<sup>20</sup>

In northeastern Sonora, approximately in the drainage basins of the upper Sonora, Moctezuma and Bavispe Rivers, is a ceramic complex of plain and rather coarse wares individualized by some poorly incised designs or crudely raked exteriors. Colors range from gray, through yellow-brown to black, with some vessels carrying a light red slip. Despite the reconnaissances of Bandelier, Amsden, and Sauer, very little is known as yet concerning this so-called Rio Sonora culture.<sup>21</sup> The main area of this culture was in the upper Rio Sonora basin (fig. 1). It is represented by incised and raked potsherds into southeastern Arizona, and the upper Bavispe of Sonora and Chihuahua. The most eastern site containing this ware was found some twenty miles west of Casas Grandes. It has been found also on the plateau where the Bavispe heads. Bandelier intimates that this was the culture of the early Opatas. From more recent research it seems probable that Bandelier confused actual Opata wares with earlier and distinct archaeologic wares.

Near the pueblo of Santa Cruz, on the upper Santa Cruz in Sonora, is developed a very localized ware which, so far, has been found predominant only in the immediate area. This local ware is thick, and of a coarse porous paste. A grayish-white slip was applied which always checked on firing to present a crackled appearance. Purplish black and blood red paints were applied in cross-barred designs and solid triangles.<sup>22</sup> Only shards of this ware have been discovered, to date. At the Santa Cruz site, this Santa Cruz polychrome was found in association with Sedentary and Classic Gila red-on-buff. A few trade pieces have appeared as far down the Santa Cruz River as Tumacacori, Arizona, southward to Remedios in the upper Magdalena drainage, and southeast to Arispe and Sahuaripa. The affinities of this anomalous ware are only conjectural.

Northwestern Sonora is the seat of a widespread and distinctive culture which has been termed that of the Trincheras because of normal association of the pottery wares of this area with the fortified hills or "cerros de trincheras" which here are most numerous and best developed. Although "cerros de trincheras" (not to be confused with the agricultural trincheras

<sup>20</sup> C. Sauer and D. Brand, *op. cit.* (1930), p. 443.

<sup>21</sup> A. F. Bandelier, *op. cit.* (1892), pp. 482-98; M. Amsden, *op. cit.* (1928); C. Sauer and D. Brand, *op. cit.* (1931), pp. 102-105, 111, 117.

<sup>22</sup> C. Sauer and D. Brand, *op. cit.* (1931), pp. 75-81, 110-11, 117.

or terraces in the Sonora-Chihuahua border country) occur sporadically over much of the Southwestern archaeologic region,<sup>23</sup> nowhere else do they attain the size or have the abundance of definitely associated painted pottery that is the case in northwestern Sonora. These fortified, usually detached and conical hills, seem to represent the defensive substitute of the open basin country for the cliff and cave structures possible in the more mountainous country to the north and east. Practically all of the associated pottery can be classified into two types: purple-on-red ware, and a purple-and-red-on-buff polychrome.<sup>24</sup>

The Trincheras purple-on-red ware is made of a coarse calcareous paste, tempered with sand. The fired product is a thin, porous and very brittle pottery, normally brick red in color. A micaceous hematite paint, purplish-maroon or purple black in color, was applied so thickly over the thinly slipped base as to present the designs in slight relief. The painting was heavy and unskilled, with almost a total lack of fine line decorations and elaborate patterns. Ollas, normally large, dominate the forms to a marked degree, although several bowl forms are known. The ollas have a heavily painted rim, with pendant solid color triangles. A common feature is large blocks of cross-hatchings, with diagonal or rectangular intersections. Wavy lines, curvilinear patterns, and heavy parallel straight lines occur frequently.

This Trincheras purple-on-red ware is the dominant pottery type at practically all of the sites from the Arizona-Sonora border south to an undetermined point in the region of the lower Bacavachi and Sonora Rivers, and from the San Miguel de Horcasitas drainage basin west nearly to the Gulf of California coast (fig. 1). Apparently the Trincheras peoples did not live in the coastal plain excepting near the mouth of the Rio Altar (also known as the Concepcion, and the Magdalena). The general extent of the culture was coincident with the Altar drainage basin and that of the San Miguel tributary of the Rio Sonora—the modern Papagueria of Sonora, and the western Opata country. The northwestern limits are uncertain, but probably did not extend past the Mexican continuation of Growler Valley, to the west of the Sonoita River.

<sup>23</sup> The great number of trincheras hills and the large type ruin at Las Trincheras in the Rio Altar region make valid this designation for the culture. It should be kept in mind, however, that trincheras hills—representative of other cultures—are known from the Gila basin in Arizona and in many parts of Chihuahua, e.g., near Bachimba, Casas Grandes, and Ojitos.

<sup>24</sup> C. Sauer and D. Brand, *Prehistoric Settlements of Sonora with special Reference to Cerros de Trincheras*, give the most complete discussion of this culture available. Some of the Trincheras designs are illustrated also in *The Red-on-buff Culture of the Papagueria* (Medallion Papers No. 4, Globe, 1929).

Although cremation burial, trough metates, three-quarter grooved axes, stone vessels (rectangular trays), and absence of corrugated and black-on-white wares, are traits of the Trincheras culture in common with that of the Arizona Canal Builders (Hohokam of some archaeologists), the paucity of bowls, dominance of ollas, and difference in decorative technique indicate dissimilar origins. The chronologic relationship of the Trincheras and Canal Builder cultures is uncertain. There was practically no overlapping of areas, the international boundary almost acting as an expression of the march between the two cultures. Only at one site, near Tumacacori, has Trincheras ware been found in quantity north of the border and in association with much Gila red-on-buff. Red-on-buff ware is practically lacking in the Trincheras sites of Sonora, and only a very few Trincheras shards have been found in the Gila country. This apparent non-interchange of pottery between two adjacent pottery-using cultures would be interpreted as representing a time difference, were it not for the marked mutual boundary which may indicate a long continued state of antagonism and absence of commerce. The occupation of the Altar region by a presumptively hostile people may explain the otherwise strange lack of red-on-buff "trailmarkers" from the Gila country to the Gulf coastlands, so rich in shells and salt. The presence of Casas Grandes polychrome tradeware at many of the fortified sites, while practically absent in the unprotected valley sites, may indicate a development of the Trincheras culture in the early peaceful period (probably Pueblo III), with a retreat to the defensive sites in Pueblo IV when Casas Grandes polychrome had become a widely traded ware.

The finest ware of the Altar country was a purple-and-red-on-buff polychrome. This Trincheras polychrome ware does not differ in paste from the purple-on-red ware, but a slip was applied that retained a buff color after firing. On this buff base the characteristic Trincheras designs were executed in the common hematite paint, with the addition of a thinly applied light red pigment. The distribution of Trincheras polychrome is pretty well restricted to the large valley sites in the north central portion of the culture area (fig. 1). The Boquillas, Altar, and Arroyo Seco tributaries of the Rio Magdalena from the north outline the distribution of this ware in important quantities. Some whole vessels have been unearthed at Nogales. No tradewares have been found in the Altar sites possessing Trincheras polychrome.

Although Gila red-on-buff does not transgress the international border from Quitovaquito to Nogales, there is a decided penetration of this ware into northeastern Sonora; principally up the Santa Cruz and San Pedro

affluents of the Gila, and southward down the Sonora and San Bernardino-Bavispe Rivers. Along the Mexican border and southward for some sixty miles the red-on-buff shards are apparently typical Sedentary (Sacaton) and Classic (Casa Grande) wares.<sup>25</sup> Unexpectedly these wares appear again along the Rio Sahuaripa, south of the Yaqui River.<sup>26</sup> The apparent intervening hiatus probably represents only the almost complete lack of archaeologic reconnaissances in that region. The southern limits are not known, but it is certain that neither Gila red-on-buff nor any other Southwestern ware is present in southern Sonora and northern Sinaloa, where an archaeologic gap exists between the lower Yaqui and the Rio Sinaloa. Gila red-on-buff is present south and eastward, in a constantly diminishing degree, as far as the Animas Valley of New Mexico and the San Bernardino Valley of Sonora (fig. 1). Along the eastern periphery, e.g., near Animas, Cloverdale, and San Bernardino, red-on-buff is found associated with Mimbres black-on-white. Little trade, however, went on to the east, as only one shard has been reported from the Mimbres Valley (Mattocks site), and none at all from Chihuahua. The sometimes postulated influence of Gila red-on-buff on Chihuahuan culture is unwarranted, considering the lack of trade shards, paucity of bowls and absence of many other items typical of the Canal Builder culture.

Middle Gila polychrome peoples seemingly colonized southeast, out of a middle Gila-upper Salt hearth, as far as the Mexican border in Cochise County, Arizona, and Hidalgo County, New Mexico. Potsherds of this distinctive ware are found in diminishing numbers up the San Pedro River and down the Sulfur Springs and San Bernardino Valleys. Practically no Middle Gila polychrome is found in Sonoran sites. The trade with Chihuahua, however, out of the Animas and Mimbres areas which were seemingly occupied in part by Middle Gila peoples after the decline of the Mimbres, was quite extensive. Shards and whole vessels of this ware have been found as far south as ten miles upriver from Casas Grandes, in the Galeana basin of the middle Santa Maria, and near Villa Ahumada in the lower Carmen Valley. It has been suggested that some of the Middle Gila polychrome ware found in Chihuahua is local imitation, because some of the designs incorporate elements peculiar to the Chihuahuan polychromes, the clay is often the same as that of the local wares, and the typical Middle

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Gila line break is lacking.<sup>27</sup> This possibility is quite probable, but the unbroken distribution of Middle Gila polychrome shards in sites from the middle Gila to Casas Grandes and Villa Ahumada warrants the belief that most of the pieces found in Chihuahua are of trade origin. Middle Gila wares were traded east all through the New Mexico-Chihuahua borderlands, and have been found in considerable numbers into the Tularosa-Hueco-Rio Grande depression and sporadically to a point sixty miles east of El Paso, Texas.

The Mimbres culture, centered in the Mimbres Valley of southern New Mexico, occupied the Animas and Playas Valleys down to the Mexican border, and penetrated a short distance into Chihuahua between Dog Springs and Mount Riley. Mimbres wares are represented at nearly all of the sites in the desolate playa-sand dune country between Columbus and Laguna Patos, some eighty miles to the southeast. In several of the sites to the east of Laguna Patos, Mimbres black-on-white, Mimbres smoothed corrugated, and Carmen red-on-gray constitute the bulk of the potsherds. It is possible that a tongue of Mimbres culture should be postulated into this region (fig. 2). Shards have been found as far south as the Carretas, Tapicetas, middle Casas Grandes, and even near Bachiniva on the Santa Maria headwaters. Below Casa de Janos, on the Rio San Pedro (or Janos), there is a site with a strong representation of Mimbres black-on-white and Three Rivers red-on-terra cotta. This combination, together with Carmen red-on-gray, is quite common in the middle and lower Rio Carmen region. Whole pieces of Mimbres ware have been recovered in Chihuahua. (A killed bowl dug out of a Chihuahua site near Janos is in the possession of the writer.) Stratigraphic investigations indicate that Mimbres black-on-white is older than most of the Chihuahua polychromes. Considering the paucity of Chihuahua shards found in definite association with Mimbres wares in the Mimbres country, it may be that the rare but widespread finds of Mimbres black-on-white should be attributed to the importation of vessels by some "archaeologically minded" Chihuahuan colonists or travelers. To the southwest Mimbres wares are found sporadically in the Dragoon and Mule mountains, east of the San Pedro in Arizona, and down to the Sonora border along the San Bernardino River. The southeastern distribution swings abruptly north from Carmen and Villa Ahumada to the vicinity of Mount Riley and eastward some miles beyond El Paso. No Mimbres polychrome has yet been found in Mexico.<sup>28</sup>

<sup>27</sup> A. V. Kidder, *op. cit.* (1916)

<sup>28</sup> H. S. and C. B. Cosgrove, *The Swarts Ruin* (Papers, Peabody Museum, Vol. 15, No. 1, Cambridge, 1932), define the Mimbres culture and outline its areal extent, as known north of the border. See also C. Sauer and D. Brand, *op. cit.* (1930), fig. 3.



From the northeast two cultures extended their areas of occupation into northern Chihuahua. El Paso polychrome ware<sup>29</sup> occupied all the region from the Rio Grande as far west and south as Columbus, the lower Casas Grandes, Sabinal, the lower Santa Maria, and the lower Carmen (fig. 2). This ware is found in such large quantities and proportions, near Villa Ahumada and at other sites within the northeastern portion of the Chihuahuan culture area, that it seems reasonable to postulate the contemporaneous occupation of this region by these two peoples. This association in time is borne out by the high frequency of Chihuahuan trade pieces in the Hueco-Tularosa hearth area of the El Paso culture, and the nearly as common occurrence of El Paso shards and whole vessels at many of the sites in the Ascencion, Janos, and Casas Grandes areas of highest population densities in Chihuahua. The eastern range of El Paso polychrome attains to the Pecos Valley, to sites thirty miles east of Carlsbad. It is possible that the fragment of a bowl of thin brown ware with black painted decoration, found by Coffin in Bee Cave Canyon, Brewster County, Texas, may represent an extreme southeastern extension of El Paso trade ware.<sup>30</sup> Westward from Columbus and Deming, El Paso polychrome shards diminish rapidly in frequency but are found as far as the Chiricahua and Dragoon Mountains of Arizona.

The distinctive Chupadero black-on-white ware,<sup>31</sup> with a paste resembling fine grained concrete, coarsely striated or scraped surfaces, and characteristic two or three roll handles on ollas, occupies much the same area as El Paso polychrome, though somewhat less extensive to the south and west (fig. 2). It is the dominant ware at sites along the Casas Grandes, below Boca Grande, and is strongly represented in the Ascencion country, lower Santa Maria and lower Carmen. Chupadero ware is normally accompanied by El Paso ware, and occurs in greater relative abundance when associated with an increasing percentage of that ware. Shards are known from Cochise County, Arizona, to Eddy County, New Mexico. No Chupadero polychrome has yet appeared in Mexican sites. A number of whole

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<sup>29</sup> E. E. Alves, Pottery of the El Paso Region (Bulletin, Texas Archeological and Paleontological Society, Vol. 3, 1931); W. S. Stallings, Jr., El Paso Polychrome (Laboratory of Anthropology, Santa Fé, Technological Series, Bulletin 3, 1931); W. S. Stallings, Jr., Notes on the Pueblo Culture in South-Central New Mexico and in the Vicinity of El Paso, Texas (American Anthropologist, Vol. 34); and H. S. and C. B. Cosgrove, *op cit.* (1932), pp. 93-95, have placed in print the knowledge at hand concerning this ware in its type locality.

<sup>30</sup> E. F. Coffin (Indian Notes and Monographs, No. 48, 1932), p. 61.

<sup>31</sup> H. P. Mera, Chupadero Black on White (Laboratory of Anthropology, Santa Fé, Technological Series, Bulletin 1, 1931); W. S. Stallings, Jr., *op cit.* (1932), pp. 71-74; H. S. and C. B. Cosgrove, *op cit.* (1932), pp. 95-98, have given the most complete descriptions

Chupadero black-on-white ollas have been unearthed by pothunters in Chihuahua, principally around Ascencion.

Four trade wares make an appearance in Northwest Mexico from Pueblo cultures not adjacent to or invading Mexico. The most widely represented of these is Little Colorado, which is found over much the same area covered by Middle Gila polychrome. It is almost never found excepting at sites with Middle Gila polychrome. The total number of shards, however, is but a fraction of the latter ware. Little Colorado polychrome, therefore, should probably be considered a ware traded in by the Middle Gila peoples. No whole vessels from Mexico are known to the writer.

Next in importance is Three Rivers red-on-terra cotta.<sup>32</sup> This ware is considered by Mera and Stallings to represent the initial phase of a pottery sequence that passed through Lincoln black-on-red and merged with Rio Grande glaze I redware. Trade shards of Three Rivers red-on-terra cotta are found southeast from the Tularosa hearth into the lower Carmen and Casas Grandes Valleys of Chihuahua. That the earlier phases were contemporaneous with Mimbres black-on-white is indicated by the frequent association of the two wares in Chihuahuan sites. The Lincoln black-on-red variation is found sparsely in the lower Carmen basin. A very few shards of Tularosa black-on-white occur in Chihuahua sites, and one complete jar or pitcher has been reported from Colonia Enriquez near Casas Grandes.

A consideration of the nature and distribution of ceramics and other artifacts of Northwest Mexico suggests the following points:

1. No basketmaker or early Pueblo (I, and perhaps II) cultural remains have been identified from Sonora or Chihuahua. If Robert's<sup>33</sup> comparison of Texas basketmaker sandals with those from Cave Valley, Chihuahua, is valid, further investigations should be made of the numerous caves in the Sierra Madre Occidental and its western foothills, especially around Chitacahui, Sahuaripa, and Trinidad in Sonora, and Cave Valley, Guaynopa, Ohuivo and Zapuri in Chihuahua. Basketmaker material is known from Coahuila, and the Big Bend and El Paso districts of Texas.

2. The absence of Central Mexican and early Pueblo artifacts suggests that the first sedentary occupation of Northwest Mexico was in late Pueblo

<sup>32</sup> C. B. Cosgrove's manuscript report on "Preliminary Survey of the El Paso Pueblo District" to the El Paso Archaeological Society (1925), S. Stubbs, Preliminary Report of Excavations near La Luz and Alamogordo, New Mexico (El Palacio, Vol. 29, 1930), E. E. Alves, *op. cit.*; H. P. Mera and W. S. Stallings, Jr., Lincoln Black-on-Red (Laboratory of Anthropology, Santa Fe, Technological Series Bulletin 2, 1931), W. S. Stallings, Jr., *op. cit.* (1932), and H. S. and C. B. Cosgrove, *op. cit.* (1932) furnish most of the available written data concerning this ware.

<sup>33</sup> Smithsonian Miscellaneous Collections, Vol. 81, p. 8, C. Lumboltz, *op. cit.* (1902), p. 68.

II or early Pueblo III times by Southwestern peoples out of the north. The possibility of a migration of the proto-Canal Builder and Trincheras peoples up the Pacific coast of Mexico is not out of the question, but probably should be shoved back into a very early period, perhaps before the rise of Archaic culture as characterized for Central Mexico.

3. Late Pueblo III and early Pueblo IV saw the most widespread occupation of Northwest Mexico. Probably around 1400 A.D. there began an extended invasion by nomadic tribes out of the east. These invasions pushed the Chihuahua peoples out of the plains (excepting at a few large and highly fortified sites, such as Casas Grandes and Hacienda Tapiécitas at the mountain margin), southwestward into the Sierra Madre proper and the western foothills in Sonora. Most of the agricultural trincheras sites and cave dwellings in western Chihuahua and eastern Sonora are practically lacking in trade wares, although possessing the latest developed polychrome ware, which bears out the thesis of a cutting off of the Mexican Southwest from the Pueblo country by nomadic invaders. Some of these aliens may have taken over agriculture, pottery making, and sedentary life to become the Opatas, Jovas and other tribes found in possession by the Spanish explorers. Much further research is necessary before the relationships of the Papago, Trincheras, Pima, and Canal Builder peoples should be hazarded.

4. Further reconnaissances and excavations are strongly urged. Excavations to determine stratigraphy, house structure, manner of disposal of the dead, etc., should be carried out as soon as possible in the principal sites of the individual cultures, and within such corridors of movement as the Rio Grande near El Paso, the Carretas-Animas, San Bernardino-Bavispe, and the upper Sonora. So far, the only authorized excavations in Northwest Mexico have been made by the American Museum of Natural History and Columbia University. The University of California, Southwest Museum, and Gila Pueblo have done practically all of the survey work of the last seven or more years.

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PALEONTOLOGICAL EVIDENCE FOR  
THE ANTIQUITY OF THE SCOTTSBLUFF  
BISON QUARRY AND ITS ASSOCIATED ARTIFACTS

By C. BERTRAND SCHULTZ  
AND LOREN EISELEY

I

THE publication of a paper by Alfred S. Romer announcing the discovery of a "fossil" camel in a Utah cave deposit of supposedly Recent age,<sup>1</sup> revived, on a new front, the long war over Pleistocene man in America. A reply by the late O. P. Hay<sup>2</sup> soon followed, and with it the conflict shifted from emphasis upon a denial, as was formerly the custom, of the authenticity of the association of man with fossil animals in America, to the question of the time of extinction of these same animals.<sup>3</sup>

It is here, in the opinion of the present writers, that the question threatens to linger for a long time to come. Everyone of fair judgment will now admit that there are undoubted cases of association of artifacts with the remains of animals no longer living on the North American continent. In that, at least, we have made some progress, and cleared the air sufficiently to gaze about us with less prejudice. At the same time, however, we have run upon a snag. For the assemblage of ghostly forms which once, in the opinion of paleontologists, characterized the Pleistocene, have, almost without exception, suddenly stirred in their graves and moved forward into the Recent. This, to the dismay of the "Pleistocene school," and the everlasting joy of those anthropologists who were beginning to feel a trifle hard pressed in their beliefs.

Though the writers feel that the last word has yet to be uttered in this controversy, and though they are of the opinion that the appearance of certain Pleistocene species within a more or less ancient and fabulous Recent does not, *ipso facto*, dissipate the problem of man's antiquity in America, they do realize that this new obstacle enormously complicates the question. For if so many of these extinct species extend into the Recent while the deposits continue to give evidence of some age, the problem bids fair to be insurmountable for some time to come. Hence it is with some hesitation that they offer the following account of the bison quarry at

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<sup>1</sup> Romer, 1925, pp. 19-20.

<sup>2</sup> Hay, 1928, pp. 299-300.

<sup>3</sup> Romer, 1933, pp. 39-83.

Scottsbluff, Nebraska. But the brief preliminary paper<sup>4</sup> which accompanied photographs of the artifacts attracted such attention, both favorable and unfavorable, that it seems only fair to issue a complete report on a site which, whether Pleistocene or Recent, evidences a notable antiquity, and has some bearing on the archaeological as well as the geological problems of the High Plains.

## II

Two discoveries of artifacts with the bones of extinct bison had previously been made in Nebraska<sup>5</sup> by the Morrill Paleontological Expeditions of the University of Nebraska, but under circumstances which, unfortunately, make it extremely difficult to establish beyond question the age of these deposits.<sup>6</sup> These discoveries had, however, been sufficient to increase interest in the problem, and Dr Erwin H. Barbour, Director of the Nebraska State Museum, had encouraged the men to be alert for any evidence. In the latter part of May, 1932, a bone bed located by Mr Ray Swanson of Scottsbluff, was called to the party's attention. The quarry was found to be situated within a few hundred yards of Signal Butte,<sup>7</sup> sixteen miles west and three miles south of Scottsbluff, on the north bank of Spring Creek about fifty feet east of the juncture with Kiowa Creek. Spring Creek, in trenching its course, had cut athwart an old stream channel in the gravels of which the bison bones were now exposed. This fossil stream was incised in a floor of Brule clay of Oligocene age, and its gravels varied in size from ordinary sand and gravel up to chunks of Tertiary sandstones and clays several pounds in weight. The coarseness of some of this material suggests a stream of considerable volume and possessing a strong current. Later, for some reason, climatic or otherwise, the carrying power of the stream was reduced, and a lighter load was transported as is now indicated by the overlying sand and silt. Still later the water apparently ceased to flow, and at the present time a covering of from twelve to twenty-seven feet of windblown material rests upon the remains of the old stream bed. The geology is such as to indicate a deposit of considerable antiquity, and the change in invertebrate life remarked on later in this paper suggests climatic change.

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<sup>4</sup> Barbour and Schultz, 1932. The term "pre-Folsom" used in this paper has been abandoned. In the opinion of the authors the theory of Yuma-Folsom sequence based on the study of technique has not so far met the test of stratigraphical evidence. It is more likely that the Yumas are knives or other implements

<sup>5</sup> Schultz, 1932.

<sup>6</sup> Strong, June 27, 1932.

<sup>7</sup> Strong, Publication, Smithsonian Institution, 1932, p. 69.

On August 4, 1932, the first artifact was found. It had been partially exposed by the caving off of a bank containing fossil bones. Its position was well toward the bottom of the bone bed about ten inches above the floor of Brule clay. Because of the position of the artifact, the party realized that it would be very difficult to keep *in situ*, so numerous pictures were taken in the presence of the six members of the Museum expedition. In the end, a slight jar caused it to slip from its position, though the impression from which the artifact slipped made positive its original location. Mr C. B. Schultz, who was in charge of the field party, immediately telegraphed Dr Barbour, Director of the Museum, who, in turn, informed *Science Service* in Washington, D.C., of the find. On August 6, the quarry was visited by Dr Barbour and by Dr E. H. Bell, Assistant Professor of Anthropology at the University of Nebraska. The artifact was studied, and also the site, in order to see if intrusion were possible.

A misunderstanding arose at the time the site was reported on<sup>5</sup> which should be clarified here for the sake of accuracy. It was stated that the artifact was "one and one-half feet below the original surface"<sup>6</sup> and "not sufficiently below the surface to eliminate the possibility of accidental intrusion through earth cracks or gopher holes." It should be noted now that more than ten feet of overburden had been removed and the artifact in reality had been buried at a depth of some fourteen feet. Photographs taken by the Museum field party and others show clearly the amount of overhead. As for intrusion, the chances were ruled out with the finding of three more dart points and four other fragments and artifacts during August and September.<sup>7</sup> These were found from four to six feet back from the original face of the bank at a depth of nearly seventeen feet, and were in the top of the gravels below the bone bed.

As for the points themselves, the photographs are largely self-explanatory, though they do not, in certain cases, reveal the full beauty of the workmanship (pl. 1). A is suggestive of the Folsom type, though it lacks the groove and one ear. A careful examination suggests that this ear was not broken, at least in the quarry, but that the artifact had been completed without it, perhaps due to a break at the time of making. There is a possibility that the artifact may have been readapted for use as a small knife

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<sup>5</sup> Bell, 1932

<sup>6</sup> Dr Bell and Dr Van Royen have themselves corrected this error, as in their latest paper they quote from the Nebraska bulletin on this point verbatim, and make no reference to the earlier statement (Bell and Van Royen, 1934, p. 61)

<sup>7</sup> Dr Bell grants this point in a recent issue of the Wisconsin Archeologist (Bell and Van Royen, p. 60)

and never used as a projectile. *C* is finely chipped, but is broken too far up the point to be of value in determining the type of base. *D* is a true and finely chipped example of the "Yuma," i.e., the ungrooved Folsom, type,<sup>11</sup> but *e*, with its exaggerated indentations at the base, is more suggestive of modern points. It is, however, worked down to a remarkable thinness in cross section, and flaked with considerable accuracy entirely across the blade. As for *f* and *g*, they may well be rejects. *H*, a snub-nosed scraper, has nothing distinctive about it.

It is worthy of note in passing that here for the first time<sup>12</sup> a Folsom and a Yuma point appeared together *in situ* in such a way that there can

7' to 14'	Yellowish gray loess
2' to 3'	Fine silts and sands
3'	Sands and gravels (bones and artifacts)
	Brule Clay (floor of old channel)



FIG. 1. Scottsbluff bison quarry (after fossils were removed).

be no question as to their equal age and contemporary existence among the people who produced them.

### III

The geology of unglaciated western Nebraska is insufficiently known to admit of positive conclusions on the age of the quarry. Dr A. L. Lugen,

<sup>11</sup> It has been suggested by Drs Bell and Van Royen that Yuma points such as *d* can be easily matched from any ordinary site in Nebraska. Since Dr W. D. Strong and Mr W. R. Wedel, previous workers in the field of Nebraska archaeology, have failed to feature artifacts of this sort from ordinary sites, it would seem that there is some difference of opinion existing as to what constitutes a Yuma point. The writers insist that true Yumas are known from later sites in Nebraska only in a few obviously intrusive cases, that is, where one or two had been picked up by later peoples.

<sup>12</sup> Jenks, 1934, reported a second find of this sort but so far the published illustrations hardly suggest the true Yuma point.

Associate Professor of Geology in the University of Nebraska, who has made a careful study of the Nebraska Pleistocene, and whose lack of bias has been well indicated by his careful and non-committal study of the two previous finds,<sup>13</sup> has this to say, however:

The deposit located near the base of Signal Butte and described by Barbour and Schultz is difficult to date. Most geologists who have visited the site are agreed that it most certainly belongs to the Pleistocene epoch. Molluscs found in the deposit are quite similar to those found elsewhere in the Upland formation. When all the somewhat inconclusive evidence is considered, the deposit seems to be not older than late Kansan, and it does not seem to be as late as Wisconsin. Apparently its age is late mid-Pleistocene, that is, post-Kansan, pre-Wisconsin.<sup>14</sup>

It is not expected by the present writers that such a dating will be acceptable to many archaeologists. It is given by them as the opinion of a competent geologist whose specialty is the Nebraska Pleistocene. It may be that other geologists will differ, but until further pronouncements by authorities are forthcoming, this tentative dating must at least be quoted --a dating, incidentally, which is supported by Baker.<sup>15</sup>

It would have been far less embarrassing to the authors of the present paper if a post-Wisconsin dating had been vouchsafed by the geologists consulted. Archaeologically, this would be more plausible, but the dating of the site is a geological as well as an archaeological problem, and the geological evidence is of prime importance. It is obvious, however, that one difficulty which confronts such datings is our lack of information concerning the antiquity of *Homo sapiens*, an antiquity which bids fair to be extended in the light of recent knowledge.

#### IV

The deposit of bison bones in which the artifacts were found was exceedingly dense, surpassing in this respect the quarry at Folsom, New Mexico. In the quarry at the base of Signal Butte there was literally a stratum two to four feet in thickness and extending for some thirty feet, composed of skulls, jaws, and other skeletal parts including some complete legs still in articulation. This latter point is important. No re-deposited material could have been so laid down that any bones would remain in alignment. Cases of such articulation immediately rule out any question

<sup>13</sup> Strong, June 27, 1932.

<sup>14</sup> Lugen. Bulletin of Nebraska State Museum, in press.

<sup>15</sup> Baker, *in litteris*. "Not later than Yarmouth and the basal beds possibly Aftonian." Mr Baker has, however, indicated elsewhere the possibility that certain of the Yarmouth loess deposits beyond the Wisconsin area may prove to be Peorian in age.





Eight artifacts from the Scottsbluff bison quarry (lettered in the order found)



Comparison of bison skulls. Scottsbluff bison quarry. Upper, *Bison bison* Linnaeus (from Nebraska). Lower, *Bison occidentalis* Lucas, referred (From Scottsbluff bison quarry, skull slightly crushed but no restoration; Nebraska State Museum, No. 2-10-6-32).

of late wash. Whatever the age of the site, its covering has been laid down by the normal process of wind deposition since the stream ceased to flow.

Many hundreds of pounds of material were secured, and though many of the skulls were badly crushed, ten were obtained in a restorable condition. Four of these are now on exhibition at the Nebraska State Museum. As has been pointed out by Romer and Figgins,<sup>16</sup> the taxonomy of the American bison is in a state of some confusion which only considerable study will resolve. Nevertheless a number of distinct species are recognized, and while the nomenclature varies and has, in some cases, resulted in the too frequent designation of new species, it remains true that the Scottsbluff bison are distinct in several important particulars, notably the length, slenderness, and different curvature of the horn cores. J. D. Figgins has recently named what he considers to be a new type of bison from Folsom, New Mexico. The type specimen was at one time regarded by Hay as *Bison occidentalis* and by Figgins as possibly the female of *Bison taylori*, but the Scottsbluff bison, which are considered by Figgins to be representatives of this same type, removes that possibility. Figgins has designated these bison as *Bison oliverhayi*. For an exact anatomical description and photograph the reader is referred to his recent paper.<sup>17</sup>

These bison are extinct, but the date of their disappearance cannot be ventured. We know them from one specimen at Folsom and a number at Scottsbluff. The deposits in which they occur can certainly not be called recent in a narrow sense, but beyond that it is a question for the geologist to answer. It is well to point out in passing, however, that in suggesting a case for the survival of extinct bison into what he designates as sub-Recent time, Romer's documentation<sup>18</sup> is most scanty, and relies largely on the assumption that all cases of association, such as that reported by Williston,<sup>19</sup> are of no "great geological antiquity." This statement, in the opinion of the writers, is more or less confusing. It has been the endeavor of archaeologists to establish the time of man's arrival in North America. This may have occurred prior to the last glacial advance or later, but in neither case has any "great geological antiquity" been assumed by any but a few

<sup>16</sup> Romer, 1933, p. 53, Figgins, 1933, p. 17.

<sup>17</sup> Figgins' paper (1933) It is a trifle early to determine beyond doubt whether the species appellation *oliverhayi* will stand in the literature or whether further paleontological study will establish these bison as *occidentalis*, or at least as no more than varietally distinct from the latter. Hence the designation on Plate 2. In either case they are extinct. Figgins' paper contains an excellent discussion of some of the points involved, and in addition the reader is referred to Hay (1913, p. 173), and to Hay and Cook (1930, pp. 28, 30).

<sup>18</sup> Romer, 1933, pp. 79, 80, 82.

<sup>19</sup> Williston, 1902, pp. 335-37.

extremists. The matter of some thousands of years' difference in arrival does have significance to the anthropologist, however, and lends importance to the effort to determine whether that arrival can be dated as pre- or late Wisconsin, or into the young Recent of the last ten thousand years. In view of Johnston's suggestion<sup>20</sup> that "a route for migration through the Mackenzie River valley has been open, let us say for 25,000 or 30,000 years," a respectable antiquity is possible for these "post glacial" bison of the Great Plains region— an antiquity which could fairly be called late Wisconsin and Pleistocene.

In the case of Williston's find this assumption of a sub-Recent dating is certainly open to question, the site having been reputably diagnosed as Pleistocene<sup>21</sup> and containing, besides *Bison occidentalis*, *Elephas columbi*, and *Platygonus campestris*. These last are reported from the drifts, but still possess significance. The Folsom quarry has so far proved undatable. Cook, after a study of the Wolf Creek, Texas, find, pronounced the latter undoubtedly Pleistocene, although an unbiased opinion today must consider the validity of the faunal evidence in terms of the possibility of survival.<sup>22</sup> It would seem to be reasoning in a circle to suggest that the Folsom and Wolf Creek finds are Recent because dart points were associated with the remains. It is better to let the dating remain unsolved for the time being, than to make use of such an assumption.

O. P. Hay<sup>23</sup> commented that he knew of no extinct bison except one peculiar species, *Bison sylvestrus*, which survived the Wisconsin glacial stage. In 1930 Hay<sup>24</sup> recorded *Bison sylvestrus* as still the only extinct form which can be considered as Late Wisconsin. This bison is so far known from a single fragmentary specimen whose type, judging from the pictured remains, would seem difficult of determination.<sup>25</sup> The circumstances of the geology in this case are not such as to inspire confidence, as Hay himself did not visit the scene of the find, and comments merely that the specimen was "found on a Tanarack swamp at some place not yet exactly ascertained in Huron County, Ohio."<sup>26</sup>

In the case of *Bison occidentalis*, the present writers know of one soli-

<sup>20</sup> Johnston, 1923, p. 44.

<sup>21</sup> Williston, 1909, p. 88.

<sup>22</sup> Hay, 1923, p. 257, 258. In 1924 (p. 200) he added, "Hay indicated a find from Manitoba which he considered to be of Wisconsin deposit. No further mention of it was made in 1930 when he wrote his conclusions on Wisconsin bison." Whether this represents Hay's final opinion we do not know. The writers are unaware of this find until after the completion of this paper.

<sup>23</sup> Hay, 1930, p. 282.

<sup>24</sup> Hay, 1930, p. 30.

<sup>25</sup> *Ibid.*, p. 31.

tary case of survival into what may possibly be regarded as Recent, and doubtless as post-Wisconsin time, in Minnesota. This is a find in a peat bog on the Wisconsin drifts near Crosby, Minnesota, as recorded by Leverett.<sup>26</sup> This specimen was identified and discussed by Hay. The bones were found during some hydraulic mining operations which had exposed about thirty feet of drift, and above this, six to eight feet of peat. The bones were found at or near the bottom of this latter material. The drift was regarded by Leverett as having been laid down about the middle of the Wisconsin stage. How long after this the bones were buried, Hay finds it impossible to say.<sup>27</sup>

It is also a point not without interest that the Morrill Paleontological Expeditions of the University of Nebraska, after years of intensive fossil collecting throughout the state, have yet to record a find of an extinct bison from a site which can be definitely identified as Recent. Moreover, they have yet to record the discovery of Folsom type points in association with *Bison bison*.

The work at Signal Butte carried out by the Smithsonian Institution, under the active leadership of Dr William D. Strong, revealed a very ancient site in the lowest level—a site which Dr Strong is inclined to regard as early Recent.<sup>28</sup> There has been no mention of extinct forms of life being found in this archaeologically rich layer on the butte, yet this site lies but a few hundred yards from the bison quarry, and one would suppose that the ancient butte dwellers, if they lived at approximately the same time, hunted the same bison. The age of the butte site thus suggests, if anything, an even greater age for the quarry.

These facts are not listed with the intention of maintaining that extinct bison never lived on into the Recent, but they are given in order to point out how questionable the issue still is, and founded on what frail evidence is the theory of their recent survival. If documentation for survival is so scanty, even though we accept what we have, the inference is such as to make the appearance of dart points in so many of these cases rather surprising, for the scarcity of recent occurrence would suggest that these animals were becoming very rare.

In addition, it must be taken in to consideration that it is not only *Bison*, i.e., *Bison occidentalis*, closely allied to the recent species, which appears in these cases, but both *Bison figliini* and *Bison figliini*, besides the recently added *Bison chitreyi*. This is an impressive assemblage of bison

<sup>26</sup> Leverett, 1932, p. 144.

<sup>27</sup> Hay, 1924, p. 208.

<sup>28</sup> Strong, 1933, p. 282.

alone and must tell heavily against any theory which would push what must have been dying species at the close of the Wisconsin glacier very far into the Recent. The Recent deposits of the Plains have so far yielded no trace of survival after the close of the pluvial period coincident with the withdrawal of the Wisconsin ice.

Neither, to venture somewhat far afield, does the Great Basin yield such clear-cut evidence of Recent survival as seemed apparent to Romer when, on the basis of certain studies of J. Claude Jones, he placed the Lahontan fauna which includes *Felis atrox*, horse, camel, and elephant into an antiquity of "between 2,000 and 4,000 years ago"<sup>29</sup> at the most.

Admittedly there is reason on other grounds for postulating a comparatively late survival of the camel and horse in the Southwest, but survival cannot be regarded as definitely proved which uses one interpretation of the highly debatable geology of Lake Lahontan as a major piece of evidence. Moreover, such a young antiquity as Jones indicates should, if the fauna is correct, have revealed further and obviously Recent finds even in Basket Maker sites. Dr Ernst Antevs, one of the most outstanding authorities on the Pleistocene, is extremely critical of Jones' views, and feels that the low salinity of certain neighboring lakes upon which Jones based part of his study have no bearing on Lake Lahontan. In concluding his monograph, Antevs says:

Thus it seems that Bonneville, Lahontan, Mono, and other waters in the Great Basin were due to the combined effect of decreased evaporation because of low summer temperature and relatively heavy precipitation connected with the climatic changes that ended the expansion of the ice sheets and made these disappear. The pluvial periods coincided with the maximum extension of the land ice and the early stages of ice retreat. A pluvial period and an expansion of the lakes represented each glacial epoch; the last high-water stage corresponded to the last, the Wisconsin glaciation, and to the last glaciation in the mountains of the West. The Last Bonneville, Lahontan, and Mono, consequently, date back about 30,000 to 35,000 years.<sup>30</sup>

It is to be noted that one of the chief arguments for the recency of Romer's camel, outside of the preservation of desiccated tissue, is the fact that it was obtained from a cave which was supposed to be slightly later than Lake Bonneville. Antevs' estimate of the age of Lake Bonneville leaves a considerable latitude of time for this camel, and while its state of preservation is remarkable, we have no means of knowing how long muscle tissue may last under perfectly dry conditions such as obtain in

<sup>29</sup> Jones, 1925, p. 47, 50.

<sup>30</sup> Antevs, 1925, p. 77. This opinion is in general agreement with that of Gilbert, Russell, Menzies and Hay.

the Southwest. Indeed there is no reason to believe it might not last thousands of years.<sup>41</sup>

To illustrate the variety of opinions existing about the Great Basin, one may also point out that Dr Charles Keyes<sup>42</sup> regards Lakes Bonneville and Lahontan as having no relation whatever with the Wisconsin ice, completely antedating, in fact, the glacial period.

The problem of the use of climatic change in the Southwest as a time measurement grows more acute when we come to consider caves in the southern portion of the area, i.e., Conkling's, Shelter cave and others, for here climatic change may not have been so great, and the pluvial period not so well marked.<sup>43</sup>

On the basis of finds elsewhere<sup>44</sup> there is every reason to believe that the sloth was one of the last of the Pleistocene animals to vanish, and the fact that his remains have been found associated with camel and horse in what appear to be late Pleistocene deposits in the Southwest certainly suggests that they survived much later than Hay had supposed. It is possible that they existed during the pluvial period, or shortly following, but that they continued on into the Recent is another question and at present an unanswerable one.

This divergence from the main topic of this paper has not been undertaken with any idea of being exhaustive or of upholding one theory against another. It has merely seemed fitting to point out a few of the problems which face us in determining the question of survival, and to indicate that we are far from attaining a definite solution. It may not be amiss, however, to suggest that every late straw in the wind points to man's presence here during the pluvial period which began as the glaciers started to recede. Jenks' latest find at glacial Lake Agassiz doing much to clinch the matter.<sup>45</sup>

#### IV

The use of invertebrates as an aid in determining the approximate geological age of a site has received little attention in the domain of archaeology. Indeed it has been thought that the molluscan fauna of the Pleistocene and Recent were too nearly identical to serve as any but the most doubtful indicators of antiquity. This view has been held in the past by

<sup>41</sup> Harrington, 1933, p. 167, Hay, 1928, Stock, 1931, p. 32, Lull, 1930, p. 347.

<sup>42</sup> Keyes, 1918.

<sup>43</sup> Antevs, 1925, pp. 71, 72.

<sup>44</sup> Hay, 1912, p. 9.

<sup>45</sup> Jenks, 1934.

Shimek<sup>36</sup> and Baker.<sup>37</sup> The passage of time has, however, resulted in an increased knowledge of the Pleistocene invertebrates and a modification of this earlier opinion. Baker himself, writing in 1931, commented that "during the last ten years . . . a large amount of Pleistocene material from authenticated stratigraphic deposits has been critically examined with the result that several forms appear to be specifically or varietally distinct from those of their relatives living today."<sup>38</sup>

Under the circumstances it seemed wise to make every effort to obtain as full a record of the invertebrate life of the deposit as possible. In this we were eminently successful, and the collected data, along with the vertebrate material, sheds additional light on the deposit. Later, additional collections were made, and in the latter case four levels were distinguished and the material from each kept separate. All identifications are by Frank C. Baker of the University of Illinois, an acknowledged authority in this field.<sup>39</sup>

Of sixteen species identified the first year, two from the bone deposit level were extinct, and according to Baker<sup>40</sup> are not represented in the recent fauna. These are *Succinea ovalis pleistocenica* and *Succinea grosvenori gelida*. The following year an augmented collection of invertebrate material added *Fossaria parva tazewelliana* to the list of extinct forms. Of this form Baker says:

Typical *tazewelliana* is very distinct from *parva* (the living species) but there are, in most collections, some specimens approaching *parva* and in some cases these variations would be classed as *parva* but for the presence of the *tazewelliana* form. The number of *parva*-like forms increases in later deposits and the Early Wisconsin collections show a mixture of the two forms. The specimens in all but one Late Wisconsin collection are referable to *parva*. No *tazewelliana*-like form has been observed in the Recent fauna.<sup>41</sup>

*Parva* has so far not appeared in the deposit, and the fact that *tazewelliana* is well nigh lacking in Late Wisconsin collections must, we think, give added weight to its appearance in the bone deposit.

<sup>36</sup> Shimek, 1913, p. 506.

<sup>37</sup> Baker, 1920, pp. 360, 369.

<sup>38</sup> Baker, 1931a, p. 270.

<sup>39</sup> Those desiring a complete list of the invertebrate fauna from this deposit are referred to a forthcoming paper in the Nebraska Museum bulletin series, which will give this information in detail.

<sup>40</sup> *In litt.*

<sup>41</sup> Baker, 1931a, p. 290.



*Succinea grosvenori gelida* is known from Yarmouth to Early Wisconsin time<sup>42</sup> and none have so far been recorded from Late Wisconsin deposits. *Succinea ovalis pleistocenica* is also unrecorded from any deposits later than Early Wisconsin.<sup>43</sup>

Of the other species which extend into the Recent there seem to be none which do not occur in the Pleistocene as well. Since by far the larger percentage of Pleistocene invertebrates are represented unchanged in recent times, the paucity of extinct material does not in itself cast doubt on the age of the find nor necessarily imply a case of survival. In fact the appearance of these forms taken in conjunction with the evidence derived from faunal change and the occurrence of fossil bison seems, if one cannot put it in stronger terms, at least suggestive of considerable antiquity.

A study of the four sections kept separate in the faunal collection revealed a definite progression from an abundance of fresh water forms in the bone horizon to an entirely land fauna in the upper layers. This is suggestive of a gradual climatic change tending toward greater aridity and, in addition, aids in ruling out any theory of a recent re-deposition of material. An indiscriminate re-deposition in recent times would not have resulted in such an orderly and progressive change from water to land forms in the invertebrate fauna.

It is not the intention of the authors to place undue weight upon the value of the invertebrate evidence tending to suggest antiquity, but to offer it for what it is worth in conjunction with the other evidence; namely, the undisturbed character of the deposit and the presence of fossil bison. It is worth passing attention, also, that the species represented are, even though mostly living, of old types with a long Pleistocene history. Recent forms, such as *Fossaria parva* appear to be absent. Both Mr Baker and Dr Lugn would perhaps assume a greater antiquity for the deposit on the basis of their specialties than the present writers, for obvious archaeological reasons, would care, at present, to venture. The all too fragmentary character of our knowledge concerning the geology of unglaciated western Nebraska is a stumbling block not soon to be removed. Under the circumstances to pronounce that the site seems definitely Pleistocene and at least Late Wisconsin<sup>44</sup> may be branded as bold in the eyes of many authorities. The writers are well aware that they are venturing into a discussion which

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<sup>42</sup> Baker, 1931a, p. 285; Baker, 1931b, p. 152.

<sup>43</sup> Baker, 1931a, p. 284.

<sup>44</sup> Or very early Wabash. The terms are relative of course, and vary according to the area concerned and the time estimates of different authorities. What the writers are trying to suggest is a dating in the neighborhood of 12,000 to 15,000 years ago.

has evoked storms of controversy and has taxed the abilities of critical and careful men. They are also aware that to conclude this problem the most varied research will be needed. Nevertheless, when it is remembered that the number of deeply buried and genuinely authenticated finds of this nature appear to be yearly growing, it would seem that to propose a late Pleistocene dating for this site is not too radical. Though many puzzling facts remain to be interpreted and future work in this area will undoubtedly permit the drawing of less tentative conclusions, the writers feel confident that such a dating will not prove to be far wrong, and, if modified, is much more likely to be extended downward than upward. In any event, if the present writers succeed in again arousing the bickering interest, often extending to billingsgate, which, in the past, has lent so much color to anthropological activities in this field, they will consider that the history of the Scottsbluff quarry has been well worth recording.

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FRITZ GRAEBNER:

MARCH 4, 1877 TO JULY 13, 1934

By JULIUS E. LIPS

SURELY I will be forgiven if my account of Graebner carries a somewhat personal note, but as his colleague for years and later as his successor both in the chair at the University of Cologne and as director of the Rautenstrauch-Joest-Museum, I was connected with him above all by ties of personal friendship. So the wire telling of his death, which I received in New York, was of all the news that reached me from the Third Reich that which touched me most deeply.

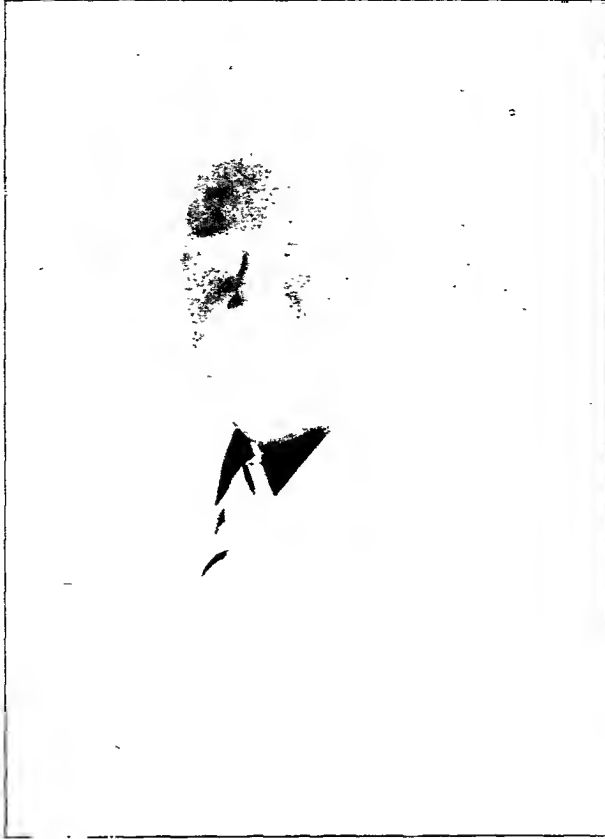
Graebner's passing impended for years, as he had his first stroke when only forty-eight. To friends who visited him in Berlin during the last years, he presented a moving sight. So death surely appeared to him as a deliverer, leading him away into that unknown land which he described in his works so often—be it the sky and stars of the Australians or Maui's land of fire, the eternal hunting grounds of Manitu, or the Christian heaven. Who knows? It is certain only that death did not mean to him the tormenting uncertainty put by Shakespeare into Hamlet's mouth:

But that the dread of something after death,  
The undiscover'd country from whose bourn  
No traveller returns, puzzles the will,  
And makes us rather bear those ills we have  
Than fly to others that we know not of?

Graebner knew too many heavens and hells of mankind, too many conceptions of life after death, to attach too much importance to his own fate. When the question of his ideas of a life after death came up, he used to answer with words of Du Bois-Reymond: "Ignoramus ignorabimus."

Graebner came to ethnology from history. Like most of the leading German ethnologists, he had studied first a science other than anthropology before turning to the special field of ethnology. This study of history, in which he was taught by von Treitschke but principally by Scheffer-Boichorst, determined his methodological attitude in his later special field. Besides history, he was most interested in Slavic languages and botany. Possibly he came to the study of botany through his brother, Paul Graebner, a student and collaborator of Ascherson. It was also from his brother that he heard, one day in the spring of 1899, that an assistantship was open at the Museum für Volkerkunde in Berlin. He set out, even though unable to show any scientific achievements, to present himself to the omnipotent

of the Berlin museum, Professor von Luschan. Graebner often described to me that day which became so significant for him; how accident—he had beforehand no interest in anthropology—and his economic circumstances brought him into the Berliner Völkermuseum, and thus into ethnology. When Weule left for Leipzig, a position became open, and Luschan accepted



FRITZ GRAEBNER

the young student of history as assistant with the words: "History is no mean preparation for anthropology." During his first period at the museum Graebner visited the seminars of Professor von Richthofen, the genial geographer; he industriously catalogued specimens under von Luschan; and published as his first scientific works in 1901 his dissertation, "Boh-

mische Politik vom Tode Ottokars II bis zum Aussterben der Přemysliden" and a small study on "Die Bedeutung des Wortes Heide"—an indication that his interest lay in a field very different from anthropology.

These first studies of Graebner, together with his purely cataloguing activity—of which he often complained—explain in large part his later scientific field of interest. During these years of work in the Berlin museum he came strongly under the influence of Ankermann, his elder by half a generation, who gave Graebner the stimulus to outline in a lecture the culture stratification of Oceania in its historic depth.

In the winter of 1904 there were given at the Anthropologische Gesellschaft of Berlin two lectures which we may note as the foundations of the "kulturhistorische" school (Ankermann: *Kulturkreise und Kulturschichten in Afrika*; Graebner: *Kulturkreise und Kulturschichten in Ozeanien*). Graebner, in his lecture on "Kulturkreise und Kulturschichten in Ozeanien," pointed to Frobenius and Ankermann as his stimulators and predecessors. It is interesting to note, as one reads these old reports, that Frobenius, who was present at the lecture, was not at all in harmony with the synthesis of Graebner, and that the two lectures had no strong repercussions in the next few years. Graebner himself had grave doubts whether he should give the talk at all. He was not at all sure of his case, and it was again a work of accident that the lecture and its subsequent publication became a reality.

In 1906 Graebner went to Cologne, to the newly founded Rautenstrauch-Joest-Museum, following the call of Foy. He headed the museum as director after the retirement of Foy on July 1, 1925. Graebner's chief works fall into this period at Cologne. Foy himself had already followed roads similar to those of Ankermann and Graebner, especially in his article "Schemelartige Kokosnussschaber" (1904). He sought in Graebner a collaborator of similar bent, but the relation between the two scholars never became a warm friendship. It was to Foy's credit that he kept Graebner occupied with museum work as little as possible and that he gave a free rein to his efforts in publication. During this period there appeared Graebner's studies on the "Soziale Systeme in der Südsee," "Volkerkunde der Santa Cruz Inseln," "Handel bei Naturvölkern," "Zur australischen Religionsgeschichte," "Wanderungen und Entwicklung sozialer Systeme in Australien" and most important of all, "Die melanesische Bogenkultur und ihre Verwandten," in which Graebner developed in further detail and gave a methodological foundation to the inquiry outlined in his Berlin lecture. It was clear—and Foy urged Graebner continuously in this respect—that his culture-historical studies called for the theoretical justifi-

cation of his "Methode der Ethnologie." This appeared in 1911, accompanied by the scientific approval of Foy: since which time anthropological literature speaks of a culture-historical school of ethnology—of Cologne.

In "Ethnologica," the periodical of the Cologne museum, there appeared a further series of such studies as "Der Erdfen in der Südsee," "Hängematten aus Neuguinea," "Zur Kulturgeschichte der Melville-Inseln," "Gewirkte Taschen und Spiralwulstkörbe in der Südsee," "Amerika und die Südsee-Kulturen," and in the third volume of "Ethnologica," edited by myself, "Kopfbänke," "Buschmesser," "Gerät oder Waffe der Blackfoot-Indianer?" and "Betel und Koka."

Graebner spent the war period in Australia, where he had been invited by the Australian government to an international congress of anthropologists. He was interned there for five years, in part in solitary confinement, because there was found, unwisely hidden in his shoe, a Baedeker map of the South Seas. It was assumed that he intended to use this map in flight. But Graebner utilized this period of imprisonment for scientific studies, chiefly linguistic. The fruit of these studies was two papers, "Thor und Maui" and "Alt- und neuweltliche Kalender."

In continuation of his struggle for the Kulturkreismethode of which he was the pioneer, and especially in defense against the accusation of being purely a museum scientist, he wrote "Ethnologie," based on his methods, in order to demonstrate the practical application of his scientific conception to a synthesis of the whole culture of mankind. He attempted a philosophical proof of this method in his last completed volume, "Das Weltbild der Primitiven." His last scientific contribution—but one already strongly impaired by his illness—is his short paper in the Schmidt-Festschrift.

In 1928 Graebner retired and moved to Berlin, where he stayed until his death.

Graebner's significance in anthropology will be always connected with his methodological work. Whatever one may hold of the Graebnerian method, the fact cannot be denied that it has influenced in a most significant manner not only anthropology but also prehistory and history during the last twenty-five years. It would mean almost writing a history of anthropology of the last quarter century if one wanted to make clear the influence of Graebner on anthropological research in the various countries. In judging Graebner's studies it is important to remind oneself that the author was a South Seas specialist in the first instance, that he came from the field of history and that, thirdly, he was gifted with "creative synthesis." The relatively clear culture stratifications in the South Seas, the proof by Ankermann of their parallels in Africa, the linguistic and socio-

logical foundation given to the Kulturkreis method by such eminent scholars as Schmidt and Koppers—all this directed Graebner to that synthesis of culture history which he offers in his principal works.

Graebner was often accused of claiming the exclusive validity of his methodology; of looking on the "Methode der Ethnologie" as *the* method of ethnology in general. This is unfair. (The qualifying word "the," as in "die Methode," is found, by the way, only in Foy's introduction, not in Graebner's text.) Graebner often stated that he would immediately accept another method, if it could throw light satisfactorily on more facts in anthropology than his own. His method meant no dogma for him, no doctrine, merely a heuristic principle. Such was the case at least in his later years, although a reading of the "Methode" may perhaps produce a different impression. But Graebner wrote as a pioneer against the then dominant biological-evolutionary concept. Time and again he refused the expression the Kulturkreis *doctrine* and valued his methodological studies merely as suggestions for a synthesis of culture history made as free as possible from presuppositions and guarded against sources of error. The whole being of Graebner was too modest to permit him to believe that only his researches followed the right road: he had only a smile for the news that at some university a lecture course "From Herodotus to Graebner" was given. He was quite amenable to new suggestions and scientific perceptions, even when they overthrew or supplanted some of his own hypotheses. Thus, among other modifications, he revised his interpretation of the allocation of certain traps to certain Kulturkreise and accepted Foy's more universal terminology of the Kulturkreise in place of his own more local terms. He sought to immediately use and incorporate new researches into his theory. It will always be among my proudest memories that he held a seminar in Bonn, in the winter of 1926, on my "Theorie der Erntevölker." He was the first to recognize the economic form of the harvesting peoples as that of the totemistic culture, although I myself was still somewhat doubtful.

Graebner's method is often misunderstood also in another respect, namely, that he meant to explain all identical or similar phenomena of culture through borrowing. There is no a priori denial of the possibility of independent origin for a culture trait, or of the possibility of a case of convergence, but a proof is asked for every specific case. Nor is another accusation which was always held against Graebner correct; that his methods of ethnology were pure museum theory. Certainly, the first conceptions of his method came to him from his intensive museum study of material culture. Yet while recognizing connections in the realm of material culture he never neglected the problems of spiritual culture, as his chief works especially prove.



Graebner's methods, to be sure, have their limitations too. They are neither capable of solving the psychological processes involved in the invention of a culture element and questions of the nature of development, nor of unravelling problems of causal interrelationships. The neglect of the dynamics of culture development and the refusal to consider psychological interpretations brought many a scholar to justified criticism of him. I myself attempted to complement and develop the culture-historical method in this respect toward a "dynamic anthropology" ("Ethnopolitik und Kolonialpolitik," *Koloniale Rundschau*, Schlussheft, Berlin, 1932, p. 530 ff. *Tagungsberichte der Gesellschaft für Völkerkunde*, 1929, Leipzig, 1930). It was clear to Graebner that he could, with his methods and with his formulation of the *Kulturreise*, give merely the foundations for further researches; that many a concluding chapter and revision would become necessary in the course of time by reason of new materials and new insights.

But all these objections will never be able to diminish the fundamental merit of Graebner's work. No doubt he will influence and fertilize anthropological work even more in the future, at least where monographic treatments are not concerned.

Graebner was engaged with problems of American anthropology at various times. This appears especially in his papers "Die melanesische Bogenkultur und ihre Verwandten," "Amerika und die Südsee-kulturen," "Krückenruder," "Alt- und neuweltliche Kalender," "Betel und Koka," and in the American section of his "Ethnologie." He himself was keenly aware of the difficulties of ranging the American complexes in his theory, and when it came to speaking of the North American culture stratifications specifically, he often talked with a humorous horror. Nevertheless it seems to me that the difference between the point of view of American ethnologists and Graebner's conception is not so great, if one takes into consideration the critical remarks offered above.

Strong as was—and is—the influence of Graebner as a writer and leader of a school of anthropological thought, just so weak was his effectiveness as a university teacher. In spite of his charming and always helpful personality, it must have been due to the unfavorable circumstances at the University of Bonn that his personal effectiveness was so limited. In fact he trained only one candidate for the doctor's degree during a teaching period of years. This young man, who takes pride in pointing to himself as the only student of Graebner, was refused as a lecturer by the University of Bonn on account of insufficient qualifications—which is much to be regretted for Graebner's sake. It was always a pleasure to hear Graebner, whether in lecture or in discussion, and to admire his extraordinary knowledge.

As a museum man, in his earlier years Graebner adopted in general Foy's conception of looking upon a museum of anthropology as a museum for scientists solely. In later years he was anxious, in contrast to Foy, to attract the public to a larger extent. He inclined in various ways toward freeing the displays from the old rigid geographical principle. Nor did he reject the notion of a separation into display and study collections, as long as both parts were housed under the same roof. Here too he fell in with the newer aims of anthropological museum methods, and it is entirely incorrect to hold his earlier, different stand against him. Unfortunately lack of funds, chronic since the war in German museums, did not permit the realization of his plans.

The museum heritage of Graebner is administered today by two students without academic degrees. Their sole qualification is membership in the National Socialist party. In an official document of these gentlemen even the manner of display in the Rautenstrauch-Joest-Museum, dating from Foy and Graebner, is characterized as "Marxist." The German museum ethnologists are silent in the face of this, as they are silent in the face of the racial nonsense or even support it. They would be silent even in the face of a dolichocephalic dictionary. They only speak now of a new "guild honor," as they conceive it.

Graebner would, no doubt, have taken a different position, just as he spoke out against the Nordic theory in the Indogermanic question in his "Melanesische Bogenkultur" and in "Thor und Maui." For him material welfare was in any case second to scientific conviction. Since he did a thing for its own sake, without consideration for his own person, he was a typical representative of that Germany and of that German scholarship which once excited the admiration of the world.

COLUMBIA UNIVERSITY  
NEW YORK CITY

## REPORT

### PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR THE YEAR ENDING DECEMBER 1934

The American Anthropological Association held its thirty-third annual meeting at the United States Bureau of Mines, Pittsburgh, Pa., on December 27-29, 1934, in conjunction with Section H of the American Association for the Advancement of Science and with the American Folk-lore Society.

#### COUNCIL MEETING, DEC. 27, 5:00 P. M.

President Fay-Cooper Cole in the chair. The minutes of the Columbus meeting, 1933, were not read, but were approved as printed in the *AMERICAN ANTHROPOLOGIST*, Vol. 36, No. 2, 272-85.

#### REPORT OF SECRETARY

The President appointed the following committees and representatives during the year:

Committee on Honorary Memberships: H. J. Spinden, C. Wissler.

Committee on Joint Meeting of the American Anthropological Association, Pacific Division with Section H of the American Association for the Advancement of Science: A. L. Kroeber (chairman), B. Cummings, E. Gunther, F. W. Hodge.

Nominating Committee: R. Linton (chairman), R. B. Dixon, W. D. Strong.

Program Committee: W. M. Krogman, R. Redfield, J. M. Cooper.

Delegates to First International Congress of Anthropological and Ethnological Sciences: F. Boas, M. Andrade, alternates, R. H. Lowie, J. J. Williams

The Executive Committee approved the establishment of a clearing house and laboratory for the comparative study of stone implements, which had been approved in principle by the American Anthropological Association at the Annual Meeting, December 28, 1933, and referred to the Executive Committee for action. Dr. Carl E. Guthe was asked by the President to act as a committee of one to work out the details.

The Executive Committee authorized the publication of future *Memoirs of the American Anthropological Association* as supplements to the *AMERICAN ANTHROPOLOGIST*.

The Pacific Division of the American Anthropological Association met with Section H of the American Association for the Advancement of Science in Berkeley, June 20-22, 1934. About thirty-five papers were read, dealing largely with problems of the Southwest, Pacific coast, ethnological theory, and physical anthropology. Attendance at the four sessions averaged around forty or forty-five.

The membership of the Association as of December 1, 1934, is as follows:

Number of members		937
Honorary . . .	0	
Life . . .	10	
Regular	910	
Exchanges	17	
Deceased during 1934		9
Dropped " "		39
Resigned " "		37
Admitted " "		106

The Association has lost by death during the year nine members: (AAA) R. B. Dixon, William J. Holland, Richard Jäschke, Dean Mason, G. H. Perkins, H. C. Warren; (CBS) Berthold Laufer; (AES) F. Gallatin, E. F. Hyde; (ASW) Felix Neumann

The Secretary attended the 10th Annual Conference of the Secretaries of the Constituent Societies of the American Council of Learned Societies, at Washington, D. C., January 25, 1934.

Respectfully submitted,

JOHN M. COOPER, *Secretary*

It was voted that the Secretary's report be accepted.

#### REPORT OF TREASURER

The present bank balances in the four funds of the Association stand as follows:

Regular Fund	\$4,663.53
Permanent Fund	3,266.91
Index Fund	1,164.31
Memoirs Fund	1,087.25

This makes a total of \$10,182.00, of which \$8,920.70 is drawing interest in four savings accounts (New Haven Savings Bank, Connecticut Savings Bank, Second National Bank of New Haven, and First National Bank & Trust Co.) The \$4,663.53 of the Regular Fund is divided between a checking account with a balance of \$1,261.30 and a savings account of \$3,402.23.

The large size of the Regular Fund (some \$700 more than a year ago) indicates a definitely favorable balance although deceptive to the extent that the 1934 printer's bills for Number 4, AMERICAN ANTHROPOLOGIST have not been paid. Of the \$1,733.26 unexpended balance of the 1934 budget appropriation, \$1,000.00 should be reappropriated to meet these bills. It is extremely gratifying to report that whereas there was an actual deficit for the year 1933, there will be a net surplus for the year 1934 of approximately \$1,000.00.

#### REGULAR FUND

##### *Gross Receipts*

Balance on hand December 1, 1933			\$3,950.14
Membership dues:			
American Ethnological Society	.	\$895.25	
Anthropological Society of Washington		190.00	
Central States Branch	.	330.00	
American Anthropological Association			
1931-32	\$	32.00	
1933		167.40	
1934		2,919.58	
1935		154.75	3,273.73
			4,688.98
Sale of Publications			329.24
Reimbursements			272.70
Interest (Regular Fund only)			144.81

**\$9,385.87**

*Gross Disbursements*

American Anthropologist:

George Banta Publishing Company

Printing.. ..	\$2,444.30	
Illustrations .	273.08	
Distribution .	211.20	
Storage, Insurance	68.50	
Reprints .	425.30	\$3,422.38

Editor's expenses	638.02	
Treasurer's expenses	580.94	
Secretary's expenses	81.00	\$4,722.34

Cash on hand, November 30, 1934	4,663.53	
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\$9,385.87

*Resources*

Cash on hand, November 30, 1934		\$4,663.50
Due from sales		\$38.30
Due from dues.		
1934: American Anthropological Association	\$294.00	
American Ethnological Society	63.75	
Central States Branch .	40.00	397.75

Due from reimbursements	64.92	500.97
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\$5,164.47

*Liabilities*

Membership dues for 1935 already paid	\$ 154.75	
Net excess resources over liabilities	5,109.72	\$5,164.47

## PERMANENT FUND

*Receipts*

Balance, December 1, 1933 .		\$3,440.31	
Interest on savings . .	\$104.94		
Interest on three bonds	12.75	117.69	\$3,558.00

*Investments*

Liberty Bonds (three)	. \$ 291.09	
Cash in savings account	3,266.91	\$3,558.00

## INDEX FUND

Balance, December 1, 1933 .	\$1,124.53	
Interest on savings account	39.78	\$1,164.31

Cash in savings account, November 30, 1934		\$1,164.31
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## MEMOIRS FUND

*Receipts*

Balance, December 1, 1933			\$1,032.58
Interest on savings account	\$42.67		
Gift of Mrs Elsie C. Parsons (royalty)	12.00		
Gift of Mrs Elsie C. Parsons (for Memoir 39)	425.88	480.55	\$1,513.13

*Disbursements*

Memoir, Number 39 (Parsons):			
Geo. Banta Publishing Co.	\$ 425.88		
Cash in savings account, Nov. 30, 1934	1,087.25		\$1,513.13

## NET EXPENDITURES AGAINST 1934 BUDGET

	<i>Allowed</i>	<i>Spent</i>	<i>Balance</i>
Secretary's expenses	\$80.00	\$71.68	\$8.32
Editor's expenses:			
Editor's assistant	480.00	445.60	34.40
Office expenses	60.00	192.42*	-132.42
	540.00	638.02	98.02
Treasurer's expenses			
Treasurer's assistant	360.00	395.40*	-35.40
Office expenses	90.00	107.27*	-17.27
Membership expenses	25.00	11.40	13.60
	475.00	514.07	-39.07
American Anthropologist.			
Printing	2,890.00	2,437.93	452.07
Illustrations	300.00	273.08	26.92
Reprints	240.00	211.16	28.84
Distribution	180.00	211.20	-31.20
Insurance; storage	70.00	68.50	1.50
	3,680.00	3,201.87	478.13
Out-of-print publications			
American Council of Learned Societies	25.00	25.00	
To meet unpaid 1933 bills	1,262.02		
To cost of transmittal of Editor's and Treasurer's offices	125.88		
Totals	\$6,187.90	\$4,450.64	\$1,737.26

\* These items include expenditures for transmittal of Editor's and Treasurer's offices

## REGULAR RECURRENT INCOME AND EXPENDITURES

	<i>Net Income</i>			
	<i>1931</i>	<i>1932</i>	<i>1933</i>	<i>1934</i>
Memb. dues from aml societies at \$5	\$1,198.50	\$1,324.25	\$1,166.50	\$1,415.25
Memb. dues collected directly at \$6 (less subscription commissions) (AAA)	3,919.92	3,703.32	3,263.16	3,273.73
Total dues	5,118.42	5,027.57	4,429.66	4,688.98
Sales of publications	729.21	570.22	239.56	329.24
Interest and royalty (Regular and Memoirs Funds)	153.00	153.72	164.27	199.48
Total	\$6,000.63	\$5,751.51	\$4,833.49	\$5,217.70
	<i>Net Expenditures</i>			
American Anthropologist, printing and illustrations:				
No. 4 of preceding year	\$104.42			\$872.61
Nos. 1-3 of year			\$2,522.40	1,838.40
Nos. 1-4 of year	3,349.55	\$3,462.33		
Total	\$3,453.97	\$3,462.33	\$2,522.40	\$2,711.01
Anthropologist and Memoirs (distrib., stor- age, insur., net costs gratis reprints)	583.77	465.81	475.38	490.86
Memoirs: printing and illus. paid by Assoc.			67.02	
Total account publications	\$4,037.74	\$3,928.14	\$3,065.00	\$3,201.87
Sec'y, Treas., and Ed.'s offices	1,272.47	1,094.03	1,102.85	1,223.77
Reprinting and purchase out-of-print publ.	181.31	296.22	165.82	
Total	\$5,491.52	\$5,318.39	\$4,333.67	\$4,425.64
Surplus carried over	\$509.11	\$433.11	\$499.82	\$792.06

Respectfully submitted,

CORNELIUS OSGOOD, *Treasurer*

It was voted that the Treasurer's report be accepted, subject to the findings of the Auditing Committee.

The President appointed the following Auditing Committee: Edward Sapir and Clark Wissler.

The President appointed the Executive Committee of the American Anthropological Association to serve as a Budget Committee and to report to the Association at the annual meeting, December 28, 1934.

## REPORT OF THE AUDITING COMMITTEE

The undersigned, appointed as Auditing Committee by the Council of the Association, report that they have examined the Treasurer's accounts as submitted for the fiscal year 1934, and find them correct.

CLARK WISSLER AND EDWARD SAPIR

## REPORT OF EDITOR

I would like to take advantage of this, my first report, to lay before you at the outset some aims which have guided my activities, before proceeding with routine matters. As aims they have no particular novelty, but it is hoped that this statement will bring such cooperation from our members as will translate them into realities.

1. An effort is being made to secure articles presenting broad views of fields in which the accumulation of detail has shut off all save the specialist, and of others now neglected or tangential to our customary interests. So far as they stress new and fertile views, they should provide a quickening of investigation.

2. Reviews, it is hoped, will become less complacent, and discussion more ready and sharp. By insisting on brevity, we can have critical commentary on many more items than heretofore.

3. A determined effort is being made to widen the range of subjects and of contributors and reviewers. Some of the older men, who have not participated in years, but on whose maturity we must depend for guidance, have shown a ready willingness to cooperate, and we have secured the support of competent colleagues abroad.

4. The quality of articles is fixed, in the long run, by what is volunteered: in large part your Editor can only select. But it is possible for the Editor to make a positive contribution by asking authors to make their presentations as compact as is consistent with clarity. The Editor holds a clear mandate from the Columbus meeting to eliminate loose writing and unimportant matter. Again, I am happy to acknowledge the patient help of our contributors to this end. By their cooperation we saved something like 113 pages of print space in the 1934 volume and definitely improved the articles.

To realize these aims—which I trust we hold in common—we must have the willing help of our whole group. There is no dearth of papers submitted for publication. For the sake of balance, however, we must have more from the ethnologists and more articles of broad or theoretical interest.

This year's *AMERICAN ANTHROPOLOGIST* is a volume of 642 pages as against 804 in 1933. Two lengthy Memoirs are in press: No. 42—"Walapai Ethnography," edited by A. L. Kroeber (about 300 pp.), and No. 43—"The Pueblo of Santo Domingo, New Mexico," by Leslie A. White (about 225 pp.). These are financed by special gifts and will appear early in 1935.

The smaller size of this year's volume was determined by the curtailed budget adopted at the Columbus meeting and by representations to the Editor throughout the year that loss of membership was very great—which happily is not the case.

Operating costs have been reduced by securing more compactness in articles, by savings on reprints, by combining illustrations to make single engravings, and by a substantial discount on engravings passed on by our printer. An increase in office expenses was due in part to the larger number of books and papers sent out for review.

The Editor respectfully requests further assistance. It does not seem to be generally recognized that producing and distributing our publications is a business: that preparation of manuscript and the like is an essential and substantial part of the cost of publication. The \$40 per month allotted for the Editor's assistant will buy only a few hours of the time of a mere typist. More help is needed. A sympathetic friend came to our rescue, providing further assistance by matching this sum through the second half of the year, but this contribution ends at the present moment.



I wish to express here my obligation to Dr Robert H. Lowie for continuing his editorial services until the first issue of this year was through the press.

Respectfully submitted,

LESLIE SPIER, *Editor*

It was voted that the Editor's report be accepted.

It was voted that the Committee on Honorary Memberships be continued and that the Committee be requested to present a report at the next annual meeting upon the pros and cons of the principle of honorary membership.

It was voted by the Council that the 106 new applicants for membership in the American Anthropological Association, whose names will appear in the next printed list of members, be elected to regular membership in the Association.

The President appointed the following Committee on Resolutions. A. E. Jenks (chairman), G. Herzog, L. A. White.

It was voted:

That the proposal of Dr Biren Bonnerjea to prepare an index of the *AMERICAN ANTHROPOLOGIST* from the beginning of Volume 31 to the end of Volume 36, and hereafter to prepare an annual index, such indices to be transmitted to the Editor, be accepted on the conditions suggested by Dr Bonnerjea.

That the following procedure for the nomination of officers of the American Anthropological Association be adopted. Within three months of his election the President shall appoint a Nominating Committee of three members and transmit the names of the personnel of this Committee to the Editor who shall publish the names in the number of the *AMERICAN ANTHROPOLOGIST* which appears next after the time of election, with an invitation for suggestions; after considering such suggestions the Nominating Committee shall report its slate to the Council which shall pass on the recommendations, with such changes as are deemed advisable, to the annual meeting.

That the invitation extended by Dr Moorehead to meet at Andover in 1935 be referred to the Executive Committee for action.

That the American Anthropological Association meet in 1936 with Section H of the American Association for the Advancement of Science at Washington, D. C., and that arrangements be made with the American Association for the Advancement of Science for the meetings to be held either in the same hotel as is chosen for headquarters or in convenient proximity thereto.

That the Executive Committee be empowered to act on the proposal for standardization of anthropometrical methods.

#### ANNUAL MEETING, DECEMBER 28, 2:00 P.M.

President Fay-Cooper Cole in the chair. The Nominating Committee (Linton, chairman, Strong) presented its report. After the presentation thereof, the following officers, Council members, and representatives to councils and associations were elected:

President, Robert H. Lowie  
First Vice-President, Nels C. Nelson  
Second Vice-President, Matthew W. Stirling  
Secretary, John M. Cooper  
Treasurer, Cornelius Osgood  
Editor, Leslie Spier

Associate Editors, M. J. Herskovits, Cornelius Osgood, F. H. H. Roberts, Jr., Frank G. Speck

Executive Committee, Fay-Cooper Cole, Carl E. Guthe, W. D. Wallis

#### Council

New Members For term 1935-38 J. Gillin, J. B. Griffin, A. T. Hansen, C. W. M. Hart, F. Hawley, W. W. Hill, J. Lips, M. E. Opler, H. Turney-High, M. Whelpley

The following members of the Council whose term expired December 31, 1934, were re-elected for the term 1935-38.

C. Amsden, H. Beyer, P. A. Brannon, J. H. Breasted, K. M. Chapman, F. E. Clements, H. S. Colton, J. M. Cooper, T. Deuel, K. P. Emory, G. Engerrand, R. Flannery, C. D. Forde, Lucy Freeland, A. H. Gayton, M. R. Gilmore, H. S. Gladwin, C. D. Gower, W. K. Gregory, S. J. Guernsey, C. E. Guthe, H. U. Hall, E. S. Handy, J. P. Harrington, C. L. Hay, L. W. Jenkins, I. T. Kelly, A. V. Kidder, O. LaFarge, A. Lesser, E. M. Loeb, G. MacGregor, T. Michelson, L. H. Morris, C. B. Osgood, H. Powdermaker, V. F. Ray, W. A. Ritchie, F. H. H. Roberts, Jr., H. C. Shetrone, J. B. Stetson, Jr., S. Tax, T. W. Todd, W. L. Warner, G. Weltsh, G. D. Williams, F. R. Wulsin.

Members for term to 1937 R. B. Bean, H. B. Collins, Jr., B. Cummings, F. Densmore, A. C. L. Donohugh, G. G. Heye, E. A. Hooton, H. Kelley, H. W. Krieger, W. M. Krogman, S. K. Lothrop, R. Mackaye, W. C. McKern, R. L. Olson, J. E. Pearce, R. Redfield, H. L. Shapiro, F. G. Speck, L. Spier, E. Spinden, H. J. Spinden, W. D. Wallis, H. N. Wardle.

Members for term to 1936 S. A. Barrett, M. W. Beckwith, R. F. Benedict, C. W. Bishop, C. S. Coon, C. B. Davenport, G. R. Fox, E. F. Greenman, G. B. Grinnell, E. Gunther, B. Haile, A. I. Hallowell, D. G. Haring, M. J. Herskovits, G. Herzog, A. E. Jenks, N. M. Judd, E. Kopta, F. de Laguna, R. Linton, J. C. McGregor, A. F. Means, H. P. Mera, N. Morss, J. L. Nusbaum, B. Oettking, L. M. O'Neale, L. Outhwaite, A. C. Parker, V. Petrullo, A. R. Radcliffe-Brown, G. Reichard, H. H. Roberts, M. J. Rogers, L. Satterthwaite, Jr., J. H. Steward, M. W. Stirling, R. J. Terry, R. Thurnwald, R. Underhill, A. Woodward.

Members for term to 1935 H. M. Allyn, M. Andrade, W. C. Bennett, H. Bingham, L. Bloomfield, B. Bonnerjea, P. H. Buck, R. Bunzel, D. Byers, W. H. Claflin, W. B. Cline, C. H. Danforth, D. S. Davidson, H. Field, R. Fortune, L. Foster, E. W. Gifford, E. A. Golomshtok, W. D. Hambly, M. R. Harrington, L. Havemeyer, J. N. B. Hewitt, E. B. Howard, M. Jacobs, D. Jenness, C. R. Keyes, C. T. Loram, R. H. Lowie, T. F. McIlwraith, R. McKennan, P. S. Martin, J. A. Mason, M. Mead, C. B. Moore, W. K. Moorehead, G. P. Murdock, N. C. Nelson, P. H. Nesbitt, F. M. Olbrechts, E. C. Parsons, E. K. Putnam, O. Ricketson, H. B. Roberts, J. T. Russell, Jr., E. Sapir, A. H. Schultz, D. Scott, F. M. Setzler, H. I. Smith, W. D. Strong, G. C. Vaillant, H. Webster, A. E. White, L. A. White, C. C. Willoughby, W. J. Wintemberg, G. Woodbury.

Past Presidents (*ipso facto* members of the Council): F. Boas, F.-C. Cole, F. W. Hodge, W. Hough, A. Hrdlička, A. L. Kroeber, G. G. MacCurdy, M. H. Saville, J. R. Swanton, A. M. Tozzer, C. Wissler.

Representative to Social Science Research Council: R. Redfield.

Representatives to National Research Council: Ruth Benedict, H. L. Shapiro, W. D. Strong

Representatives to Section H, A.A.A.S.: A. Hrdlička, W. Lloyd Warner

Representative to American Council of Learned Societies: F. Boas

The Budget Committee presented the following budget recommendations for 1935:

## BUDGET FOR 1935

1 Secretary's expenses		\$100 00
2 Editor's expenses:		
Editor's assistant	\$960 00	
Office expenses	125.00	1,085 00
3 Treasurer's expenses:		
Treasurer's assistant	600.00	
Office expenses	90 00	
Membership charges	25 00	715 00
4 American Anthropologist:		
Printing	2,300 00	
Illustrations	400.00	
Reprints	240.00	
Distribution	250 00	
Insurance, storage, back numbers	70 00	3,260 00
5. Anthropological Reprint Series		150 00
6 American Council of Learned Societies		25 00
Total for 1935 expenditures		\$5,335 00
7. To meet unpaid 1934 bills		1,000 00
8. To publish Memoir 44 from surplus in General Fund		275 00
		<hr/> \$6,610 00

In presenting this budget the Committee presented the following comment. It has been recommended by the Treasurer that an audit for the financial accounts of the Association be made by a certified public accountant. This recommendation of the Treasurer has been approved by the Executive Committee at a meeting held on December 28, 1934. The Executive Committee proposed that the cost of the audit be allowed and that the audit be made if the Auditing Committee should deem it advisable.

It was voted that the budget as submitted by the Budget Committee be accepted and that the recommendation of the Treasurer and the proposal of the Executive Committee as regards the audit be approved.

The following resolution presented by the Committee on Resolutions (Jenks, chairman, Herzog, White) was adopted:

*Resolved*, that the American Anthropological Association record its sense of loss by death during the past year of the following members: Roland B. Dixon, F. Gallatin, William J. Holland, E. F. Hyde, Richard Jäschke, Berthold Laufer, Dean Mason, Felix Neumann, G. H. Perkins, and H. C. Warren. In Dr Laufer's death anthropology has lost one of its great leaders and America one of the world's foremost Orientalists. Dr Dixon's long and fruitful record both in education and in research is well known to all. His passing will be a keen loss not only to his many students and colleagues but to the sciences of man everywhere.

It was voted that the decision as to place of meeting for December, 1935, be left to the Executive Committee and that the Association meet with Section H of the American Association for the Advancement of Science at Washington, D. C., in December, 1936.

It was voted that a committee be appointed to codify existent practices of the Association with a view to drafting a set of by-laws to be presented for adoption at the next annual meeting. The President appointed the following committee to codify such practices: Sapir, chairman, Collins, Guthe.

# PROGRAM

THURSDAY, DECEMBER 27

9.30 A.M.

- 1 Folk-lore from the Hills of Schoharie County, New York. Emelyn E. Gardner, Wayne University, Detroit
- 2 The Hiatus or "non sequitur" in African Folk-tales (Lantern) Agnes C. L. Donohugh, White Plains, N. Y.
3. Technics in North American Indian Poetry. George Herzog, Institute of Human Relations, New Haven
- 4 Some aspects of Eastern Cree Recreative Culture. Regina Flannery, Catholic University of America
5. An Outline of Seneca Ceremonies at Coldspring Longhouse. William N. Fenton, Yale University.
- 6 Indian Elements in Zapotecan Folk Tales (Lantern) Elsie Clews Parsons, Harrison, New York
7. Southwestern Folksongs. A. L. Campa, University of New Mexico, Albuquerque
8. Blanc Sablon, A Study of an Isolated Labrador Fishing Village. (Lantern.) O. W. Junek, Central YMCA College, Chicago.
9. A Note on Labrador Eskimo Masking and Clowning. (By Title) F. G. Speck, University of Pennsylvania
- 10 Tonal Patterns in the Mendé Language. Ethel Aginsky, New York City.

2:00 P.M.

- 1 Stone Hatchets and Their Variants (Lantern) W. K. Moorehead, Phillips Academy, Andover, Massachusetts.
- 2 A Method of Classifying Stone Cutting Tools. (Lantern) Frederick Johnson, Milton, Massachusetts
- 3 The Relation of Physical Types to Mound Areas (By Title) George Woodbury, Harvard University
- 4 The Zones and Strata Theory, A Biological Classification of Races (Lantern) Griffith Taylor, University of Chicago

7:30 P.M.

- 1 Ancient Cultural Contacts between Mexico and Peru. (Lantern) Herbert J. Spinden, Brooklyn Museum.
- 2 How to Approach the Problem of Primitive Law (By Title) Julius F. Lips, Columbia University.
3. Functionalism in Social Anthropology. Alexander Lesser, Columbia University.

- 4 The Mechanics of Kinship Bernard W. Aginsky, New York City
- 5 The Grave-post among the Tiwi of North Australia C. W. M. Hart, University of Toronto
- 6 Spanish and Indian Traits of a Zapoteco-speaking Town (By Title) Elsie Clews Parsons, Harrison, New York
- 7 The Netted Snowshoe and the Question of its Asiatic Origin and American Elaboration (Lantern) D. S. Davidson, University of Pennsylvania
- 8 Northern Algonquian and Athapaskan Land Tenure John M. Cooper, Catholic University of America.
9. Caste in the Present Situation (By Title.) George W. Briggs, Madison, N. J.

## FRIDAY, DECEMBER 28

## 10:00 A.M.

- 1 Culture Trait and Behavior Pattern Edward Sapir, Yale University.
- 2 The Participation of the Individual in Culture. Ralph Linton, University of Wisconsin
- 3 The Study of the Individual in Culture. Margaret Mead, Columbia University.
- 4 Clinic and Culture H. Scudder Mekeel, Cambridge, Massachusetts
5. The Effect of Social Change in Yucatan on the Development of Magic Asael T. Hansen, University of Chicago
6. Culture Change at Mitla, Oaxaca, Mexico Elsie Clews Parsons, Harrison, New York.

## 2:00 P.M.

- 1 Time Perspective in the Northern Plains (Lantern) W. D. Strong, Bureau of American Ethnology.
- 2 Prehistoric Relationships in the Northern Mississippi Valley. (Lantern) Thorne Deuel, University of Chicago
- 3 Dendrochronology in the Mississippi Valley. Florence Hawley, Albuquerque, New Mexico.
- 4 A Consideration of the Culture Sequence and Chronology of the New York Area. (Lantern) W. A. Ritchie, Municipal Museum, Rochester, N. Y.
- 5 Prehistoric Remains in the Yampa Canyon, Colorado, F. Martin Brown, Colorado Biological Survey, Colorado Springs, Colorado.
6. Minnesota's Brown's Valley Man and Artifacts (Lantern) A. E. Jenks, University of Minnesota
7. Madison Buffalo Cliff M. L. Sayre, University of Montana.

## 7:00 P.M.

At the annual dinner of Section II and affiliated societies, Dr T. Wingate Todd of Western Reserve University gave the address of the retiring Vice-President on the subject "Anthropology and Growth."

## SATURDAY, DECEMBER 29

## 10.00 A.M

1. The Physical Anthropometry of White Males with Cardiac Disease (Lantern) Raymond Pearl and Antonio Ciocco, Johns Hopkins University
- 2 A Preliminary Report on English Medieval Crania from Abingdon, Berkshire, and Rothwell, Northamptonshire. (Lantern) Jack Trevor, Northwestern University

3. The Pelvis from Fish to Man: A Study in Paleomorphology. (Lantern) W. K. Gregory, American Museum of Natural History.
4. Some New Observations on Hair Form. (Lantern) Madeline Kneberg, University of Chicago.
5. The Myth of the Comb. (Lantern.) M. Russell Stein, Columbia University.
6. Age-Weight Relations in Negro College Men. (Lantern) K. B. M. Crook, Hampton Institute, Virginia.
7. Human Occipital Third (Spurious) Condyl Viewed as Half of Most Cephalic Vertebral Metamere. (Lantern) T. H. Evans, Freeport, N. Y.
8. Location of Nasion in the Living. (Lantern.) M. F. Ashley-Montagu, New York University.
9. Statistical Studies of Theories of Handedness. Mary M. Roos, Colorado Springs, Colorado.
10. Population Growth. Victor Von Szeliski, National Recovery Administration.

2:00 P M.

1. Some New Sound Recording Apparatus. (With Demonstration.) Helen H. Roberts and Lincoln Thompson, Yale University.
2. Incised Stone Tablets from Burial Mounds. (Lantern) E. F. Greenman, Ohio State Museum, Columbus, Ohio.
3. An Analysis of the Fort Ancient Culture. (Lantern.) James B. Griffin, University Museums, Ann Arbor, Michigan.
4. Psychic Suicide in Primitive and Civilized People. A. A. Brill, New York City.
5. Chief Day-Bway-Wain-Dung's Medicine Bark Parchment, with His Interpretation of the Glyphs Thereon. (By Title.) Albert B. Reagan, Brigham Young University, Provo, Utah.
6. The Tribal Divisions of the Western Cree. David C. Mandelbaum, Institute of Human Relations, New Haven.
7. The Indian Sites of Adams County, Pennsylvania. (Lantern) S. W. Frost, Pennsylvania State College, Arendtsville, Pa.
8. Ethnopolitics and Acculturation. (By Title.) Julius E. Lips, Columbia University.
9. Pomo Law and Social Control. (By Title.) Bernard W. Aginsky, New York City.
10. A New Mixtec Codex. Emma Reh, Washington, D. C.

4:00 P M

Address by Mr John Collier, Commissioner of Indian Affairs

JOHN M. COOPER, *Secretary*

*American Anthropological Association*

## BOOK REVIEWS

### NORTH AMERICA

*The Netsilik Eskimos. Social Life and Spiritual Culture* KNUD RASMUSSEN. (Report of the Fifth Thule Expedition, Volume 8, 1. The Netsilingmiut, 464 pp.; 2. The Utkuhikjalingmiut, 75 pp. Copenhagen: Gyldendalske Boghandel, Nordisk Forlag, 1931.)

*Intellectual Culture of the Copper Eskimos.* KNUD RASMUSSEN. (Same series, Vol. 9. 350 pp. Kr. 12. 1932.)

With the passing of Knud Rasmussen the Eskimo lost their best interpreter, and science the leading student of a fascinating people. The present large and well illustrated volume, one of the fruits of his long trek across Arctic America, is a worthy addition to the great heritage which he left.

The first, and by far the largest portion, deals with the Netsilingmiut or "Seal Eskimos," a group of about 260 persons living in and adjacent to King William Land. Although emphasis is laid on the nonmaterial aspects of life, so intimate and complete is the picture of their adjustment to a hard environment that the work is ethnographic in the best sense. As in his report on the Iglulik, Rasmussen has achieved such a masterly blending of the individual experiences and reactions of his informants that their totality presents a living culture to the reader.

According to the author, the Netsilik Eskimo are probably a branch of the Caribou Eskimo that have moved to the coast. He does not discuss the possibility of the movement having been the other way. At present the region affords no whale or walrus and the little fiord seal and the caribou are the main game animals. The Netsilik divide their life sharply between hunting on land in summer and the sea ice in winter and their mores strongly reflect this dichotomy. A cheerful people, their social rules are entirely for themselves: the outsider is usually regarded as an enemy. Those who believe in the innate kindness of hunters and gatherers will find little to fit their theory in Rasmussen's account of these primitive villages where every man has at one time or another been involved in a homicidal affair. In the light of Amundsen's fervent wish that they might be spared all contact with other Europeans it is paradoxical but cheering that Rasmussen believes their condition to be improved since the arrival of the whites.

In the field of sex mores, as in that of religion, the individual data which the author presents have special value. In the former the psychologist will find interesting data on sexual habits and abnormalities. The sections on religion, taboo, amulets, magic words, life after death, and shamanism are particularly full. In every case the author's conclusions are based on native material rendered in the original or in close translation. The same is true in regard to songs, versed lampoons and mythology, and the volume contains a great deal of material in text. These texts, aligned as they are in association with the subject under discussion, have a living quality

and usability so woefully lacking in many American collections. Occasionally, however, one feels that the literary artist may have influenced the scientist in certain free translations but the reviewer is certainly not one of the very few Europeans qualified to offer correction on such subtle points. Particularly helpful in judging the objectivity of the work is the manner in which the author states his own relationship to the informants. While never intrusive, this factor enables the reader to judge for himself the opportunities enjoyed by the ethnologist and to see where the line between hearsay and observation can be drawn.

The second section is brief and treats of the Utkuhikjalingmiut who number about 170 persons living around the mouth of Back River. Their culture is much closer to that of the Caribou Eskimo and they never go to the sea, though they did so formerly. Their contacts with whites have been extremely limited and the present report is the first detailed account of their ethnology. It deals at some length with the distribution of population based on native maps of their terrain. Caribou hunting and trout fishing are the most important occupations. At present, though they use snow huts, they do not use blubber lamps for heat, their kayaks are made of caribou skins and, like the Netsilingmiut, they have few dogs, due to scarcity of food. Their main deity is the woman who controls all the game animals and there are taboos concerning the differential usage of fish, seal, and caribou products. The caribou have the most numerous rituals. Rasmussen sums up: "To me the Utkuhikjalingmiut will always be the most handsome, elegant and hospitable people I met on that long journey, indeed, the healthiest and happiest I have ever lived with."

On leaving the Netsilingimut or "Seal Eskimo," Rasmussen crossed Queen Maude Gulf and lived for three months with the Umingmaktormiut or "Musk Ox Eskimo." This group, living east of Bathhurst Inlet, is a southeastern division of the Copper Eskimo. They were chosen for study because Stefánsson and Diamond Jenness had made studies of the Copper Eskimo groups to the northwest. Though musk oxen were formerly abundant in the region they are rare today and the Umingmaktormiut rely on caribou in summer and ice hunting in winter. The author states that these Eskimo were the most gifted singers and poets he encountered on his journey, but that their mythology was extremely limited in content. The people are described as a joyous, active and self-assured group. The latter characteristic, which sometimes becomes presumptuous, has gotten them into numerous difficulties with the whites. Quarrels among themselves are usually fomented by jealousy over the women who, due to female infanticide, are less numerous than the men. The latter are at times characterized by a flaming, unreasoning anger that takes to immediate action. Murders are casually committed and many examples are cited, including certain cases that will be more or less familiar to readers of Peter Freuchen's "Eskimo." Rasmussen agrees with Jenness that the so-called "blond Eskimo," comprising a considerable portion of the population, are not the result of early Norse nor later Caucasian admixture but owe their special characteristics to biological conditions which as yet are obscure.

The life cycle among the individual bands is treated in detail. This treatment in turn is based upon careful ethnogeographic studies accompanied by maps drawn



by the informants. Similarly, in discussing their material culture, native drawings of implements are employed. The author points out that the people were well disposed to ethnological research and the bulk of the report, secured in such a short time, bears out this fact. It also forcibly demonstrates the remarkable abilities of the author both as a practical linguist and an ethnologist. The data on shamanism includes observations of seances and an extensive treatment of magic words and shamanistic formulae. There are a large number of texts, and a vocabulary from the most easterly of the Copper Eskimo groups. The use of Anglo-Saxon monosyllables in translations, where most ethnologists perforce use polysyllabic scientific terms, preserves the folk characteristics of the tales. As is true of most of Rasmussen's work, the presentation is living and factual. Other men may use these data for sweeping and penetrating generalizations but when the mosaic of circumpolar culture is finally in place the work of Knud Rasmussen will comprise a goodly section.

DUNCAN STRONG

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*The Archaeology of Cook Inlet, Alaska.* FREDERICA DE LAGUNA. (263 pp., 5 figs., 72 pls. \$3.00. Philadelphia: The University Museum, University of Pennsylvania Press, 1934.)

It is fortunate that the first archaeological investigations in this important region should have been undertaken by so competent a worker as Dr Frederica de Laguna. The present volume presents the results of three seasons investigations, mainly at three large sites in Kachemak Bay. Included also are brief descriptions of many smaller sites, and an interesting section on rock paintings. The skeletal material is described in a separate chapter by Dr Bruno Oettinger.

Dr de Laguna's careful and systematic excavations in the large kitchen middens on Yukon Island and Cottonwood Creek produced evidence of four main periods of what is designated as the Kachemak Bay culture.

The stone industry of the earlier times is characterized by the greater relative importance of chipping, including even the chipping of slate. The use of the stone saw is unknown. The notched stone is conspicuous by its absence, and of the grooved stones, only the type with groove about one end is found. . . . Later, polished slate grows in importance, and is applied to new types, while chipped stone becomes relatively less common. . . . Notched stones suddenly appear in great numbers in the Second Period, with the large type at first predominating, then losing ground, and at last giving way entirely to the small type.

In the bone industry, we must note the importance of the Thule Type I harpoon head in the First Period. . . . In succeeding periods this very primitive harpoon head yields place to more developed types, and becomes less important than the barbed dart head. . . . the labret is found even in the earliest period.

Pottery and copper are rare and restricted to the very last stage of the Third Period. . . .

In many respects, the culture of the last two periods (sub-III and III) was richer than that of the first, even discounting the exaggerated impression of the variety of cultural types which depends in some measure upon the large number of specimens from the Third Period.

The First Period is simpler, and . . . has more points of resemblance to the Arctic Thule culture of Canada. The culture of the Third Period shows a development away from the more typical "Eskimo" pattern towards a more specialized, local complex. This seems to be in part due to the accretion of culture elements peculiar to the North Pacific regions. And yet, throughout its evolution, the Kachemak Bay culture has maintained an individual stamp (pp. 129-31).

The sixty-three pages devoted to an analysis of the cultural material show an alert comprehension of the basic problems of Arctic archaeology. In this section there is a full discussion of the range and relationships of the Kachemak Bay types from which it is seen that the greatest number of resemblances are found in the Canadian Thule culture, the Aleutian Islands, and Port Moller on the Alaska Peninsula, with fewer but significant resemblances among the Salish, on Kodiak Island, Alaska north of the Alaska Peninsula, Kamchatka, and Neolithic Japan. It is significant that many of the most important Kachemak Bay types are restricted to Southwest Alaska (from Prince William Sound to the Alaska Peninsula). It seems to the reviewer that the relationship with the Thule culture is somewhat over emphasized. The elements which are common to the Kachemak Bay and Canadian Thule cultures are for the most part simple, utilitarian types with a wide distribution, and it is difficult to see in these any evidence of the hypothetical Thule or proto-Thule culture which Dr de Laguna suggests as basic in Alaska. However, the author recognizes that the data available for comparison are not all of equal weight and that lack of information on neighboring regions prevents for the present a full understanding of the position of the Kachemak Bay culture. That culture itself is clearly depicted in the most complete and scientific report that has yet appeared on Alaskan archaeology.

HENRY B. COLLINS, JR.

UNITED STATES NATIONAL MUSEUM

*Dakota Texts.* ELLA DELORIA. (Publications of the American Ethnological Society, Vol. 14. xvi, 279 pp. New York: G. E. Stechert, 1932.)

Miss Deloria's excellent collection of tales from the Teton Dakota with text, grammatical analysis, and English translation, together with notes upon customs and idioms of speech, supplies a definitive volume by a trained and competent hand upon this important and much neglected division of the Siouan stock. Not since Riggs's grammar has any attempt been made to analyze texts from the Sioux, and Riggs worked with the Santee division of the eastern reservation. Bushotter's Teton texts, analyzed by J. O. Dorsey, have unfortunately never been published. Collections of tales by Walker, Wissler, Wallis, Mrs McLaughlin, and my own brief study, are unaccompanied by texts. As a linguistic study therefore it is invaluable.

Besides this linguistic advantage, the arrangement of tales according to age sequence gives a clearer view of the Indian story-teller's own appraisal of his stock in trade than I can recall from any other similar collection. The stories drawn from the oldest period are listed first, followed by a second group of tales, perhaps later in action and more novelistic in form but similarly thought of as fiction and making

use of traditional incidents, or sometimes whole sequences of such, in which supernatural beings of the past figure. Here Miss Deloria classes the Coyote trickster stories without any attempt at further differentiation and here are to be recognized familiar Indian type tales of wide distribution. To this fictional division succeeds a second of true tales, again arranged into two groups according to age, the action of which is supposed to have taken place within the tribe not more than two or three generations back. These tales have a purely local interest. They live as stories merely through the thrill of reality emphasized by the story teller's affirmation of the truth of the event. They are not of wide distribution, although four from the older tales (numbers 40, 44, 50, 53) seem to bear direct relation to Mandan-Hidatsa ceremonial origin stories current at Fort Berthold.

It should be observed that this subjective distinction does not necessarily represent a permanent attitude on the part of the relater toward any particular incident. Under other circumstances the traditional tale here classed as fiction might be regarded as of authentic value in tribal history. The Coyote trickster stories are palpably fictions told to raise a laugh, but not always are they discredited as a behavior pattern. In clearing up for us the confusion in the minds of most ethnologists between the Coyote and the Spider figures in Dakota stories, Miss Deloria has done us a special service. The Coyote is the animal form of Iktomi, who is never thought of in spider form, and this suggests, although Miss Deloria leaves the matter to our own speculation, that Coyote has drawn his human name from some foreign source. I think it unfortunate however that no distinction in genre is made between the humorous tales in which the action centers about the trickster figure and those more romantic in tone in which the trickster appears as a subsidiary character. No light is thrown moreover upon the "sacred stories" which are ordinarily included in a story-teller's category. There are no new finds, scarcely a fresh combination, among the fictional tales, and no suggestion of the relation of story-telling of either class to those organized mythical conceptions such as Walker's informant asserted for the Oglala Tetons. In so far the collection is disappointing.

In detail, however, the competence and clarity, both in individual phrasing and in the range and picturesqueness of ideas give the collection unusual value to the student of Indian narrative art. Miss Deloria's mastery over the native phrase leads to such fine precision of speech as that in which an arm is thrust into the ground "as far as the place-for-tapping the vein"; a man in terror "uttered some bear cries to make himself brave." On the whole the style of Siouan stories is European as compared to that of the Northwest Coast or the Pueblo Indians, and this Miss Deloria has brought out admirably in her excellent translations.

MARTHA BECKWITH

VASSAR COLLEGE

*Uto-Aztecan Languages of Mexico.* A. L. KROEBER. (Ibero-Americana: 8. 28 pp., map. \$0.35. Berkeley: University of California Press, 1934.)

In this paper Kroeber gives the distribution of the Uto-Aztecan languages in Mexico, on the basis of the sub-groups Nahuatl or Mexicano, Huichol-Guachichil,

Cora, Cáhita-Opata-Tarahumar, and Pima-Tepehuán, illustrated by a map which also shows Kroeber's classification of the Shoshonean groups in the United States. A sketch of the predominant linguistic differences between the groups is given, with many interesting notes on individual languages, and a table of cognate phonemes compiled from Sapir 1913, 1915. Then follow some newly published vocabularies recorded in 1930 from various Mexican languages, which, though brief, contain valuable data for Uto-Aztecan comparative linguistics, and some notes on kinship terms. Kroeber gives the latest version of his classification of the stock, which differs little from preceding ones. From his linguistic notes emerges the general idea that none of the sub-groups presents any strongly marked-off characteristics with the exception of Pima-Tepehuán, with which the reviewer agrees.

The material on geographical distribution is based on the latest research (Sauer's, eked out for earlier times by a good résumé of past findings), but the linguistics is not. The linguistic treatment is based on the latest *published* work, Sapir's and Mason's, but this is fairly old. Considerable work, as yet unpublished, has since been done in this field by Whorf, drawing on newly available material including Ute-Paiute (Sapir), Hopi (Whorf), Kern River (Voegelin), Luiseño (various), Paviotso (De Angulo and others), Tarahumar (Bennett), Opata (various), Huichol (unpublished vocabulary collected by Lumholtz), Nahuatl (Whorf), Pochutla (Boas). This implies no adverse criticism of Kroeber's valuable paper, the scope of which does not contemplate going beyond sources published or otherwise at hand.

Nevertheless it may be pointed out in the light of this later research that (1) Kroeber's table of cognates (the early Sapir) is due for revision, (2) Huichol "grouping uncertain"—Kroeber—is closely related to Cora; (3) the sub-group lumped as "Nahuatl" deserves separating into its component languages (thus one of these, Pochutla, agrees with general Uto-Aztecan as against Aztec, Pipil, Cora, and Huichol on an important sound-equivalence); (4) in Shoshonean, Hopi is hardly divergent, and Ute-Paiute is undoubtedly one of the most, if not the most, divergent (though in the whole stock it is less so than Pima-Tepehuán); (5) Shoshonean is due for the melting-pot and a recasting—it may even disappear, like "Sonoran," in the process, though I would not yet definitely predict this.

The biggest change in the cognate table is that the lumped  $*(u, o, \gamma)$  splits into two distinct phonemes.  $*o > \text{Azt } o, \text{ Pochutla } u, \text{ Hui } o, \text{ Co } u, \text{ P-T } o, u, \text{ C-T-O } o, u, \text{ Sh } o, \gamma, o, \text{ and } *u \text{ (perhaps } *i) > \text{Azt } i, e, \text{ Poch } o, \text{ Hui } i, \text{ Co } i, \bar{i}, ue, \text{ P-T } u, \text{ C-T-O } u, o, \text{ Sh } u, o$ . Shoshonean cannot properly be lumped in such a table, its various languages differ on many of the equivalences. Mason's astute surmise that  $tl, t$  is a purely Nahuatl splitting of  $*t$  was undoubtedly right, so also was his suggestion that  $*l$  represents two original sounds. He was probably wrong on  $*\eta$ ; it should be retained. The  $*h$  splits into  $*'$  and  $*h$ . For Co  $y$  read  $r, y$ . The question marks in Hui may be replaced by  $ts, kw, l, y$ . The reflexes of  $*p$  are very complicated throughout the whole stock. Kroeber's footnote no. 4 should have been applied also to Co  $k (> t\bar{s}), kw (> t\bar{s}a)$ . Enough for criticisms, though the table will need more revision than this.

For all that, Kroeber's contribution is valuable as a concise presentation of the general Uto-Aztec Mexican lay-out, and as a sifting and condensation of much material from many sources. It points the way to many lines of research, such as the need for good modern studies of Yaqui, Varohío, and Huichol, and will be welcomed by all who are interested in native American linguistics.

B. L. WHORF

WETHERSFIELD, CONNECTICUT

*The Historic Trail of the American Indians.* THOMAS P. CHRISTENSEN. (193 pp. Cedar Rapids, Iowa: Laurance Press Company, 1933.)

The author has set himself the tremendous task of giving "an up-to-date account presenting in broad outlines the history of the Indians in both Americas from the earliest times to the present." He has attempted to give archaeological background, ethnographic facts, history and present conditions for this wide range of peoples. Naturally each group is discussed so briefly that only sweeping generalizations can be made. Such attempts always present opportunities for many errors.

The whole tone of the book seems to be that of one which was done by careful library work, but it does not have the flavor of any working contact with its subject. The sources upon which the author bases his material are a quaint assortment. It is rather pointless to quote today the Reverend Stephen Peet on the Mound Builders when more recent and scientifically accurate works are available. Yet beside this stand such excellent works as that of Kidder on Southwest archaeology and Kroeber on the Indians of California. The author does not seem always to have exercised a proper critical attitude toward his source material.

A compilation such as this may be very useful for the person who wishes a short cut to information on a vast subject. It is unfortunate that the outsider who has no background for criticism is so often imposed upon by giving him material not prepared by a person thoroughly and actively conversant with the field.

ERNA GUNTHER

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*Ancient Americans, the Archaeological Story of Two Continents.* EMILY C. DAVIS. (311 pp., 35 illus. \$3.50. New York: Henry Holt and Co., 1931.)

The author has compiled here from well chosen and authoritative sources the results of many decades of archaeological work in North and South America. The book is not written for the anthropologist who has access to the same sources that have been used, but takes its place among the many intelligent attempts to lay the findings of scientific fields before an interested but untrained audience. And in that field it stands among the best. In an interesting chapter on "Strange Tales of America's Past," the author takes each of the Sunday supplement stories of the origin of the American Indian generally believed, and shows its shortcomings.

This book can be highly recommended for general public libraries and for high schools.

UNIVERSITY OF WASHINGTON

ERNA GUNTHER

## SOUTH AMERICA

*Textile Periods in Ancient Peru* LILA O'NEALE AND A. L. KROEBER. (University of California Publications in American Archaeology and Ethnology, Vol. 28, No. 2, 1930.)

This work is an important contribution toward the study of textile arts of ancient Peru. It determines the periods to which the specimens extracted from the pre-Columbian graves of the coast belong, taking into account the distinct technologic, stylistic, and regional differences.

The specimens of which this work treats have been divided into three groups, corresponding to the Early, Middle and Late periods in accordance with the chronology established by Kroeber. The technics examined are minutely registered and classified, according to their periods and the places of discovery, in a chart which permits one to appreciate at a glance all the material studied. The work is completed with forty-eight plates, giving numerous clear reproductions of the most important textile examples.

The technologic differences discovered permit the authors to establish the following conclusions:

1. That the fundamental technics were already known from the most remote periods attributable to archaeologic material;
2. The textile periods are characterized by the preference given during the period to one or another process of manufacturing, and by the predominance of certain types of ornamentation, the stylistic differences being of more importance than the technologic; and
3. That each region tends to preserve its local characteristics and favorite technics, which gives the regional or provincial stamp to the art.

The conclusions arrived at by the authors are solidly established, being the result of a careful and methodic analysis of varied and excellent specimens, and of the experience acquired in the field bearing relation to the discovery of textiles, associated with other archaeologic elements of recognized styles and periods.

Investigations conducted after those by Kroeber in Peru in 1926, have confirmed his conclusions, have increased knowledge relative to the chronology established by him, and have also made it possible to better segregate the several cultures included in each horizon or period.

It is my belief that certain modifications may be introduced in the first horizon. In Paracas there are two textile periods corresponding to two cultures: that of the "Cavernas," the older, and that of the "Necropolis," which springs immediately from the former. The first should be included in the first horizon, and the second, in the beginning of the second horizon. Painted textiles, needleknitted without assistance of the loom, predominate in the first period. The painted cloths, in their technic and ornamentation, are almost identical with those discovered in Supe, close to the deposits where I have discovered fragments of pottery recognized by me as the "Chavin" type. The textiles of the "Cavernas" are associated with a type of

pottery similar to the "Chavin" type, but very distinct from the "Necropolis" and "Nasca" types of pottery. The big funeral bales of the "Necropolis" type, discovered by me in 1927, were found above the ruins of dwellings and in rubbish heaps containing fragments of pottery of the "Cavernas" type. The principal techniques and decorations of the textile art of the "Cavernas" appear in the "Necropolis" specimens, in which the technic of the embroidery connected with the realistic or semirealistic decorations of the "Nasca" style reach their maximum development, giving a specific character to the culture. In this way, the "Necropolis" textile art approximates in one sense that of the "Cavernas," and in another, that of the "Nasca." In the "Necropolis" no pottery of the "Nasca" style has been found, not even in fragments, but that of the "Cavernas" has been found. In Cayangos, Ica, rubbish containing fragments of pottery of the "Cavernas" style is found underneath the graves of the "Nasca" type. In the "Nasca" region no pottery of the two periods of the "Paracas" type has been found. Therefore the culture of the "Cavernas" must be considered in the first horizon and that of the "Necropolis" in the beginning of the second.

In Nasca there are graves of the "Pre-Nasca" type, differing from those of the "Andine" and "Epigonal" types. The "Early Nasca" of Kroeber must be considered in the first period of the second horizon, as it succeeds the "Necropolis" culture. The "Andine" culture of the Nasca or Epigonal must be considered in its double character: "Pre-Nasca," which precedes or is contemporaneous with "Early Nasca" of Kroeber, and "Andine," properly "Epigonal," which occurs above "Nasca," and exists up to the period of the Incas. This Coastal Andine culture, in its double chronologic character, must be considered as successive or periodical offshoots of the archetype recently examined in the basin of the Mantaro River

JULIO C. TELLO

LIMA, PERU

*Les textiles anciens du Pérou et leurs techniques.* RAOUL D'HARCOURT. (170 pp., 81 figs., 109 pls. Subscribers' ed., 180 fr. Paris: Les Éditions d'Art et d'Histoire. 1934.)

M d'Harcourt's latest volume is a detailed study of a selected group of fabrics and certain textile techniques either outstanding for their frequent occurrence, or for their interest as novel variations of familiar processes. No emphasis is placed upon any particular site or time period in making the selections. This type of study was initiated by M. D. C. Crawford, and aids in an approach to the whole subject of Peruvian textiles from any one of its several aspects. Thanks to the extraordinary ingenuity of the ancient craftsmen who furnished material for an indefinite number of studies, there is little need to duplicate efforts. As a consequence, M d'Harcourt's lists of techniques, although they sound familiar, present many new details.

The brief and clearly written text (123 pages) is topically organized, the line drawings are models of simplicity, and the heliotype plates are unusually fine. In

some cases the techniques are almost as well shown in them as in the drawings. Texts, figures, and plates are so unified that one is able to follow the variations in the plain weaves, which include the tapestries and interlocking types, the twills, brocades, double cloths, and gauzes. The analyses of the less well known textile fabrics, the imitation filets, knotted types, embroideries, resist-dyed pieces, tassels, and fringes are welcome additions to the available published material. One point raised by the author is of more than usual significance. He suggests that knowledge of felt making might have been found among the ancient Peruvians (p. 119, pl. LXIV), but reserves judgment in the matter.

A short description of each fabric covering the main technical points—yarn content, method of construction, designs, and colors—provides data upon which the text is based. The longest analyses together with seventeen plates are devoted to the famous Paracas embroidery in the Musée d'Ethnographie du Trocadéro. This reviewer acknowledges an error in the interpretation of a photographic reproduction of a portion of the embroidery (AMERICAN ANTHROPOLOGIST, Vol. 36, p. 407). Now, with the description of a device in M d'Harcourt's volume (p. 65, fig. 37), the "Paracas textile," as the piece is called, gains added uniqueness through a method of patterning not to be found among more than two hundred Paracas fabrics analyzed at the Museo Nacional in Lima. No more convincing proof of the imaginative skill of the ancients is required than is offered by the continual discovery of singular devices to vary effects.

A bibliography of eighty-three titles and an outline-index complete the volume. Too much cannot be said for the beautiful format and the superb photography. M d'Harcourt has contributed substantially to an increased understanding of the art and weaving craft of early Peru.

LILA M. O'NEALE

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## AFRICA

*Pagan Tribes of the Nilotic Sudan*. C. G. SELIGMAN AND BRENDA Z. SELIGMAN. Lxxiii, 565 pp., 38 figs., 60 pls., maps 42 s. London: George Routledge and Sons, Ltd., 1932.

This comprehensive volume presents the results of Professor and Mrs Seligman's anthropological and ethnographic researches in the Nilotic Sudan, which were begun in 1909 and continued at various intervals partly by Professor Seligman himself, partly by Professor Evans Pritchard, his pupil.

The authors have limited their investigations to a study of the social and religious aspects of culture. The facts are arranged in the form of tribal monographs and placed against a sketchy general background, comprising in each case some notes on the physique (indices of measurements and general descriptions), natural environment, and economic life of the respective tribes.

The authors distinguish among the pagan tribes of the Nilotic Sudan two main groups, a dolichocephalic and a mesocephalic, the former comprising the Nilotes



(Shilluk, Dinka, Nuer, Anuak) and the Nilo-Hamites (Bari, Lotuko, Lango, etc.), and the latter the Nuba-Fung peoples on the one hand and on the other the numerous small tribes of the Bhar El Ghazal region and the populous Azande. The bulk of the information relates to the Nilotes and Nilo-Hamites; while of the mesaticephals only the Nuba and the Azande receive more than a sketchy treatment. The material for the description of the latter was furnished by the excellent researches of Professor Evans Pritchard.

Historically the authors assume the origin (i.e., Hamitization) of the Nilotes and Nilo-Hamites in the neighborhood of the Great Lakes of East Africa, while they consider the mass of the mesaticephals as a northeastern projection of the round-headed peoples of the Congo, with cultural influences even from West Africa. Psychologically they correlate with the Nilotes a proud conservatism, intense religiousness and introversion which—perhaps with the exception of the Azande—sets them apart from the rest of the Sudanese peoples. Economically the dolichocephals are essentially pastoral and the mesaticephals agricultural; a fact which, however, is not paralleled by a similar clear-cut distinction in regard to cultural institutions. These, as described by the authors, present a rather complex picture which, for lack of space, we can review only in its most outstanding features.

The social and political organization in the larger tribes is characterized by the institution of a paramount chief, which among the Shilluk, and to a lesser degree among the Dinka and Fung, takes the form of a "Divine King" of Frazerian type. Among some peoples—especially Nuer the Bari—the functions of the chief are divided between land, cattle, and village chiefs, etc. (departmental experts); while among the Dinka, Lotuko, and Nuba the chieftainship is in the hands of the rain maker. A social stratification is found among the Shilluk and Azande, who recognize a privileged class of nobility; while among the Bari there exists an hereditary servile class of distinct physical type and probably of foreign extraction. Chieftainship is of little moment among the Bhar El Ghazal tribes.

Age class organization and initiation ceremonies are almost universal and have developed a military character among the Dinka and the Azande (with institutionalized homosexual practices among the latter), while circumcision is practiced only among the Fur, Fung, Azande, Bhar El Ghazal tribes and Dinka, and constitutes (except among the Fur) a recently adopted custom. Forehead scarring and knocking out of the lower incisors are also widespread, but not always connected with the age group organization.

In the magico-religious sphere the most impressive feature is the rain making ceremonies, with the hereditary office of a rain maker and rain stones, rain spears, animal sacrifices and other methods of invoking the ancestral spirits as the principal but not universal ritual elements. The core of religious practices is ancestor worship, which among the politically highly organized tribes (Shilluk, Dinka) centers around the worship of the royal ancestors in sacred shrines of graded importance. The conception of a high God exists to a certain degree among the Shilluk, Nuer, Lotuko and Nuba; while among the Dinka, Bari, and Azande there is a belief in a supreme spiritual agency but without the personal features of a God or Creator.

The chief magico-religious practices, however, are everywhere directed towards spirits of lesser range and more departmental importance.

The kinship structure—which receives the fullest and most systematic treatment—is, with the exception of the Nuba, characterized by patrilineal, mostly exogamous clans, with partly totemic origin among the Shilluk, Dinka and Nuer. The prevalent form of clan totemism is that of the "twin pattern," one twin being the animal, the other the human ancestor of the clan. In spite of patrilineal succession and inheritance, there are numerous ties to the mother's family and clan, as in the almost universal marriage restrictions, the close bond with the mother's brother (especially among the Shilluk and Azande), and in many features of the terminology. Of avoidances, the mother-in-law taboo is most prominent and sometimes extended in a classificatory manner, but is often lifted after the birth of the first child. Dinka, Lotuko, Nuba. Other avoidances toward relatives-in-law, cousins, etc., occur but are too varied in scope and character to allow of generalization.

Marriage is everywhere, except among the Nuba, characterized by the institution of the bride price. Its often numerous and widely spaced instalments and the complex system of distribution are one of the main forces of social stability, as has been well demonstrated in the chapter on the Azande. Concerning, finally, the kinship terminology, the authors try to classify the various systems according to the rather unfortunate distinction of "descriptive" and "classificatory," although most systems show features of both. The systems of the Bari, Nuba, and Fung come closest to the classificatory type, while the main "descriptive" feature among the other systems is the distinction of a brother as father's son and mother's son (Shilluk, Dinka, Nuer, Azande). The Bari and Lotuko are especially rich in terms for relatives-in-law, while among the Azande elder and younger brothers are distinguished by separate terms. The kinship structure of the southern Nuba stands quite apart from the rest, being a typical example of a matrilineal society with no clans, marriage within the community, no bride price, initial matrilineal residence, instability of the marriage tie, and a manifold and intimate relationship between mother's brother and sister's son which leaves the father in the position of an outsider in regard to his wife and children.

In view of the fact that these investigations were intended to be ethnographic surveys rather than exhaustive studies, it would not be fair to criticize this work for certain lacunae. Nevertheless the question may be raised whether it would not have been wiser to have limited the number of tribes investigated in favor of a more thorough study of a few cultures. The material presented in the book proves again that a real understanding of social, political, and religious institutions can only be attained if studied in close correlation with other aspects of cultural life.

GUNTHER WAGNER

KISumu, KISumu Colony.

*The Pre-Historic and Pre-Colonial History and Background, South Africa.* M. WILSON. (Lan. 77 pp., 16 figs., 70 pls., map, 25 s.) Cambridge: Deighton Bell and Co.; Kimberley: Alexander McGregor Memorial Museum, 1933.

After many years of labor in studying the rock engravings in the region of

Kimberley, where Miss M. Wilman is Curator of the Alexander McGregor Memorial Museum, a volume which is both a work of art and a scientific thesis has been produced.

An historical introduction gives a summary of the observation of rock paintings and engravings since the time of Robert Moffat (1842) to the present day; then follows a discussion of the geographical distribution of these works of art. Apparently the localization of engravings depends to some extent on a geological factor since the diabases, dolerites, and some sandstones do not favor the formation of rock shelters.

The classification of the works of art gives four categories: (1) the oldest or "classical" styles depicting animals with which the artists were familiar, also human beings, plants, and geometrical patterns; (2) the engravings which are derivatives from class 1. In division 3 the spoors of human beings and animals are represented, while class 4 consists of scribblings of recent date.

With regard to the chronological sequence of engravings, the majority of the "classical" styles are divisible into groups based on differences in technique, but misleading factors are found in the chemical composition of different rocks. On some surfaces rust and black oxide form readily, and lichens grow quickly, so giving an unwarranted impression of old age. Differences of opinion exist respecting the tools used by the engravers, but a feasible hypothesis states that the boart (a form of diamond used for industrial purposes) may have been used.

Who were the engravers? and do the stylistic differences justify an assumption of successive migrations? On the whole the engravers showed more stylistic diversity than did the painters; yet some resemblance between the products of the engravers and the painters is evident. There are areas where paintings predominate, and also localities where engravings are more numerous than paintings. Miss Wilman inclines to the view that the early Bushmen to whom the works of art are ascribed were the phylogenetic forerunners of Bushmen who have produced recent engravings. The osteological evidence for support or refutation of theories is too meagre to be convincing. The suggestion that artists were actuated by impulses of magic and religion has possibly been overstressed.

Miss Wilman confines her hypotheses to a South African problem of racial identity, lines of migration, and the association of types of stone artifacts with styles of art. But a wider problem affecting the racial history of Africa is involved. Rock paintings and engravings of the Sahara, Libya, Kordofan, and Tanganyika Territory lend plausibility to the hypothesis of pre-Bushman migrations on a large scale from the north of Africa, down the east side, across the Zambezi, and over the southern part of the continent.

The book is well indexed, an excellent map showing distribution of engravers' sites about Kimberley is provided, and the bibliography gives more than a hundred and twenty names of contributors to the problem of Bushman art. To an expert the book is a valuable work of reference, and to a novice in the archaeological problems of South Africa the lucid text and the artistry of the pictures are stimulating to further study.

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## ASIA

*The Hisago-Bune or Calabash Boat.* SHINJI NISHIMURA. (v, 86 pp., 21 figs., 7 pls. 3 yen. Tokyo, Society of Naval Architects, 1934.)

This book, a revised and enlarged edition of a brochure issued in 1918, is a part of a comprehensive series undertaken by Professor Nishimura, "A Study of Ancient Ships of Japan," of which eight volumes, in whole or in part, have already appeared. In a sense the book is misnamed, for it is not concerned with calabash boats, but rather with the employment of calabashes as floats tied to the waist of swimmers or attached to rafts or other vessels. Professor Nishimura infers the early use of the calabash float in Japan, basing his opinion upon his interpretation of rather obscure passages in some ancient legends of the Japanese and Koreans, on his construction of certain old Japanese words which is a radical departure from the views of other Japanese scholars, and on the somewhat different, but contemporary use of gourds by diving Korean fisherwomen of Chyoi-jyu Island. Since Professor Nishimura asserts that "the present is the age of the diffusion theory" (p. 63) he does not hesitate to cite African or other distant sources concerning the use of gourds as direct evidence to strengthen the case for an ancient similar use in Japan. He also goes so far as to claim that since a calabash water container can be made watertight by the use of a stopper, it is also airtight and, therefore, wherever gourds or gourd-like ceramic vessels may be found "it can be theoretically deduced that the calabashes had been used as floats" (p. 72). Professor Nishimura finds no difficulty in assuming that the world-wide appearances of the use of gourds represent the results of diffusions from a common center of what he terms the "calabash culture." Apparently he believes it unnecessary to consider evidence of a contrary nature or to seek pertinent facts to substantiate such a conclusion.

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*Bulletin of the Museum of Far Eastern Antiquities (Östasiatiska Samlingarna), No. 4.* (ix, 321 pp., 61 pls. 87 50. Stockholm, 1932.)

Half the articles in this bulletin are based on archaeological collections presented to the Crown Prince of Sweden in the Far East during his world tour of 1926. Ivar Schnell describes ceramics, stone tools, and bone implements from Japan, and in the same article ("Prehistoric Finds from the Island World of the Far East") discusses the finds of the Sten Bergman expedition to the Kurile Islands in 1929-30 and the archaeological results of the Swedish expedition to Kamchatka in 1920-22. The first part of Schnell's article consists of a summary of the position so far attained by archaeology in Japan, which will be most useful to those who desire a superficial knowledge of Japanese prehistory. In such a summary it is of course difficult to please everyone. Some anthropologists will not accept without murmuring the author's conclusion, apparently based largely on Sternberg's work, that "Relatively certain proofs have been obtained establishing the Ainu's connection with the peoples of the East Indian archipelago." Students unfamiliar with the terminology

of Japanese archaeology will welcome the chart showing the various kinds of prehistoric pottery with their Japanese names and type samples

Among the finds of the Bergman expedition in the Kuriles the author considers the pottery most interesting. He has distinguished three types, smooth, cord-ornamented, and textile-ornamented, whose history he traces to influences from Hondo and Hokkaido.

The archaeological work of the Swedish expedition to Kamchatka supplements Jochelson's investigations in 1910-11. The general conclusion is that Stone Age culture in Kamchatka was affected both by the Koryak area to the north and by the Kuriles to the south.

The most noteworthy feature of the Formosan Neolithic collection described by Margit Bylin is a smooth-tanged adze similar to a type found in southeastern Asia. The author discusses the influence of Indo-China on early Formosan culture.

A brief note by Professor Pelliot deals with some animal style bronzes.

Dr Olov Janse's paper ("Tubes et Boutons Cruciformes Trouvés en Eurasie") suggests an answer to the questions raised in his earlier article, "Quelques Antiquités Chinoises d'un Caractère Hallstattien," in the second bulletin of the series. He describes a collection of objects from China and Mongolia, and then notes similar finds from Central Europe, the Caucasus, and Siberia. He discusses their probable development from a tube holding together parts of harness to an ornamental button with possible magical connotations, and suggests that they were made in the metallurgical centers of the Hallstatt era, diffusing along the great route from Europe to China. It appears that their use survived in Asia much longer than in Europe since no finds of the kind are known from the latter region for the period 500 BC to 500 AD, when they were apparently reintroduced by invading groups from Asia.

The article contributed by Dr Karlgren is an attempt to establish the phonology of archaic Chinese by a study of the phonetic value of the rimes in the *Shu king* and will be of interest chiefly to sinologists.

To the anthropologist the most stimulating contribution will probably be Dr J. G. Andersson's "Hunting Magic in the Animal Style." This article continues the discussion in the author's earlier paper "Der Weg über die Steppen," which appeared in the first volume of this bulletin. Dr Andersson here concentrates on a study of the animal style of art among the nomad groups of Central Asia. Examples of this style are found from the Black Sea to the Yellow River, with apparent centers in the ancient Scythian area on the north shore of the Black Sea, in the Minusinsk area of southern Siberia, and around Ananino west of the Urals. In recent years it has become evident that a fourth center exists in the Ordos region of the Chinese-Mongolian borderland. In studying the animal style bronzes from this area Dr Andersson noted a strongly naturalistic art and, more particularly, vivid mating scenes which are not paralleled by the more sophisticated types of the Scythian and Siberian centers. From this he concludes that the animal style . . . developed to a very large extent under the stimulus of an active hunting magic along

much the same lines as that which . . . gave rise to the other grand Animal Style . . . of the late Palaeolithic time.

After describing the Ordos bronzes, Dr Andersson cites historical sources illustrating the importance of hunting in the life of the steppe nomads. He then develops the striking parallel between much of Magdalenian art and the Central Asian animal style, especially in scenes dealing with propagation of the game stock. The theme of increase magic can be traced not only in the obvious mating scenes but also in the frequent multiplications of figures and in the scenes of pre-mating combat.

Dr Andersson submits his theory with great modesty and disarms the critic by himself pointing out many of the objections which can be raised against it. Of these the most obvious are that only part of the evidence, such as the mating scenes, contributes directly to the conclusion and that much of the rest becomes relevant only when the premise is granted. The reviewer also feels that the author tends to exaggerate the importance of hunting in a primarily pastoral culture. Why, incidentally, if the theory is to hold, is there no evidence of an animal style magic art looking to an increase of domestic animals? Concerning the importance of hunting there appears to be a contradiction between the author's conclusions and his material. He sums up (page 278).

The notes which we have collected . . . give us the picture of cattle breeding steppe dwellers who were at the same time great hunters for the purpose of increasing their stock, collecting furs for clothing and, above all, *as the main source of their food supply* [italics mine].

But on page 273 in quoting Minns we find:

Hunting supplied *some* of their food, *more* was produced by their cattle especially by their horses [italics mine].

But these criticisms cannot impugn the value of Dr Andersson's fresh and vigorous treatment of a subject which might easily become stereotyped into a mere description of objects.

The contributions to the present bulletin follow the patterns of those in previous volumes of the series (Bulletin No. 1, 1929; No. 2, 1930; No. 3, 1931). These fall into two general types, archaeological and linguistic, with the former heavily predominant. Relationships between European and Asiatic forms are stressed, and much space is given to discussions of symbolism, particularly fecundity symbols. With the exception of Schnell's paper on the Japanese islands, all the articles are based on Chinese material. Most of the authors are associated with the Museum of Far Eastern Antiquities.

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#### GENERAL

*Verhandlungen des XXIV. Internationalen Amerikanisten-Kongresses, 1930.* R.

GROSSMAN AND G. ANTZE, eds. (lxvi, 322 pp., illus. Hamburg: Friederichsen, de Gruyter and Co., 1934.)

Of the thirty-nine articles in this volume of proceedings—articles of varying merit on a wide range of anthropological subjects—a few strike the reviewer as

worthy of special mention. Sapper (*Der Kulturzustand der Indianer*) attempts a general survey of aboriginal American culture, especially as influenced by diverse geographic conditions, and of the changes wrought therein through European contact. Lowie (*The Omaha and Crow Kinship Terminologies*) attributes the Omaha and Crow types of kinship systems, i.e., those in which cross-cousins on one side are classed in the first ascending generation and those on the other in the first descending generation, respectively to patrilineal and matrilineal descent where either of these occurs in conjunction with a matrimonial arrangement which overrides generation distinctions. Wegner contributes two valuable descriptive articles (*Die Quruñg'ua und Siriono* and *Die Mojos-Indianer*) on certain tribes of eastern Bolivia. Kirchoff (*Versuch einer Gliederung der Sudgruppe des Athapaskischen*) classifies the southern Athapaskan languages into two main divisions: (1) Navajo-White Mountain-San Carlos-Tonto and (2) Chiricahua-Mescalero-Lipan, with Jicarilla and the aberrant Kiowa-Apache forming a third group revealing certain affiliations to both the others.

GEORGE PETER MURDOCK

YALE UNIVERSITY

*Emile Durkheim on the Division of Labor in Society: Being a translation of his "De la division du travail social" with an estimate of his work.* GEORGE SIMPSON. (440 pp. \$3.56. New York: Macmillan, 1933.)

The translation of Emile Durkheim's great work, "De la division du travail social," by George Simpson, is poorly done. The introductory statement which purports to be an estimate of his work by the translator clearly demonstrates that he does not understand Durkheim's general theoretical position. He is also more concerned with the philosophical implications of Durkheim's theory than with his scientific contributions. It is unfortunate that all the works of this great French social theorist are not available in English, and that he seems fated to continue being misunderstood by English and American anthropologists.

W. LLOYD WARNER

HARVARD UNIVERSITY

*Our Primitive Contemporaries.* GEORGE PETER MURDOCK. (xxii, 614 pp., 117 figs. \$5.00. [College ed., \$3.60.] New York: Macmillan, 1934.)

*Our Primitive Contemporaries* consists of succinct, orderly, and very live descriptive accounts, about thirty-three pages each on the average, of "eighteen different primitive peoples representative of all the great regions and races of the world and of all the major types and levels of culture. Each account, though short, aims to cover with reasonable adequacy every important aspect of economic, political, and social life, with some reference also to the racial, geographic, and historical background" (p. vii). Each tribal description is followed by an excellent selected bibliography, with the key source or sources marked with an asterisk. The approach throughout is exclusively factual. Theoretic interpretations and reconstructions are

avoided. The accounts are based on a critical use of the best sources. The account of the Haida, however, represents a summary of the author's own field work. Incidentally, he expresses a graceful recognition and tribute to the soundness of the earlier pioneer work of Dr John R. Swanton and others among the Haida.

The present volume is marked by rigid reserve and objectiveness. Each of the eighteen descriptions gives a reasonably complete bird's-eye-view of the total culture of the tribe or people dealt with. The selection of tribes seems to the reviewer to have been particularly happy. These representative samplings should give the general reader and the college student, for whom the work was primarily written, an excellent concept of the life and culture of preliterate humanity. College teachers of anthropology and of sociology will be particularly grateful to Dr Murdock for this valuable adjunct to their work. But most professional anthropologists, too, will find in these synoptic summaries much to jog their memories and to expand their knowledge of the tribes and culture levels treated. A very full index provides a ready way of following particular traits through the wealth of factual data that have been incorporated into the eighteen sketches.

The reviewer has followed the good or bad tradition of going more critically through the accounts of those peoples with whose culture he happens to be more familiar personally, for the purpose of discovering if possible any errors of fact. To date, however, he has had no luck. Not only do the accounts appear as very accurate, but, in spite of the condensation which the aim and plan of the work called for, they are eminently readable. And the numerous excellent illustrations add very much to the attractiveness of the volume.

JOHN M. COOPER

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*The Oriental Institute.* JAMES HENRY BREASTED. (455 pp., 207 figs., 2 color pls., map. Chicago: University of Chicago Press, 1933.)

This volume is part of a projected series of volumes which has as its purpose the survey of the activities and problems of the University of Chicago. As Volume XII of the series the book aims at setting forth the various research enterprises of the Oriental Institute, and sketches scope, character, and purpose of that institution. The nature of the book is essentially historical in so far as it deals with the rise and development of the Oriental Institute and its expansion from a Department of Oriental languages into a scientific institution which interests itself in the Near Eastern area in the widest scope in order to "recover the lost story of the rise of man." By organized endeavor the Oriental Institute has widened its activities to such an extent that in the main it has become anthropological in character. What the Americanist endeavors to do, especially in our southern American hemisphere, the Oriental Institute plans to accomplish in the Near East, with a probable extension into wider Asiatic regions.

A truly gigantic undertaking for a single institution! With what enterprising spirit the work has been carried on so far is demonstrated on practically every page



of the book. What science may expect from the wide-flung labors of the Oriental Institute can already be gathered from the published accounts of the field expeditions in Egypt and Western Asia, and the scientific labors carried on at the home institute. Archaeologist, architect, geologist, anthropologist, historian, and philologist work together for the ultimate realization of the purposes of the Oriental Institute—each one a *mustashriq* of his own calling. Dr Breasted has succeeded admirably in telling the story by weaving into the account of the present labors the historical background, thereby enabling the reader immediately and easily to acquaint himself with the problems that the Orientalist of to-day encounters. It is a work which no anthropologist should miss reading.

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## DISCUSSION AND CORRESPONDENCE

### PRIMITIVE CONCEPTS OF DISEASE

The aim of Clements' study so named<sup>1</sup> is "to offer a scheme of classification for the disease concepts of primitive peoples: to obtain a fairly complete geographical distribution of the classified concepts, and to frame certain conclusions as to their relative antiquity, probable origin and historical connections." With respect to the sources to which diseases are ultimately attributable Clements mentions three broad and recurrent interpretations in human thought, natural causes, human agencies and supernatural agencies. These he finds too broad for "practical" purposes, evidently meaning for use in tracing historical connections. He does not relate these generic categories systematically to the concepts which he adopts. These latter are sorcery, breach of taboo, disease-object intrusion, spirit intrusion, and soul loss. A comprehensive sampling of ethnographic literature, as well as secondary sources was undertaken to establish the occurrence of the concepts. Citations to the literature are embodied in Table 1 in which opposite the name of each tribe, people, or area, are to be found in separate columns page references under the five disease concepts. Four world maps exhibit graphically the distribution of each of the concepts except sorcery.

After a preliminary analysis of each concept, the greater part of the text is given over to weighing the evidence furnished by geographical distribution in favor of or against historical connections between areas having similar disease concepts. For the most part the concepts isolated are manipulated as independent unit traits throughout the discussion. Associated methods of treatment, such as confession with breach of taboo, sucking as a means of removing disease believed to be caused by the intrusion of material objects, exorcism with spirit intrusion, are sometimes interpreted as of independent origin and functionally derived from the concept (confession, pp. 208-209); or as of unitary origin, adhering to the concept throughout its spread (sucking with disease-object intrusion, p. 213, "once linked, they diffused together simultaneously;" exorcism with spirit intrusion, p. 221). Since, according to the views of the author, sucking as a therapeutic method is an independently developed trait, not intrinsically connected with the concept of disease-object intrusion, the constant association of these two traits can be considered evidence for historical connection between peoples. The same interpretation is applied to the relation between exorcism and spirit intrusion, because "there is no reason why the practices of exorcism should arise spontaneously from the concept of spirit intrusion" (p. 221).

After having established the connections between the disease concepts of peoples in various regions the author further essays a chronological sequence of the concepts. The oldest in the series is disease-object intrusion, which is of unitary origin

<sup>1</sup> Forrest E. Clements, *Primitive Concepts of Disease* (University of California Publications in American Archaeology and Ethnology, Vol. 32, No. 2, 1932).

in a Paleolithic horizon. This is followed by soul loss, again of unitary origin, possibly in Siberia. Then comes spirit intrusion, all occurrences of which are also "historically" connected. Finally in the case of breach of taboo, Clements makes Southern Asia and Oceania prior to the Arctic, which is followed by Mexico, but he considers the concept to have originated independently in each of these regions.

The reviewer must confess that he is not in sympathy with the general methodological approach adopted by Clements, nor is he convinced of the validity of the "historical" conclusions drawn by the author. One of the intrinsic difficulties which is basic to the whole investigation is the isolation of the disease concepts themselves. This difficulty emerges clearly in respect to sickness ascribed to sorcery, which is Clements' first concept. Sorcery involves (a) a human agent, (b) a technique (means) employed by the sorcerer, and (c) often a specific proximate cause of the malady which is believed to produce the symptoms observed in the victim. It is not always easy to distinguish b and c because b may be emphasized in native thought to the exclusion of c or vice versa. When we are speaking generically, magic is the term most frequently employed to refer to b. But in strict analytical terms the distinctions noted must be borne in mind. In any one culture, where disease is involved with sorcery, all these factors must be considered with reference to each other and the wider circle of native belief, in order to explain the theory of disease entertained. If, for instance, sorcery involves disease-object projection, accomplished by imitative magic, and resulting (according to native belief) in the actual presence of a material object in the body of the victim and removable by some special kind of therapy, we have all the basic elements necessary to an understanding of diseases and their treatment due to sorcery of this type. If we wish then to classify the disease concepts of this culture we may use the generic concept of sorcery and let it go at that; or, since sorcerers may cause sickness in their victims by other means than disease-object projection, we may wish to list the ways in which victims are so affected, and by doing so we adopt a different level of classification. But it is inadmissible to employ both levels at once unless they are related in some explicit and comprehensive scheme. Clements seems to recognize this (p. 187) in his introductory remarks on sorcery, where it is discussed in generic terms. But then he proceeds to consider "soul loss" and "disease-object" intrusion as parallel, not subordinate concepts. His analytical point of departure is shifted from a generic type of human agency in disease causation to the proximate cause of the disease as it is believed to affect the victim. In cultures, therefore, where "soul loss" and "disease-object intrusion" are the result of sorcery he records three "concepts," where in terms of more precise analysis there is only one from the standpoint of the ultimate source of the malady and two with respect to the proximate cause. Actually, as even a casual inspection of page references in Table 1 suggests, sorcery means soul loss or disease-object intrusion or both in many cases, and is not an independent concept. Consequently it is not surprising to find that in Clements' historical discussion of sorcery, he is compelled explicitly to recognize its generic character and the need for a more precise analysis of its form as preliminary to more exact treatment.

The same type of difficulty is involved in the attempt to handle breach of taboo as a unit trait. It is an enormously broad generic concept based on the belief that moral transgression on the part of individuals will provoke disease. As an important type of social sanction connected with customs of great variety, it is easy to comprehend the functional importance of such a disease concept in human cultures of diverse patterns. No wonder that Clements is compelled to posit multiple origins in this case. Historical connections, however, within narrower limits than those set up in this study might be sought if attention was directed to the class of custom which was under such a disease concept, or the specific source of a taboo itself. Although not discovered by Clements in the literature on the Eastern Woodland tribes except in the case of the Iroquois, I have found this concept of disease among the Saulteaux I have been investigating recently. In this case one type of taboo under a disease sanction is the command of a guardian spirit. In this particular instance the occurrence of the same connection elsewhere in North America might be historically significant. But interestingly enough in the case of the Saulteaux, while confession as a means of curing illness caused by breach of taboo is known, it does not apply to the case mentioned above. Variations such as this within the culture of one people seem to me to challenge Clements' assumption that confession as a method of treatment is functionally derived from the breach of taboo concept.

Even the more specific concepts used by Clements are by no means always clean cut, and this difficulty is further complicated by the necessity, in such a world wide study, of using sources of varying levels of intrinsic value, and the temptation to interpret ambiguous statements positively. For example, Clements himself points out (p. 188) that in certain areas the disease-object is reported to contain

a spiritual essence which is the real cause of the sickness . . . accordingly the real cause [of the disease] would be a spirit in the body, and such diseases should be classed along with those caused by demons who enter the body without the aid of a tangible vehicle.

They might, in short, be relegated to the category of spirit intrusion. Clements, however, chooses to ignore this complication and the possibility that such cases may not belong to either of the concepts he has adopted. It is obvious that while this procedure facilitates classification and "historical" treatment of the type essayed, it does not aid us in grappling with realities.

In respect to the use of sources this paper is curious, since in a world-wide sampling of the data China is represented only by citations to Hastings' "Encyclopedia" and Tylor's "Primitive Culture," and Tibet and Mongolia ("Buddhist [!] tribes") likewise by a sole reference to the last named source. Hence there is a huge area in the heart of Asia, the theater of important movements of peoples and elaborate culture developments, which remains almost entirely unconsidered. Is it to be assumed that the so-called primitive peoples of Asia existed from time immemorial entirely removed from the influences emanating from the centers of culture developing there? I raise this query simply because European countries, ancient and modern Egypt, as well as ancient Babylonia and Assyria, are included, although for the civilizations of the eastern Mediterranean Hastings is the chief source utilized.

Although the author cannot be blamed altogether for the inclusion of evidence from loose or ambiguous statements of other observers, it seems to me that he has not been over cautious in the matter. I was struck at once, for example, by the attribution to several Algonkian peoples, among two of which I have done field work (Cree and Saulteaux), of a belief in spirit intrusion as a disease concept. To Clements the Cree and Saulteaux are evidently one since he cites the same page references in Skinner's "Eastern Cree and Northern Saulteaux" opposite both groups, despite the fact that Skinner's book is divided into two parts and what he says about one group does not apply to the other. As a matter of fact the references are to the Cree. One of them (p. 78, not p. 76) is to a statement that "an idiot is popularly supposed to be possessed of an evil spirit;" my only comment here is that I never uncovered such a belief myself among either the Cree or Saulteaux, which is worth no more intrinsically than Skinner's positive assertion, except for the fact that Clements (p. 219) stresses the point that Skinner's statement is the only evidence of "spirit intrusion as a cause of insanity [?]" in America. "It crops up again in Borneo and Africa," he adds. Surely this should have made him wary. The only other statement on which spirit intrusion for the Cree could be based is that on p. 63 (although p. 76 is likewise given), where, with reference to a miteo crossing rivers without a canoe, Skinner says, "for the spirits transported him bodily." Guardian spirits (*pawaganak*), by whose aid a man can accomplish miraculous things, are undoubtedly meant; but they never enter a man's body nor can I discover in the context any reference to disease. In respect to the references in Hoffman (pp. 159, 197), the only authority on the Ojibwa used, we do have positive assertions in regard to spirit intrusion, but the fact that the term exorcism appears in the same sentence, with the statement that "evil spirits are sometimes removed by sucking them through tubes" suggests at once the characteristic therapy for objects projected into the body by sorcerers, and Clements, it may be noted, gives the same page references for disease-object intrusion. This is undoubtedly the concept which is being described somewhat loosely by Hoffman. In the case of the Naskapi Turner is the authority given, and Clements rightly queries the evidence here which rests on the statement that remedies for disease are in the hands of the shaman "who claims to have control of the spirit which causes all disease and death." As I see it, there is no reason whatsoever to interpret this as a belief in spirit intrusion as a cause of disease, and Prof. Speck tells me that he is not acquainted with the concept among the Naskapi. The case for spirit intrusion among Algonkian peoples is then not supported by the evidence cited by Clements.

I have gone into some detail here only as a specific example of one of the pitfalls into which one is led by such an exclusive dependence upon so many and diverse sources. With such a large margin of error to cope with in respect to a realistic isolation of concepts, and in addition the difficulties involved in the sampling and interpretation of sources, it seems to me that a genuine historical interpretation of the data can hardly be expected to proceed to valid conclusions.

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## ANENT THE KUTCHIN TRIBES

In a recent article in the *AMERICAN ANTHROPOLOGIST* (Vol. 36, pp. 163-79) Dr Cornelius Osgood has performed a real service for students of the American Indian by compiling a synonymy of the terms by which the various Kutchin groups have been designated. Dr Osgood has also made a most helpful revision of Cadzow's map by including the Birch Creek Kutchin and eliminating the so-called Atai and Hun Kutchin. As Dr Osgood points out, the term Kutchin has been used most loosely ever since Dall's time and it is well that the name should be restricted to what Osgood calls the "true Kutchin." Certain it is that the adjacent groups of the Upper Yukon and of the Tanana are not Kutchin, nor does the term have any meaning for them.

In designating the Kutchin groups Dr Osgood prefers the term tribe to that of band. No doubt he was struck as I was by the feeling of group or tribal unity which seems to characterize the Kutchin peoples. Such a feeling stands in marked contrast to the situation among the Athabascan groups on the Tanana and Copper Rivers with whom I have worked. Among the latter groups the term band would be more appropriate.

Thanks to Grants-in-Aid from the Social Science Research Council and from the National Research Council I was able to spend the summer of 1933 in field work among the natives of the Chandalar River in Alaska. Information secured from various Chandalar informants confirms all of Dr Osgood's eight Kutchin tribes but adds a ninth tribe, the Dihai Kutchin. It is because the Dihai Kutchin have escaped previous mention that I venture to write this note.

The Dihai Kutchin formerly inhabited the territory about the north fork of the Chandalar and the middle and south forks of the Koyukuk River. Virtually no Indians inhabit this region now and within comparatively recent times a number of Eskimo families have settled in the area. The term *dihai* signifies yonder or farthest distant, or, to be more precise, farthest distant in the downstream direction. As one informant expressed it, "If there were a string of cabins along the river bank, the last cabin on the downstream side would be termed the 'dihai' cabin." In other words the Dihai Kutchin were the most westerly of the true Kutchin tribes.

Like their neighbors, the Chandalar Kutchin, who inhabited the territory about the east fork of the Chandalar River, the Dihai Kutchin were a highly nomadic group of mountain dwellers living almost exclusively by hunting. The small size of the tribe plus the fact that they seldom came down to Fort Yukon may explain the lack of mention by early writers. My informants were agreed that the Dihai spoke a Kutchin dialect which differed but slightly from that used by the Chandalar or Nidse Kutchin. (Nidse seems the nearest phonetic equivalent of the name the Chandalar apply to themselves.)

The Dihai Kutchin were never numerous, and famine and disease together with frequent warfare with the Eskimo and Chandalar Kutchin further depleted their numbers, with the result that about two generations ago the surviving Dihai moved to the east fork of the Chandalar River where through inter-marriage they have been merged with the Nidse Kutchin. As a result many of the present Chandalar natives trace their ancestry to the now extinct Dihai group. Since the Dihai Kutchin formerly existed as a distinct Kutchin tribe they should be so recorded.

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## THE MESCALERO APACHE BOW-DRILL

In an article which recently appeared in the *AMERICAN ANTHROPOLOGIST* Paul S. Martin furnished evidence that the bow-drill, hitherto thought to be confined to northern North America, was used by the Pueblo Indians of the Southwest.<sup>1</sup>

I am able to confirm Dr Martin's conclusion of a southern extension of the distribution of the bow-drill by data I have gathered from the Mescalero Apache Indians, who ranged, before reservation days, over what is now western Texas, southeastern New Mexico, and northern Mexico. According to my Mescalero informants the bow-drill was employed for making fires by those who had difficulty with the hand-drill. The use of the latter was much more common, however.

No elaborate pains were taken in the manufacture of the Mescalero bow-drill. Pieces of pliable wood of lengths which varied according to the tastes of the individual users were crudely strung with sinew. No wooden nut or hand-piece was made ordinarily. Any available object, such as a piece of buckskin or rawhide, which would protect the hand was utilized instead. Even fighting-bows were converted into bow-drills on occasion. The Apache bow-drill was never used for drilling.

It is interesting to note that the Chiricahua Apache, the western neighbors of the Mescalero, denied that the bow-drill was ever used among them.

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## THE AZTEC CALENDAR STONE: A REPLY

TO THE EDITOR:

Since Señor Alfonso Caso of the Mexican Museum, in the *AMERICAN ANTHROPOLOGIST* for July-September, 1934, so caustically criticized my brief article, in the issue of October-December, 1933, on the remarkable resemblance of the design on the Aztec "Calendar Stone" to a compass card, calling it a "fantastic interpretation," may I be allowed a final word to say it was not an "interpretation" but merely a statement of fact.

A fact is a fact. Anybody can explain it anyway he prefers. The fact remains. And it is a fact that the so-called Calendar Stone of the Aztecs carries a design that is an exact, ornamental reproduction of the ancient compass card of the mariner's compass. Therefore, call it anything you like, the design is there. The resemblance seemed to me to be worth noting. It was not done through ignorance of Mexican and Central American archaeological literature, for I have been somewhat acquainted with that field for fifty years or more.

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NEW YORK CITY

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<sup>1</sup> Paul S. Martin, *The Bow-Drill in North America* (*American Anthropologist*, Vol. 36, 1934, pp. 94-97).

## NOTES AND NEWS

### REORGANIZATION OF THE LABORATORY OF ANTHROPOLOGY

At the Annual Meeting of the Board of Trustees of the Laboratory of Anthropology at Santa Fé, it was voted to amend the constitution to provide for an Advisory Board of not to exceed thirty-two members. The Advisory Board elects from among its members the Chairman and Vice-Chairman of the Laboratory to serve for one year. These officers, together with six other members of the Advisory Board serving for three years each, will constitute the active Board of Trustees. The Board of Trustees will meet twice a year, once in Santa Fé and once in the East, the eastern meeting being timed to correspond with the Annual Meeting of the Advisory Board.

The present make-up of the Board is as follows: A. V. Kidder, Chairman, C. E. Guthe, Vice-Chairman, Elsie Clews Parsons, A. L. Kroeber, J. F. Zimmerman, Fay-Cooper Cole, H. S. Colton, and Duncan Strong.

A. V. KIDDER

### TWO NEW JOURNALS

Kongo-Overzee, Tijdschrift voor en over Belgisch Kongo Ruanda-Urundi en aanpalende gewesten, is a new journal under the editorship of Dr A. Burssens (Melle bij Gent) with its first issue dated October, 1934. (Published by "De Sikkel," Kruishofstraat 223, Antwerpt; subscription, 85 Belgian francs.)

A second new journal, Zeitschrift fur Rassenkunde, is announced to appear in January, 1935, under the editorship of Egon Freiherr von Eickstedt (Breslau). Articles of moderate length in German, English, and French will concern "the evolution and the nature of race, and . . . the biological causes of its different physical and mental aspects." (Ferinand Enke Verlag, Stuttgart-W: two volumes per year, RM 18 per volume )

A NUMBER OF CASTS OF LARGE MAYA MONOLITHS FROM COPAN AND QUIRIGUA, for which the University Museum (University of Pennsylvania, Philadelphia) has no exhibition space, are offered to any museum having room for their display. Requests should be addressed to the Director of the University Museum, Mr H. H. F. Jayne.

SEVERAL SUMMER SEMINARS in fields of interest to anthropologists are announced by the American Council of Learned Societies. These are designed to give mature scholars opportunities for expanding their interests and competence in fields generally underworked by Americans. The Section on Chinese and Japanese Studies will be held at Columbia University (July 5-August 16, 1935) under the direction of D. C. Goodrich; a Russian Language Institute, also at Columbia (June 25-August 31), will be in charge of George Z. Patrick; while it is planned to hold a

Seminar on Arabic and Islamic Studies at Princeton University (June 20-July 31) under Philip K. Hitti for those whose major interests do not lie in this field.

It is hoped that some financial assistance can be offered to a few qualified scholars who might otherwise be prevented from attending any of these seminars. Further information can be had from Mortimer Graves, American Council of Learned Societies, 907 Fifteenth Street, N.W., Washington, D.C.

THE INSTITUTO ARQUEOLÓGICO DEL CUZCO, PERU, has been inaugurated as a center of advanced studies whose principal concern is with the past and present of the aboriginal American races. Luis E. Valcárcel, Director General del Museo Nacional, desires exchanges for the new institute with individuals and institutions, and offers all facilities for investigation in the Cuzco-Puno-Apurímac region.

THE 1935 SUMMER MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE will be held in Minneapolis, June 24th-29th. In this connection the University of Minnesota invites attention to some of its unique anthropological collections; among them, the skeleton of "Minnesota Man," skulls of Mechte-el-Arbi type and artifacts of the North African Capsian culture, and collections from Minnesota.

BY ERROR, in the AMERICAN ANTHROPOLOGIST, Vol. 35, p. 162, the book "Bland indianer i Ecuadors urskogar" (Helsingfors, 1920-1921, 2 vols.) was ascribed to the late Erland Nordenskiöld in a bibliography prepared by Henry Wassén. The author of this work is Raphael Karsten.



# American Anthropologist

NEW SERIES

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## NOTES ON THE CULTURAL PROVINCE OF THE SOUTHEAST

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**E**XCLUSIVE of peninsular Florida the aboriginal cultural province of the Southeast was about 1200 miles long by 600 broad. It was bounded by the Atlantic Ocean and Gulf of Mexico on the east and south respectively, and by the dry country beyond Trinity River, Texas, on the west, while on the north it merged into the region of the upper Mississippi and Ohio. Anciently much of this last formed a part, and seems to have been alternately attached to it and to the province of the Great Lakes where the principal tribes belonged to the Algonquian stock. This region is well watered everywhere and for the most part level or gently rolling though cut into on the northeast by the Appalachian Mountains and on the northwest by the Ozark and Ouachita plateaus. From the botanical point of view the southern and eastern sections constitute part of the southeastern mesophytic evergreen forest area and the northern part belongs to the deciduous forest. With the exception of southernmost Florida and very limited regions in Alabama, Mississippi, and Arkansas, it was all thickly forested when first known to Europeans. The Carolinian life zone of Merriam corresponds roughly to the deciduous area, his Austro-riparian life zone to the mesophytic evergreen forest, and the coastal region falls mainly in his Sabalian zone, leaving a bit of southeastern and southern Florida in his Tropical province.

Such was the natural environment of the American Indian of the Southeast, and we may, for our present purposes, discount any changes which took place after he first made his appearance. We are now to consider his adaptation to it. As is well known, the civilizing process consists in large measure in the change from a condition in which man has to adapt himself to nature to one in which nature becomes adapted to man. Food, both animal and vegetable, was to be had throughout the region, but, even before horticulture and the domestication of animals began, human selection, like the selection of an animal, would introduce a certain lack of conformity with the environment never to be entirely restored. For it does not follow

that man would utilize all of the products of nature suitable for his nourishment. Down to the present day he has continued to discover utilities in plants once neglected or even deemed harmful. In the case of animals his selective power was modified somewhat by the animals themselves. Thus, the potential food value of a deer was greater than that of some smaller animals but its ability to escape capture was also greater, and the choice of carnivora as food was somewhat discouraged by the defensive powers of those beasts. The abundance of fish in certain spots might have attracted a dense population, but not so long as the bare hands were the sole implements with which one could catch them. On the other hand plants did not have any means of resisting exploitation other than those which are purely passive. This fact, but more particularly the prevailing herbivorous character of man's nearest relatives, the anthropoids, suggests that his original diet was mainly vegetarian. But even then, as we learn from those same anthropoids, man probably had no aversion to animal flesh, and took to it readily as soon as he could provide himself with hunting and fishing implements.

If there ever was any perfect "adjustment of man to his environment" it was when he first became man, and such an adjustment has never been restored. But even during what we may term his vegetarian period it is probable, as hinted above, that man developed selective tastes which somewhat modified the original condition. He may have taken to roots rather than fruits or grains, or to one of these latter rather than the other two. Or tribes—if there were then tribes—may have developed mutual differences in this respect. Although algæ and other marine growths may furnish a certain amount of nourishment and man may soon have opened up an acquaintance with shellfish, it seems probable that in the vegetarian period the greater part of the human race lived inland.

When hunting and fishing techniques came into existence, a shift of population undoubtedly occurred. Grazing territories and salt licks, the resorts of deer, elk, and other herbivorous animals would become much more attractive and would tend to draw mankind to the open country. But the principal change was certainly brought about by the invention of devices for catching fish, whether by lines, nets, or traps. As the potential food supplies were much greater along the sea shores and appreciably greater along lakes and rivers than elsewhere, population would mass primarily along the former and secondarily along the latter.

Up to this point the movements of population I have suggested are hypothetical and, in any event, they did not take place in the province which is the subject of our discussion because there is every reason to

believe that the hunting and fishing stages were reached long before the western hemisphere was occupied by man. The assumed later massing of population along oceanic coasts and on lakes and rivers is, however, testified to by the estimates of Indian population north of Mexico made by Mooney and so skilfully handled and supplemented by Kroeber. Contrast between coast and interior is most marked along the Pacific but is apparent in almost equal degree on the Atlantic side of the continent and along the shores of the Arctic.

It happens that it is precisely in the Southeast that the greatest apparent exceptions occur, and this is interesting because of evidence that at one time the littoral population actually was heavier, and thus we seem to have evidence that still another revolution had begun. In terms of square miles the number of Indians on the Atlantic coast from the Arctic as far south as Cape Lookout, N.C., and again from Charleston Harbor around the southern end of Florida to Tampa Bay was upon the whole denser than the interior population. Again, beginning with the Chitimacha of Louisiana, the coastal population of Louisiana and Texas was, generally speaking, heavier than that of the interior, if we except the Mississippi Valley itself which constituted something of an independent province. The region immediately at the mouth of the Mississippi was unsuited to large settlements. The coast of the Carolinas between Cape Lookout and Charleston, except in spots, is not well protected by offshore bars, but it may be added that the hinterland itself was thinly occupied. The coast of western Florida and neighboring states, between Tampa Bay and the Mississippi, constitutes our greatest exception, but it is precisely here that we find the most abundant marks of an earlier heavy population. The mounds and shell heaps of Tarpon Springs, Crystal River, St. Andrews Bay, Choctawhatchee Bay, Pensacola, and Mobile are well known. There is also evidence that the abandonment of at least part of this shoreline was very recent. Evacuation of the Georgia coast took place after Spanish colonization and might be attributed to the whites, but the same can hardly have been true of the abandonment of Mobile Bay. In 1519 the Spanish navigator Pineda entered a "river," which was almost certainly Mobile Bay or Mobile Bay and River, and found forty towns along its shores. In 1699, however, when the French colonized this region, there were no Indians living on the bay, though there were plenty of deserted town sites. The preponderant littoral population which the white colonists found in some sections seems, therefore, to have represented a condition formerly general.

Next to the sea coast the rivers apparently furnished favorite locations for the native population and for the same reason, because they provided

an immediate and considerable source of food and were at least as convenient to the hunting territories as any others. But there were spots on most of the rivers where fishing was particularly good, and in the eastern part of the province the most important of these were marked out by the "fall line," where the piedmont plateau gave place to the coast plain. Down to the present day such places are the sites of important towns and cities and the same was true in aboriginal times, although in part of Virginia this line fell between the two great stocks of the region, the Algonquian and Siouan. Along the Mississippi Indian population usually collected at points where the river touched the bluffs, though there is evidence of a massing of people at an earlier day throughout the lowlands between the Mississippi and Ouachita, the Mississippi and Yazoo, and the Mississippi and St. Francis. This is something of a mystery. Are we to assume that fishing advantages outweighed the discomfort created periodically by the overflow of the great river?

In the eastern part of the province the coast and the fall line account between them for much of the distribution of population except for the presence in the southern Appalachians of the largest tribe of all, the Cherokee. In this case, however, we have fairly good evidence that we are dealing with an invading people originally belonging to the riparian lands of the upper Ohio, and it is probable that, though a favorable climate and facilities for defence offered certain attractions, the Cherokee were being pressed into it by other tribes, probably their own congeners on the north. To the other advantages which it possessed, we should very likely add, however, the control of quarries and mines and the opportunity of acting as middle-men between the Indians of the Ohio and those of the Gulf.

In spite of the strong influences exercised by the coast and inland waters of the section, I think we have evidence that, when white men appeared, their dominance was beginning to give way. Still another revolution was in progress. Introduction of agriculture from the south, although at first it provided merely a supplementary means of livelihood to the existing communities and had little effect on the location of the people as a whole, came in time, it would seem, to occupy such a commanding position that every other source of livelihood was sacrificed to it, and the geographical positions of several tribes were changed bodily. Even in Florida we find the bulk of the Timucua population on the Alachua plains or along the St. Johns instead of on the coast, while the Apalachee were more of an inland agricultural people than a fishing or hunting tribe. Among the smaller Siouan communities we hear of the Occaneechi and Eno, including probably the Shakori, as devoting particular attention to their crops and raising a surplus

for trade. This and their favorable location for commerce, probably explains the inland position of the Catawba. The possibility of raising corn without the necessity of laborious forest clearing operations must be set down as an added influence determining the inland and upland location of the Cherokee.

A factor operating over almost the whole region east of the Mississippi was the presence of what is known as the pine-barren region which occupies over half of the coastal plain. In the *Journal of Geography* for October, 1916 (page 43) Harper says:

Half a century ago this region was regarded as almost worthless for agricultural purposes. The population averaged less than ten inhabitants to the square mile, and the chief industries were lumbering, turpentine, and stockraising. . . . Cultivation of the soil was chiefly confined to the vicinity of rivers, to spots where a clay stratum approached the surface, and to the small areas that could be fertilized with the available stable manure; the cleared land made up considerably less than 10 percent of the total area.

Harper's paper was written to point out the great change that had taken place in the use and value of that land with the application of commercial fertilizer, and we can easily understand why the Indians, who used no fertilizer, when they came to prefer agriculture to fishing, moved back out of the barrens to the richer soils beyond. Incidentally it helps to explain the persistence of the whites in driving the Indians from their inland territories.

To the west the situation is still more striking, since we find the three great "nations" known as Creeks, Chickasaw, and Choctaw withdrawn from the Gulf coast, which is less favorable to corn raising, and from the Mississippi. Rivers of secondary importance ran through the Creek country, but both the Chickasaw and Choctaw were noticeably removed from the larger streams and massed about the heads of minor water courses. In particular, we are told that the Choctaw were rather a tribe of farmers than hunters, that they rarely left their territory even to fight, that most of them were not able to swim, and that they raised a surplus of corn every year which they sold to the Chickasaw. West of the Mississippi the Hasinai confederation presents us with an analogous example, located as it was between, but not on, the Sabine and Trinity and among the headwaters of such minor rivers as the Neches, while the Caddo confederation was about the raft of Red River which effectually prevented the use of that stream as a thoroughfare.

Aside from a hypothetical shift of population from interior to coast which took place, if at all, in the Old World, we have indicated in the South-

east a shift from the coast and important inland waterways to the interior, not perfected but apparently taking place when Europeans made their appearance. The first change we may assume to have been due to the substitution of fishing for food gathering, the second to the introduction of agriculture. In its last phase determination of population away from large bodies of water may have been due to fear of enemies and perhaps also of epidemics, though the thinning of population along main arteries of communication may itself have been due to the epidemics. From the purely economic point of view, the inland Choctaw would seem to stand on the top of the pyramid, though the Creeks were little if any inferior to them.

It is impossible to weigh the esthetic evolution of tribes in this territory as confidently as their relative economic development because of lack of material. We know that wood was used considerably for artistic treatment but specimens of the art have been preserved only in Florida. The artistic talents of most of the tribes of the section were expressed in tattooings and paintings on the human skin and have, of course, disappeared, and on textiles and copper of which only a few fragmentary specimens remain. According to early writers, baskets and mats were often beautifully woven in colors but in only a few spots has the art been preserved, and in only one place, in Louisiana, has it retained a suggestion of its ancient charm. It would manifestly be unfair to estimate the artistic accomplishments of the several tribes by this art as it exists today, but it is perhaps not accidental that the Chitimacha, who retain the best technique, were near neighbors of the Caddo who seem to have surpassed all other tribes in their ceramics.

There is much excellence in the pottery ware of the eastern Gulf section, particularly that of northwestern Florida, and in the paddle marked productions of the Cherokee, but ceramics clearly improve as we go west. That of Moundville, represented also on the Coosa and Tallapoosa and even in northwestern Florida, distinctly surpasses anything to the east of it, and is connected with the pottery of the middle Mississippi extending from the mouth of the Arkansas to the Ohio and beyond. This is itself superior in many respects, considering the variety and ornamentation of effigy pots. In the esthetic feeling exhibited in the treatment of its scrolls and in its lustrous finish the Natchesan or lower Mississippi ceramics are at least equally good. When we come to the engraved pottery of the Caddo, however, made largely by the Indians of northwestern Louisiana, southwestern Arkansas, and northeastern Texas, we reach the real summit of artistic expression in the region as exhibited in the superb scroll work, the harmonious coloring, and the excellent finish. In general, we may say with-

out risk that the esthetic center of the Southeast, as illustrated by pottery and at the present time by basketry, was along the lower Mississippi and the middle course of Red River, but that the highest excellence seems to have been attained in the western part of this area at the very edge of the Southeastern province.

Is there any possible way to account for the eminence of the tribes in question? I do not venture a simple categorical reply to this. So many and various have been the influences at work in the section, so numerous the displacements of tribes and the shufflings of the pack of peoples, that a single cause is practically excluded. At the same time I cannot refrain from pointing out two possible factors which may have contributed to the results noted. One of these is the relative antiquity of culture in the region where it reached its apex. Although we cannot state the fact with certainty, and shall not be able to until archaeology has completed its labors, there are strong indications that the lower Mississippi included the center of culture for the eastern area of the present United States. Except for a belt along the Gulf of Mexico which was perhaps responsible for the secondary peak, cultural influences seem to have spread from the lower Mississippi rather than to it, and the spread of peoples was similar. Other things being equal we should look for higher culture where culture has had most time to develop. Secondly, the Caddo were so situated that they may well have constituted a focal point for more diverse cultures than any other group of tribes. To the northeast of them were the Siouan Quapaw, and, not far beyond these, representatives of the Algonquian stock. Directly east lay the Tunica, Yazoo, Koroa, and a few other bands of Indians who constituted a somewhat unique division, beyond them the great Chickasaw and Choctaw nations of the Muskogean stock, and on the lower course of Red River and the banks of the Mississippi beyond it were tribes belonging to the Natchesan division of the Muskogean family. These last had attained, as we have seen and as we shall continue to see, a peculiarly high civilization. A little farther south were the Chitimacha Indians, fabricators of those excellent baskets of which mention has just been made. West of these again were the Atakapa, and directly west of the Caddo miserable wandering bands of Tonkawa. While the Tonkawa and Atakapa could have added little of a positive nature to Caddo culture, there is a certain stimulus in difference even though of a minus character provided it is not too ubiquitous and overpowering.

We may sum up, then, by saying that, exclusive of the Algonquians, who were relatively late comers, the Caddo were in communication with no less than seven groups of tribes of widely varying speech and at one

time at least of widely varying culture. As much could not be said for the greater part of their neighbors, the ones circumstanced most like them being the tribes along the Mississippi who were on almost the same cultural level. It may be added that ethnological research suggests prehistoric contacts of a more intimate character with the Muskogee or Creeks, and a prehistoric people who once found homes in the skirts of the Ozarks.

Within the historic period the tribes most noted for their ceremonialism were in the region just indicated, including particularly the Natchez and Taensa. Moreover, our earliest records of the region, the chronicles of the De Soto expedition, affirm that the lower Mississippi tribes were the most civilized except for one group near the present Augusta, Georgia, which there is reason to think had emigrated from the Mississippi section shortly before the Spaniards encountered them. Ceremonialism was further developed by the Creeks than among the Chickasaw and Choctaw, but there is rather strong evidence that they formed part of the same movement away from the great river, and that the position of the Chickasaw represented a later intrusion.

But when we turn our attention to attainments in the art of government, we are compelled to award first place to the Iroquois and second place to the Creeks. In doing so we must reject the dictum that "that government is best which governs least" as a principle upon which to base our conclusions. From the point of view of the factors involved, or for the purposes that they had to serve, the governments of the Choctaw, Cherokee, Caddo, and many smaller tribes were just as good as the two just mentioned—from Jefferson's point of view better. We must also reject absolutism as a criterion of superiority, one which, if accepted, would have placed the Natchez in the foremost rank. But although the Natchez theocracy included some alien tribes, the problem confronting Iroquois and Creek statesmen was much more difficult than that which the Natchez Great Sun had to face, for the problem with them was not the mere addition of alien tribes but the evolution of a common system of usages, legal and governmental procedure and the accompanying concepts, acceptable to a considerable number of originally independent and mutually hostile peoples. So far as we may judge there never was any great diversity among the people which constituted the Choctaw, Cherokee, and Caddo nations. A few small bands were added to certain of them at a late period, but they were too insignificant to affect the polity of the tribe as a whole, and this was equally the case with the Natchez.

The Creek state partook less of the nature of a free union of peoples than that of the Iroquois since one particular group of bands occupied a



position of numerical and moral dominance. To this group belongs properly the name Muskogee, though that is probably foreign in origin. As the original organization seems to have been confined to this group, it may once have been comparable to the organization of the Iroquois, a voluntary union among equals, but from the time when it came clearly to the knowledge of Europeans almost half of the federal body consisted of peoples of alien speech who, though not oppressed, were in some measure looked down upon by the Muskogee. According to the only fragments of the national epic which have survived, two of the main bands of Muskogee, having subdued or driven away all of their enemies, agreed to institute an intertribal ball game in order to keep their martial spirit alive. These were the Coweta and the Kasihta. It was also agreed that each might adopt bodies of related or alien people who would then form one "fire" with it and might participate in the game on its behalf if this were subscribed to in preparing for the contest. Coweta and its towns came to be associated with war, while Kasihta headed the peace side, and for obvious reasons took in more outsiders than its rivals.

Putting history and certain traditions together, however, we find rather strong indications that the most important step toward the confederacy was taken when peace was made between some of the eastern Muskogee and their Hitchiti-speaking neighbors in southern Georgia. These last comprised eight or ten towns or tribes which formed an eastward extension of the southern group of Muskhogean, of which the Choctaw, Chickasaw, Alabama, Apalachee, and Yamasee were other typical representatives. The Muskogee apparently came in contact with these after having entered the country from the northwest. This accounts for the traditional place of origin recorded by Bartram at the Hitchiti town of Ocmulgee on the river of that name, and the one given by Chekilli, the Creek chief, to Oglethorpe, to wit the Apalachicola town of the Hitchiti on the Chattahoochee River.

When De Soto visited the Creek country it would seem that an understanding or very loose alliance existed between the Muskogee bands on Savannah River and those on the Coosa, but the former were then at war with the Hitchiti, and therefore it is probable that the second phase of the alliance did not take place until later. It was fully established before the opening of the eighteenth century, probably as early as 1650, when it had brought together most of the tribes of central and southern Georgia and central and eastern Alabama. While it never got appreciably east of the Savannah or west of the Tombigbee, and lost some northern territory to the Cherokee, it gradually extended southward, aided by the English colonists, until it came to include the Floridian peninsula and destroyed or

incorporated most of the former tribes of that section.<sup>1</sup> Part of the Apalachee remained independent, however, and removed to Louisiana. Most of those who first settled in Florida belonged to the Hitchiti division of the nation and included bands which preferred an isolated, unregimented life to contact with the great towns. Such people were known as Seminole, and the name became applied more particularly to the settlers in Florida. After the Creek war of 1813-14, the Seminole were joined and well-nigh swamped by numbers of Indians from the Upper Towns. These were principally true Muskogee, so that the complexion of the Florida Indians was changed from one prevailing Hitchiti to one prevailing Muskogee. Owing first to the Hitchiti origin of the original nucleus and second to the factional opposition between the later refugees and those Creeks who remained in their old homes, particularly their opposition to the Lower Creeks, a distinct Seminole tribe grew up which received recognition from the United States Government eventuating in a distinct treatment for this part of the original nation. Those who removed to Oklahoma were given a separate strip of territory and organized a government of their own which endured until all of the governments of the Five Civilized Tribes were brought to an end.

The organization of the Iroquois confederation was still more remarkable since it appears to have been a result of the deliberate attempt of two Indian reformers, Dekanawida and Hiawatha, to put an end to the fratricidal warfare between tribes. It extended rapidly to the so-called Five Nations, the Mohawk, Oneida, Onondaga, Cayuga, and Seneca, later incorporated the Tuscarora, and added portions of the Huron, Tionontati, Neutral Nation, Erie, and Susquehanna who were of the same stock, some New England tribes, Nanticoke, Delaware, Conoy, and even Fox of the Algonquian family and remnants of the Siouan Tutelo and Saponi, besides numbers of individuals of all the surrounding people. These last were brought in by successful wars which the federated Iroquois waged, as, it would seem, an unpremeditated result of their union for peace. The organization, so successful in defence, was found to be equally efficacious in conducting offensive operations, so that the "Iroquois empire" extended at one time from Ottawa River to the Tennessee and from the Kennebec in Maine to Lake Michigan and the River of the Illinois. As in the case of the Creeks, considerable of this expansion took place after white contact, and was due in some measure to supplies of firearms obtained from the colonists.

The above review of Southeastern culture suggests that there has been

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<sup>1</sup> Since, however, the Florida immigrants very rapidly assumed an independent status, the extension of the Creek Confederation over Florida was rather theoretical than real.

a movement of population from the coast to the interior, and a further trend away from the larger rivers. Nevertheless, economic, esthetic, and ritualistic primacy seems to have been attained by peoples on or near the lower course of the Mississippi. Governmental and legalistic superiority, however, appear in connection with marginal tribes, though one of these had probably moved from the same region shortly before the appearance of the whites. This marginal position is not singular, however, since the Assyrians and Persians were marginal to the higher cultures of Babylonia and Egypt, Rome was marginal to the cultures of Greece and the Orient, and the Aztecs were marginal to the Maya.

Tradition, linguistic diversity, and what little we know of the archaeology of the section indicate, however, that the history of its population has been much more complicated than the preceding discussion might indicate.

A few tribes belonging to the Algonquian family occupied outlying sections of the territory. When white men first appeared, they were as far down on the Atlantic seaboard as Pamlico Sound and were pushing southward from the Great Lakes, one tribe, the Shawnee, having reached Cumberland River. This movement has sometimes created the impression that Algonquian occupancy of the region was very modern, but archaeological investigations in various parts of eastern America indicate clearly that there was an earlier expansion of this group of people. Their remains have been found in New York, apparently antedating Iroquois occupancy, and, what is more important for our purposes, M. R. Harrington discovered along the upper Tennessee remains of a people which he ties up with the coastal Algonquians. Webb has made similar discoveries in Kentucky, and, although Claflin appears not to have noticed it, his report on the Stallings Island culture at Augusta, Georgia, exhibits potsherds strikingly like those of the Chesapeake Bay region. As the Stallings Island people are reported to have been brachycephalic, either the physical type had undergone a change or some other stock had adopted Algonquian pottery types. However, of the resemblance there can be no doubt, and the extent of this Algonquian or Algonquinoid occupation is a problem requiring careful examination.<sup>2</sup>

While Algonquians are usually associated with the north, the remaining stocks may well have come from the west where their languages find closer affinities. In the light of certain resemblances between Iroquoian and Cad-

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<sup>2</sup> The term "Algonquian" has been used in a rather loose way as applied to physical types and archaeological remains. While the associations of the cultural material mentioned seem clearly northern, the accompanying physical types do not appear to be and further investigation may render a wholly new interpretation necessary.

doan speech, it is suggested that the ancestors of these families may have entered the country at about the same period and we may assume from their geographic position that they may have preceded all but the Algonquians. At a later date the Muskogean-Siouan group may have followed them, the Siouans pushing north and ultimately separating Iroquoians from Caddoans. The Yuchi are a people apart, but seemingly nearest related to the Muskogean-Siouans, as were the smaller stocks of Florida, Louisiana, and Texas. What relation most of these stocks and their subdivisions bore to the archaeological remains constitutes a sheaf of problems equally interesting and intricate.

The possibility of unraveling these relations and so reconstructing the prehistory of the Southeast has hitherto seemed so remote that many have been deterred from entering upon the investigation. But, in the first place, there is no more certain way of insuring failure than to begin by postulating it, and, besides, enough has already been accomplished to give every encouragement for the future. A few of these accomplishments may be listed:

1. The presence of Algonquians or of people sharing the culture of Algonquians has been demonstrated by Harrington, Webb, and Claflin in territories later occupied by the more characteristic Southeastern stocks.

2. Discovery of the Bluff Culture of the Ozarks gives direct proof of intimate contact between the Southwest and Southeast at one period in the past, though the part which that culture played in Southeastern evolution is as yet uncertain.

3. Connection between the Lower Mississippi Culture and the Natchez Indians has been proved by Collins and Ford.

4. Connection between some of the Muskogean tribes and the Middle Mississippi Culture is strongly indicated.

5. Connection seems to have been established between one of the Siouan tribes and the Fort Ancient Culture.

6. A connection has been established by Setzler between the Hopewell culture of the upper Mississippi, particularly Ohio, and the culture exhibited on several sites in Louisiana.

7. Kelly's splendid work on the Macon and Lamar mound groups in central Georgia has opened up unexpected possibilities in chronological reconstruction in the area.

8. The investigations of Webb and Lewis in Kentucky and Tennessee, and the beginnings made synchronously by Douglass and Hawley toward the establishment of a dendrochronology for the Cumberland-Tennessee section represent another important development.

9. And cultural sequences of great interest and well supported by field

data have been worked out both in the upper and in the lower Mississippi Valley, though, as yet, little of this is widely known.

In the above discussion a few suggestions have been ventured regarding the cultural condition of the Southeast when it was first visited by Europeans, some factors in its prehistory, and some problems for future investigation. There is every reason to believe that we are about to see an expansion of our knowledge in this area comparable in many ways to that which has already taken place in the Southwest.

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## FUNCTIONALISM IN SOCIAL ANTHROPOLOGY<sup>1</sup>

By ALEXANDER LESSER

THE most vigorous tendency in social anthropology today is that of functionalism. To some the functional approach has seemed a radically new departure which invalidated earlier methods and interests; to others it has appeared a false doctrine, itself invalidated by already established technics. Several factors may be held accountable, however, for the fact that the issue between these antithetical reactions has not been clearly drawn.

The functionalist tends to assume that there exists, apart from functionalism, a homogeneous and unified subject matter, so that a disjunction can be made between functional social anthropology on the one hand and non- or pre-functional social anthropology on the other. Since as a matter of fact a considerable variety of methods and interests is to be found among non-functionalists, the functionalist often appears, from the standpoint of particular adversaries, to deny what is not asserted and to assert what is not denied.

The non-functionalist, motivated as he is at times by an understandable desire in the face of attack to hold fast to familiar ideas, often fails to distinguish the particular interests and conceptions of individual functionalists from the broader, more basic meaning of the functionalist approach. This confusion arises not only from the lack of unity among functionalists, but rests also upon a failure to differentiate content from method. Functionalists themselves are in part to blame for this ambiguity. Individual functionalists often concentrate upon the study of special phases of the subject matter and tend on occasion to identify the subject matter as such with these particular interests, and method with their particular procedure in handling a special subject. It is often not at all clear whether the functionalist is insisting upon the adoption of a certain method or the study of a certain subject.

Clarity demands the discrimination of content and method. Scientific method as such is not limited in its application to any one phase of phenomena but can be applied wherever intellectual control of things is necessary. If functionalism involves a method which is of value in ethnology, it must be a mode of procedure which is independent of the particular use that has been made of it by functionalists.

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<sup>1</sup> As read at the joint sessions of the American Anthropological Association, the American Folk-Lore Society, and Section II of the American Association for the Advancement of Science, Pittsburgh, Pa., December 27, 1934.

Functionalists have not been alone in bringing to attention aspects of culture which in the past have been too much overlooked, but in particular they are responsible for the most part for the introduction into social anthropology, or in any case for the emphasis in social anthropology, of certain special types of cultural phenomena. Outstanding is, no doubt, the stressing of psychological aspects of culture. Factors in the molding of individual personality, conflict points in family and social life, dominant group or cultural attitudes, and the psychological roots of institutional life, have been included in ethnological subject matter by various students. There has been a tendency also to emphasize the study of particular kinds of content more than others: sociological problems of aggregation and institutional function, economic institutions and their cultural roots, law in its basic relation to social life and culture, and, not the least of the emphases, detailed consideration of sexual life and education for the light they throw on fundamental differences of psychological patterns.

These suggestions, though obviously inadequate as a summary of functionalist interests, may serve for my purpose to indicate the way in which certain emphases in content have come into the subject matter of social anthropology along with functionalism. The question that concerns us is to what extent the ethnologist who is committed to functional method is committed to functional content.

The introduction of new emphases in content or interest must be expected in any field of inquiry. The growth and development of ideas in other fields is bound to produce a demand for comparable information such that a cross-fertilization will result. What has happened in social anthropology has obviously been that students have brought interests from other fields, notably psychology, sociology, economics, and law, and have worked ethnologically along the lines of these interests. All who share the conviction that social sciences are not water-tight compartments but merely the specialized treatments of different aspects of culture, must recognize that coöperative and collaborative effort between social scientists in different fields must be the dominant tendency if sound and unified social science is to result. Some of us, be it admitted, have felt that social sciences which have been limited to the phenomena of our culture alone have in the end more to learn from anthropology than social anthropology has to learn from these narrower disciplines. We have therefore at times deplored the introduction from other disciplines into anthropology of methods which seemed shaky and immature. Nevertheless, ultimate judgment of the value of approaching ethnological data from the standpoints of other social and psychological sciences must rest upon the pragmatic test. The enterprise will finally be judged in terms of its accomplishment, and meanwhile,

recognizing the difficulties inherent in these transfers of method, we must welcome the attempt.

But the fundamental question to which I return is whether the ethnologist who values the functional approach must therefore adopt the content stressed by functionalists. Must the traditional ethnologist, who, for example, has been primarily interested in cultural structure—in social and religious life, in economics and material culture, in mythology and art—must that ethnologist put aside his own interests and assume that newer tendencies of method demand that he become psychologically minded and concern himself primarily with the patterning of attitudes?

It is my contention that there is no necessary connection between the essentials of functional method and the particulars of functional content. Subject to critical consideration, there seems much of value for the ethnologist in the broad fundamentals of functionalism as a procedure. But the ethnologist must discriminate between particular functionalist claims and essential ideas. He may or may not adopt the interests of the functionalists, but he should adopt from them certain attitudes toward the study of culture and cultural problems. From this standpoint I should like to review briefly the antithesis between functional and non-functional approaches as regards fundamentals of method.

Functionalism, as any new movement in science, represents a reaction against doctrines felt to be outmoded. Functionalists have explained their own beginnings as determined primarily by an attempt to get away from the evolutionary conception of social history and from the so-called evolutionary comparative method. When this earlier view dominated investigation, an observed cultural fact was seen not in terms of what it was at the time of observation but in terms of what it must stand for in reference to what had formerly been the case. Investigation brought out little of what the facts are and much of what the course of evolution was conceived to have been.

From the functionalist standpoint this earlier approach substituted theorizing for the discovery of facts. The reality of events, however, consisted of their manifestations in the present. Hence if events are to be understood it is their contemporary functioning which must be observed and recorded. The past as such is irrelevant, the present is primary. What is the case is what is seen to be the case here and now, and definition of process and function must be determined in phenomena as they are observed. The functionalist emphasizes the doctrine that investigation of customs and institutions must begin with their relation to immediate or contemporary conditions, he stresses the fallacy of assuming that remote factors are always more important than immediate conditions; and he tends toward the extreme



of assuming that *only* contemporary conditions and factors are relevant.

That knowledge begins in the understanding of present experience is fundamental and unassailable. What is given is always contemporary experience, and it is true that knowledge of the past must necessarily be based upon contemporary events and processes. Since our experience in our own contemporary world is primary, and since our knowledge must be based upon it, the functionalist is correct in asserting that the primary subject of attention must be the present and contemporary functioning of things. This is true whether we are concerned with knowledge of the past or knowledge of the present. The archaeologist infers the past from remains found in the present, in terms of their relations to other factors known in the present. The past is always an inferential reconstruction drawn from present facts and conditions.

If we are concerned with the determinants of institutions and customs it follows that, whether in the end the determinants lie in past or in present conditions, procedure must begin by seeking explanation of the events first of all in present conditions and processes. Only when and in so far as the form being analyzed cannot be understood in terms of relations to other factors in the present, need we turn backward in time to find the past or former conditions which are major or relevant determinants. The procedure begins by using present conditions to define the determinants and relations of present events so far as that is possible, and then seeking factors more remote in time for the understanding of what is left unexplained by present conditions.

The fundamental question that concerns us here is in how far this turning backward to the past actually becomes necessary in practice. It is the tendency of the functionalist to deny that it ever does. What exists in the present has a present function, and to discover that function is the end of research. The functionalist often seems to assert that we must not only begin with the relations of cultural aspects in the present, but we must end there. Since history is merely inferred from present conditions, it is conceived irrelevant to the understanding of present conditions. This conception betrays a tendency to believe that knowledge based on inference is necessarily hypothetical and relatively uncertain. Inference, however, is a mode of thinking which is basic not only to the reconstruction of history, but to the derivation of functional relations in the present as well. Results that are based on inference are not as such theoretical or uncertain. The extent to which such results are to be accepted as valid and factual depends, both in the case of the establishment of functional relations and in the case of historical inferences, upon what the evidence is and what implications it supports.

The distrust of the functionalists for history derives in part from their distaste for evolutionary conceptions and their recognition that evolutionary ideas stemmed from historical interests. Evolutionary conceptions were embedded in nineteenth century historical tendencies in social science and human thought generally, and evolutionary anthropologists were no doubt historically minded when they set out to reconstruct a history of human culture and institutions. But is history to be impugned because of the errors in a particular conception of its nature?

Functionalists were not alone in their reaction against evolutionary methods. In point of fact they were anticipated in time by Franz Boas and the American school, and the reaction against evolutionary methods includes also the diffusionist approaches of English and German anthropologists. In the approach of the American school the point of departure was different. Evolution as a principle in social anthropology was attacked not because it was conceived as historical, but because it was shown by an appeal to the facts that it was not history. The critique was founded not on metaphysics but on an empirical investigation. The method called for a return to the study of cultures as such, insisted that hypotheses must spring from and be adjusted to the realities of cultural phenomena, and denied that cultural data could be deduced from and fitted to theories. The facts observed did not indicate that the course of human history had been what the evolutionists described. So much the worse for evolutionary conceptions. But evolution was never identified with history. Evolutionary doctrines were recognized to be deductive philosophies of history. Factual history was quite otherwise, and in attacking evolution, the American school did not impugn the significance of valid history, but on the contrary found that valid history was instrumental to adequate cultural understanding.

The American position is associated with a definite conception of history. The processes which control events lie embedded in time as well as place, hence the determining conditions and the associations and connections of events are in the past as much as (if not more than) in the present. From the standpoint of the time at which any event is viewed, we are dealing with a temporal cross-section of a continuum of events in time, and any aspect to which attention is directed is an end-point of continuing change, an end-point of the historic process. Hence while investigation must begin with what is the case in the present, it cannot end there. It is impossible to disregard the existence of a past because the career in time of anything or any event is more than momentary, and its nature and characteristics must be understood in terms of its relations to other events and things regardless of temporal limitations.

Justice demands that extremist tendencies of the American school be

also kept in mind. Affirming an emphasis in research upon exact historical fact, the American ethnologist too often assumes that the determination of what the facts are in temporal historical terms is not only the basis of empirical knowledge but the end of research. Thus, whereas the functionalist, annoyed at the results of false historical theorizing, turns away from history to limit himself strictly to the consideration of immediate conditions in the present and contemporary, the American ethnologist, starting from a factual critique of inaccurate deductive history, and attempting to replace it with sound history, has in so doing often limited himself too strictly to the consideration of remote temporal relations of the conditions and events of the present.

In short, both the functionalist school and the historical school have at times been guilty of special and narrow biases. Historical ethnologists have attempted to develop short-cut methods to history and historical reconstruction, which short-cut methods are not only open to question as empirical methodologies, but which can in any case result only in the reconstruction of mere chronology in time of unrelated events. History cannot be identified with such mechanical reconstructions.

Functionalists, on the other hand, have too often identified the investigation of particular questions and the study of specialized aspects of the subject matter with correctness of method and content, and are too ready to identify history with bad history.

But apart from such confusions and occasional short-sightedness, there are at the root of both approaches certain sound and unassailable methodological assertions. Both have affirmed the necessity of a return to the study of cultural facts as they are found living and functioning in the present; both have insisted that customs and institutions be investigated in relation to their contexts and not apart from them. Cultural functions and functioning in the minds of the functionalists is no different in kind from the familiar emphasis of American ethnologists upon the necessity of studying the interrelationship of the aspects of culture.

The extremes toward which these apparently divergent doctrines tend point the moral of sound method. On the one hand, the functionalist, insisting upon founding his functional statements upon immediate relations in the present, is too blind to the fact that determining and fundamental relations only too often lie beyond the present in the past. And, on the other hand, the historically minded ethnologist is too ready to seek remote historical relationships and overlook others nearer at hand. Obviously, it is true that the first consideration must be of the context of cultural phenomena in the present, and it is also true that for the most part determinations of events in the present lie in the past. In beginning with present conditions,

exact understanding of any particular institution or custom demands not only the calculation of its apparent connections in the present, but even more a recourse to the past, *so far as it is relevant* to the particular inquiry, for an understanding of the determining relationships which lie behind the event.

In its logical essentials, what is a functional relation? Is it any different in kind from functional relations in other fields of science? I think not. A genuinely functional relation is one which is established between two or more terms or variables such that it can be asserted that under certain defined conditions (which form one term of the relation) certain determined expressions of those conditions (which is the other term of the relation) are observed. The functional relation or relations asserted of any delimited aspect of culture must be such as explain the nature and character of the delimited aspect under defined conditions.

How are such functional relations to be established? First of all, we begin, as in any science, with observation. We see such and such events going on. Many things are always happening at the same time, however. How are we to determine whether or not those things which happen at the same time are related to one another? For it is obvious that they may be contemporary events, or even serial events, not because they are related to one another but because their determinants, unknown and unobserved, have caused them to happen at the same or subsequent times. In short, contemporary or associated events may be merely coexistences. Culture, at any one time, is first and foremost a mass of coexistent events. If we are to attempt to define relationships between such events it is impossible in view of the known historicity of things, to assume that the relations lie on the contemporary surface of events. Whatever occurs is determined more by events which happened prior to the occasion in question than by what can be observed contemporaneously with it. As soon as we turn to prior events for an understanding of events observed, we are turning to history. History is no more than that. It is a utilization of the conditioning fact of historicity for the elucidation of seen events.

There is, however, a further difficulty. Just as it is impossible to assume, or to derive by intuitional methods the functional relations of things in the present, so by turning backward to the past it is impossible by mere inspection to find the significant relationships of past to present events. The required methodology is more complicated than that. It demands the consideration of all alternatives, and for exact determination of relevant relationships it calls for the comparison of many instances. Generic and fundamental relationships must be rigorously defined so that it can be asserted of them that these were the actually related conditions of the seen

phenomenon, and that this was the phenomenal expression of those conditions and not any other.

The determination of such relationships is the definition of cultural functions and is inescapable if ethnological method is to be scientific. The *conditions* which functional investigation must take account of can be generalized as historicity—the fact that institutions, customs, beliefs, artifacts, have careers in time, and that their form and character is molded more by what has happened to them in the course of that history than by what particular things they occur associated with at any one time. Progressive method in social anthropology must increasingly eschew narrow biases and limitations and must approach the study of culture in terms of a functional historicity.

But the conception of functional historicity does not predetermine the content of subject matter. Content is determined by problems selected for treatment. Given a certain problem, the investigator carves out of the whole a relevant subject matter which includes his necessary data. What problems are to be attacked, and hence what subject matters are to be emphasized, will be determined at any time by the interests and training of the investigator and by considerations of the relevance of ethnological subject matter to broad questions of interests both inside and outside of ethnology. It is not only the problems and content stressed by the functionalists which can be handled in terms of a functional historicity. Whatever problems the ethnologist finds of importance can and should be so treated, including not only psychological and socio-psychological problems, but also the familiar questions of musical style, mechanical principles in material culture, form and style in art and artifacts, the structures of institutions and beliefs, etc., and even the historical reconstruction of the past itself—that primary bugaboo of the functionalist. For why should there not be a reconstruction of the past which takes account of the functional and significant relations of events?

The suggestions I offer imply no radical departure. Functional historicity calls only for a realization of the necessity of defining the functional or significant relations in culture—without which scientific knowledge is impossible—and of accepting as a basic condition the historicity of things.<sup>2</sup>

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<sup>2</sup> A variant, but related critical approach to questions treated in this paper will be found in the conclusion of my *Pawnee Ghost Dance Hand Game* (Columbia University Contributions to Anthropology, Vol. 16, 1933), pp. 329–37.

## ON THE CONCEPT OF FUNCTION IN SOCIAL SCIENCE

By A. R. RADCLIFFE-BROWN

It has been suggested that the comments which I made on Dr Lesser's paper when it was read at Pittsburgh should be printed to accompany it. It is unfortunate that Dr Lesser, instead of specifying those whom he regards as "functionalists" and giving references to their works, offers us only an abstract description, even while he himself indicates the "lack of unity amongst functionalists." Dr Lesser assures me that he regards me as a functionalist. I have never claimed the appellation but it is true that I have made constant use of the concept of social function, in lecturing and in writing, since 1909. However, as will be seen, I do not define "function" in the same way as Dr Lesser. In the circumstances I cannot offer any real criticism of his paper. All that I can do is to offer, for any interest that it may have, a statement of the way in which I myself apply the concept of function in the study of human society.

THE concept of function applied to human societies is based on an analogy between social life and organic life. The recognition of the analogy and of some of its important implications is at least as old as Protagoras and Plato. In the nineteenth century the analogy, the concept of function, and the word itself appear frequently in social philosophy and sociology. So far as I know the first systematic formulation of the concept as applying to the strictly scientific study of society was that of Émile Durkheim in 1895.<sup>1</sup>

Durkheim's definition is that the "function" of a social institution is the correspondence between it and the needs of the social organism. This definition requires some elaboration. In the first place, to avoid possible ambiguity and in particular the possibility of a teleological interpretation, I would like to substitute for the term "needs" the term "necessary conditions of existence," or, if the term "need" is used, it is to be understood only in this sense. It may here be noted, as a point to be returned to, that any attempt to apply this concept of function in social science involves the assumption that there *are* necessary conditions of existence for human societies just as there are for animal organisms, and that they can be discovered by the proper kind of scientific enquiry.

For the further elucidation of the concept it is convenient to use the analogy between social life and organic life. Like all analogies it has to be used with care. An animal organism is an agglomeration of cells and interstitial fluids arranged in relation to one another not as an aggregate but as an integrated whole. For the bio-chemist, it is a complexly integrated system of complex molecules. The system of relations by which these

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<sup>1</sup> Règles de la Méthode Sociologique.

units are related is the organic structure. As the terms are here used the organism *is not* itself the structure; it is a collection of units (cells or molecules) arranged in a structure, i.e., in a set of relations; the organism *has* a structure. Two mature animals of the same species and sex consist of similar units combined in a similar structure. The structure is thus to be defined as a set of relations between the entities. (The structure of a cell is in the same way a set of relations between complex molecules, and the structure of an atom is a set of relations between electrons and protons.) As long as it lives the organism preserves a certain continuity of structure although it does not preserve the complete identity of its constituent parts. It loses some of its constituent molecules by respiration or excretion; it takes in others by respiration and alimentary absorption. Over a period its constituent cells do not remain the same. But the structural arrangement of the constituent units does remain similar. The process by which this structural continuity of the organism is maintained is called life. The life-process consists of the activities and interactions of the constituent units of the organism, the cells, and the organs into which the cells are united.

As the word function is here being used the life of an organism is conceived as the *functioning* of its structure. It is through and by the continuity of the functioning that the continuity of the structure is preserved. If we consider any recurrent part of the life-process, such as respiration, digestion, etc., its *function* is the part it plays in, the contribution it makes to, the life of the organism as a whole. As the terms are here being used a cell or an organ has an *activity* and that activity has a *function*. It is true that we commonly speak of the secretion of gastric fluid as a "function" of the stomach. As the words are here used we should say that this is an "activity" of the stomach, the "function" of which is to change the proteins of food into a form in which these are absorbed and distributed by the blood to the tissues.<sup>2</sup> We may note that the function of a recurrent physiological process is thus a correspondence between it and the needs (i.e., necessary conditions of existence) of the organism.

If we set out upon a systematic investigation of the nature of organisms and organic life there are three sets of problems presented to us. (There are, in addition, certain other sets of problems concerning aspects or characteristics of organic life with which we are not here concerned.) One is

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<sup>2</sup> The insistence on this precise form of terminology is only for the sake of the analogy that is to be drawn. I have no objection to the use of the term function in physiology to denote both the activity of an organ and the results of that activity in maintaining life.

that of morphology—what kinds of organic structures are there, what similarities and variations do they show, and how can they be classified? Second are the problems of physiology—how, in general, do organic structures function, what, therefore, is the nature of the life-process? Third are the problems of development—how do new types of organisms come into existence?

To turn from organic life to social life, if we examine such a community as an African or Australian tribe we can recognize the existence of a social structure. Individual human beings, the essential units in this instance, are connected by a definite set of social relations into an integrated whole. The continuity of the social structure, like that of an organic structure, is not destroyed by changes in the units. Individuals may leave the society, by death or otherwise; others may enter it. The continuity of structure is maintained by the process of social life, which consists of the activities and interactions of the individual human beings and of the organized groups into which they are united. The social life of the community is here defined as the *functioning* of the social structure. The *function* of any recurrent activity, such as the punishment of a crime, or a funeral ceremony, is the part it plays in the social life as a whole and therefore the contribution it makes to the maintenance of the structural continuity.

The concept of function as here defined thus involves the notion of a *structure* consisting of a *set of relations* amongst *unit entities*, the *continuity* of the structure being maintained by a *life-process* made up of the *activities* of the constituent units.

If, with these concepts in mind, we set out on a systematic investigation of the nature of human society and of social life, we find presented to us three sets of problems. First, the problems of social morphology—what kinds of social structures are there, what are their similarities and differences, how are they to be classified? Second, the problems of social physiology—how do social structures function? Third, the problems of development—how do new types of social structure come into existence?

Two important points where the analogy between organism and society breaks down must be noted. In an animal organism it is possible to observe the organic structure to a large extent independently of its functioning. It is therefore possible to make a morphology which is independent of physiology. But in human society the social structure as a whole can only be *observed* in its functioning. Some of the features of social structure, such as the geographical distribution of individuals and groups can be directly observed, but most of the social relations which in their totality constitute the structure, such as relations of father and son, buyer and seller, ruler



and subject, cannot be observed except in the social activities in which the relations are functioning. It follows that a social morphology cannot be established independently of a social physiology.

The second point is that an animal organism does not, in the course of its life, change its structural type. A pig does not become a hippopotamus. (The development of the animal from germination to maturity is not a change of type since the process in all its stages is typical for the species.) On the other hand a society in the course of its history can and does change its structural type without any breach of continuity.

By the definition here offered "function" is the contribution which a partial activity makes to the total activity of which it is a part. The function of a particular social usage is the contribution it makes to the total social life as the functioning of the total social system. Such a view implies that a social system (the total social structure of a society together with the totality of social usages, in which that structure appears and on which it depends for its continued existence) has a certain kind of unity, which we may speak of as a functional unity. We may define it as a condition in which all parts of the social system work together with a sufficient degree of harmony or internal consistency, i.e., without producing persistent conflicts which can neither be resolved nor regulated.<sup>3</sup>

This idea of the functional unity of a social system is, of course, a hypothesis. But it is one which, to the functionalist, it seems worth while to test by systematic examination of the facts.

There is another aspect of functional theory that should be briefly mentioned. To return to the analogy of social life and organic life, we recognize that an organism may function more or less efficiently and so we set up a special science of pathology to deal with all phenomena of disfunction. We distinguish in an organism what we call health and disease. The Greeks of the fifth century B.C. thought that one might apply the same notion to society, to the city-state, distinguishing conditions of *eunomia*, good order, social health, from *dysnomia*, disorder, social ill-health. In the nineteenth century Durkheim, in his application of the notion of function, sought to lay the basis for a scientific social pathology, based on a morphology and a physiology.<sup>4</sup> In his works, particularly those on suicide and on the division of labor, he attempted to find objective criteria by which to judge whether

<sup>3</sup> Opposition, i.e., organized and regulated antagonism, is, of course, an essential feature of every social system.

<sup>4</sup> For what is here called *dysnomia* Durkheim used the term *anomia* (*anomie* in French). This is to my mind inappropriate. Health and disease, *eunomia* and *dysnomia* are essentially relative terms.

a given society at a given time is normal or pathological, eunomic or dynomic. For example, he tried to show that the increase of the rate of suicide in many countries during part of the nineteenth century is symptomatic of a dynomic or, in his terminology, anomic, social condition. Probably there is no sociologist who would hold that Durkheim really succeeded in establishing an objective basis for a science of social pathology.<sup>5</sup>

In relation to organic structures we can find strictly objective criteria by which to distinguish disease from health, pathological from normal, for disease is that which either threatens the organism with death (the dissolution of its structure) or interferes with the activities which are characteristic of the organic type. Societies do not die in the same sense that animals die and therefore we cannot define dynomia as that which leads, if unchecked, to the death of a society. Further a society differs from an organism in that it can change its structural type, or can be absorbed as an integral part of a larger society. Therefore we cannot define dynomia as a disturbance of the usual activities of a social type (as Durkheim tried to do).

Let us return for a moment to the Greeks. They conceived the health of an organism and the eunomia of a society as being in each instance a condition of the harmonious working together of its parts.<sup>6</sup> Now this, where society is concerned, is the same thing as what was considered above as the functional unity or inner consistency of a social system, and it is suggested that for the degree of functional unity of a particular society it may be possible to establish a purely objective criterion. Admittedly this cannot be done at present; but the science of human society is as yet in its extreme infancy. So that it may be that we should say that while an organism that is attacked by a virulent disease will react thereto, and, if its reaction fails, will die, a society that is thrown into a condition of functional disunity or inconsistency (for this we now provisionally identify with dynomia) will not die, except in such comparatively rare instances as an Australian tribe overwhelmed by the white man's destructive force, but will continue to struggle toward some sort of eunomia, some kind of social health, and may, in the course of this, change its structural type. This process, it seems, the "functionalist" has ample opportunities of observing at the present day, in native peoples subjected to the domination of the civilized nations, and in those nations themselves.<sup>7</sup>

<sup>5</sup> I would personally agree in the main with the criticisms of Roger Lacombe (*La Méthode Sociologique de Durkheim*, 1926, Ch. IV) on Durkheim's general theory of social pathology, and with the criticisms of Durkheim's treatment of suicide presented by Halbwachs, *Les Causes du Suicide*.

<sup>6</sup> See, for example, the Fourth Book of Plato's *Republic*.

<sup>7</sup> To avoid misunderstanding it is perhaps necessary to observe that this distinction of

Space will not allow a discussion here of another aspect of functional theory, viz., the question whether change of social type is or is not dependent on function i.e., on the laws of social physiology. My own view is that there is such a dependence and that its nature can be studied in the development of the legal and political institutions, the economic systems and the religions of Europe through the last twenty-five centuries. For the pre-literate societies with which anthropology is concerned it is not possible to study the details of long processes of change of type. The one kind of change which the anthropologist can observe is the disintegration of social structures. Yet even here we can observe and compare spontaneous movements towards reintegration. We have, for instance, in Africa, in Oceania, and in America the appearance of new religions which can be interpreted on a functional hypothesis as attempts to relieve a condition of social dysnomia produced by the rapid modification of the social life through contact with white civilization.

The concept of function as defined above constitutes a "working hypothesis" by which a number of problems are formulated for investigation. No scientific enquiry is possible without some such formulation of working hypotheses. Two remarks are necessary here. One is that the hypothesis does not require the dogmatic assertion that everything in the life of every community has a function. It only requires the assumption that it *may* have one, and that we are justified in seeking to discover it. The second is that what appears to be the same social usage in two societies may have different functions in the two. Thus the practice of celibacy in the Roman Catholic Church of to-day has very different functions from those of celibacy in the early Christian church. In other words, in order to define a social usage, and therefore in order to make valid comparisons between the usages of different peoples or periods it is necessary to consider not merely the form of the usage but also its function. On this basis, for example, belief in a Supreme Being in a simple society is something different from such a belief in a modern civilized community.

The acceptance of the functional hypothesis or point of view outlined above results in the recognition of a vast number of problems for the solu-

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economic and dysnomic social conditions does not give us any evaluation of these societies as "good" or "bad." A savage tribe practicing polygamy, cannibalism, and sorcery can possibly show a higher degree of functional unity or consistency than the United States of 1935. This objective judgment, for such it must be if it is to be scientific, is something very different from any judgment as to which of the two social systems is the better, the more to be desired or approved.

tion of which there are required wide comparative studies of societies of many diverse types and also intensive studies of as many single societies as possible. In field studies of the simpler peoples it leads, first of all, to a direct study of the social life of the community as the functioning of a social structure, and of this there are several examples in recent literature. Since the function of a social activity is to be found by examining its effects upon individuals, these are studied, either in the average individual or in both average and exceptional individuals. Further the hypothesis leads to attempts to investigate directly the functional consistency or unity of a social system and to determine as far as possible in each instance the nature of that unity. Such field studies will obviously be different in many ways from studies carried out from other points of view, e.g., the ethnological point of view that lays emphasis on diffusion. We do not have to say that one point of view is better than another, but only that they are different, and any particular piece of work should be judged in reference to what it aims to do.

If the view here outlined is taken as one form of "functionalism," a few remarks on Dr Lesser's paper become permissible. He makes reference to a difference of "content" in functional and non-functional anthropology. From the point of view here presented the "content" or subject-matter of social anthropology is the whole social life of a people in all its aspects. For convenience of handling it is often necessary to devote special attention to some particular part or aspect of the social life, but if functionalism means any thing at all it does mean the attempt to see the social life of a people as a whole, as a functional unity.

Dr Lesser speaks of the functionalist as stressing "the psychological aspects of culture." I presume that he here refers to the functionalist's recognition that the usages of a society work or "function" only through their effects in the life, i.e., in the thoughts, sentiments and actions of individuals.

The "functionalist" point of view here presented does therefore imply that we have to investigate as thoroughly as possible all aspects of social life, considering them in relation to one another, and that an essential part of the task is the investigation of the individual and of the way in which he is moulded by or adjusted to the social life.

Turning from content to method Dr Lesser seems to find some conflict between the functional point of view and the historical. This is reminiscent of the attempts formerly made to see a conflict between sociology and history. There need be no conflict, but there is a difference.

There is not, and cannot be, any conflict between the functional hypothesis and the view that any culture, any social system, is the end-result of a unique series of historical accidents. The process of development of the

race-horse from its five-toed ancestor was a unique series of historical accidents. This does not conflict with the view of the physiologist that the horse of to-day and all the antecedent forms conform or conformed to physiological laws, i.e., to the necessary conditions of organic existence. Palaeontology and physiology are not in conflict. One "explanation" of the race-horse is to be found in its history—how it came to be just what it is and where it is. Another and entirely independent "explanation" is to show how the horse is a special exemplification of physiological laws. Similarly one "explanation" of a social system will be its history, where we know it—the detailed account of how it came to be what it is and where it is. Another "explanation" of the same system is obtained by showing (as the functionalist attempts to do) that it is a special exemplification of laws of social physiology or social functioning. The two kinds of explanation do not conflict, but supplement one another.<sup>8</sup>

The functional hypothesis is in conflict with two views that are held by some ethnologists, and it is probably these, held as they often are without precise formulation, that are the cause of the antagonism to that approach. One is the "shreds and patches" theory of culture, the designation being taken from a phrase of Professor Lowie<sup>9</sup> when he speaks of "that planless hodge-podge, that thing of shreds and patches called civilization." The concentration of attention on what is called the diffusion of culture-traits tends to produce a conception of culture as a collection of disparate entities (the so-called traits) brought together by pure historical accident and having only accidental relations to one another. The conception is rarely formulated and maintained with any precision, but as a half unconscious point of view it does seem to control the thinking of many ethnologists. It is, of

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<sup>8</sup> I see no reason at all why the two kinds of study—the historical and the functional—should not be carried on side by side in perfect harmony. In fact, for fourteen years I have been teaching both—the historical and geographical study of peoples under the name of ethnology in close association with archaeology, and the functional study of social systems under the name of social anthropology. I do think that there are many disadvantages in mixing the two subjects together and confusing them. See *The Methods of Ethnology and Social Anthropology* (*South African Journal of Science*, 1923, pp. 124–47).

<sup>9</sup> *Primitive Society*, 441. A concise statement of this point of view is the following passage from Dr Ruth Benedict's *The Concept of the Guardian Spirit in North America* (*Memoirs, American Anthropological Association*, 29, 1923), page 84: "It is, so far as we can see, an ultimate fact of human nature that man builds up his culture out of disparate elements, combining and recombining them; and until we have abandoned the superstition that the result is an organism functionally interrelated, we shall be unable to see our cultural life objectively, or to control its manifestations." I think that probably neither Professor Lowie nor Dr Benedict would, at the present time, maintain this view of the nature of culture.

course, in direct conflict with the hypothesis of the functional unity of social systems.

The second view which is in direct conflict with the functional hypothesis is the view that there are no discoverable significant sociological laws such as the functionalist is seeking. I know that some two or three ethnologists say that they hold this view, but I have found it impossible to know what they mean, or on what sort of evidence (rational or empirical) they would base their contention. Generalizations about any sort of subject matter are of two kinds: the generalizations of common opinion, and generalizations that have been verified or demonstrated by a systematic examination of evidence afforded by precise observations systematically made. Generalizations of the latter kind are called scientific laws. Those who hold that there are no laws of human society cannot hold that there are no generalizations about human society because they themselves hold such generalizations and even make new ones of their own. They must therefore hold that in the field of social phenomena, in contradistinction to physical and biological phenomena, any attempt at the systematic testing of existing generalizations or towards the discovery and verification of new ones, is, for some unexplained reason, futile, or, as Dr Radin puts it, "crying for the moon." Argument against such a contention is unprofitable or indeed impossible.

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## PLAINS GHOST DANCE AND GREAT BASIN MUSIC

By GEORGE HERZOG

AN inquiry into the relation and stability of musical form and function finds in the Ghost Dance songs of the Plains Indians an excellent example for study. In them we have music associated with a movement definitely known to have arisen in a different setting, with the Paiute of the Great Basin. Examination of the musical material brings forth specific answers to some of the basic questions which prompt such an inquiry.

Practically all songs found associated with the Plains Ghost Dance are so closely related to each other that they must be conceived as representing a distinct type, forming an integrated "style" of their own. This style is foreign to the Plains; its patterns are different from those prevalent in Plains music. The style can be traced to the Great Basin: musical evidence reflects the diffusion of the Ghost Dance from that region to and through the Plains. In the Basin the style is not restricted to Ghost Dance songs; it is represented in other song categories so generously that its pattern may be regarded as the strongest and most characteristic element of some Great Basin musical styles. While this pattern penetrated into Plains music as the "Ghost Dance style," it has also found a place in the music of other tribes, unaffected by the Ghost Dance; there naturally it is not associated with the same function.

In this paper the foundation for these statements is offered as briefly and as much freed from technical detail as possible. The presentation of the full evidence with more abundant musical illustration and complete analysis would overstep the limits of this article and must be left to another occasion.

Comparison of all the Plains Ghost Dance melodies available to me (altogether thirty-eight) revealed in most of them a striking similarity amounting to a uniformity of style. The melodic range is usually narrow, essentially a fifth. As a rule there is no accompaniment.<sup>1</sup> Many of the phrases end on the tonic. They fall into sections so symmetrical as to be startling in primitive material. This symmetry is achieved by the most essential feature of the style, a simple structural device: *every* phrase is rendered twice. The emphasis on "every" is important, since doubling one or two phrases is a fairly commonplace feature of many styles, in Indian and other music. Repetition, in one guise or another, is one of the most significant principles of primitive musical form. But this particular repetitive device

<sup>1</sup> See James Mooney, *The Ghost-Dance Religion and the Sioux Outbreak of 1890* (Fourteenth Annual Report, Bureau of American Ethnology, Pt. 2, 1896), p. 921

is quite unusual, and is unique in Plains music. At the same time, just because it is so simple and unequivocal, it constitutes a trait which can be traced and treated with ease. Its various forms will be referred to as "paired patterns." Or, since the melody often progresses through phrases of changing melodic content, each rendered twice, the expression "paired progression" will be applied.

Ghost Dance songs are not easy to record. They are either forgotten or are still cherished with especial reverence. Many collectors made no particular effort to get them.<sup>2</sup> The short description above is based chiefly on second hand material. Many of the melodies in the literature were not taken on the phonograph, but merely transcribed by ear. While such material must always be treated with caution, it is reliable enough for present purposes. The salient trait, paired pattern, is so simple that it could not easily be lost through imperfections of notation. However, faulty transcription would be more likely to obscure its presence than to suggest it where it was absent. It was possible to make a check on part of Mooney's melodies, as some of them were recorded on the phonograph.<sup>3</sup> Comparison of No. 1 of the music examples with his Arapaho song No. 67 shows that while the transcriptions in his volume are not in all respects satisfactory, they represent structure with sufficient accuracy. The first version is the transcription made by me of a record in the Bureau of American Ethnology, taken during or perhaps after Mooney's study. The songs in Natalie Curtis' collection,<sup>4</sup> on the other hand, are to be trusted implicitly, so excellent is the workmanship of the volume.

The analysis is condensed below into tabular form. Those traits were selected for the table in which the style contrasts significantly with its setting—Plains music—and which can be treated without cumbersome musical detail. They have been chosen to satisfy not only the requirements of convenience, but also those of fair representation. Such a selection can in no case be avoided in describing a musical style, and the terms in which one style may best be discussed do not necessarily apply to another style. In the present case some features like manner of singing, to which the nature of the material does not give clear clues, have had to be left out of consideration.

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<sup>2</sup> For example, in the collection of phonograph records in the American Museum of Natural History in New York, which contains over four hundred records of Plains music, there seems to be only one Plains Ghost Dance song; No. 8 among the music examples appended to this paper.

<sup>3</sup> See Mooney, p. 655.

<sup>4</sup> *The Indian's Book* (New York and London, 2nd ed., 1923).



The number under "range" gives the interval within which the song is confined, 5 standing for the fifth, etc. The letters under "Structure" stand for phrases or still smaller units, one letter for each. Recurrent letters indicate the recurrence of phrases. If two phrases are almost, but not absolutely, identical they are marked with the same letter but with different index numerals. Purely formal elements of meager melodic or rhythmic content are denoted by *x*: introductory, connective, or final phrases ("codas"). If the song splits into two sections, a break between the letters indicates the division.

"Phrase" is a unit not easily defined, just as "phrase" or "sentence" are troublesome units in linguistics. The musical phrase does not invariably coincide with a text phrase. Its limits can be established by various means, taking into consideration text, rests, accentuation, divisions of the melodic or rhythmic movement, etc. The musician distinguishes between the longer unit of a phrase and the shorter unit of a motif, but this distinction is not germane to our material. In many Ghost Dance songs the structural element rendered twice may be quite long or quite short; in fact, the unit of structure may best be defined in this style as that element which is rendered twice, irrespective of its length. This in turn strengthens the contention that the essential earmark of the style is the tendency to double. While to most investigators the presence of phrase units in primitive music is no news, some do not mark them in notation, or do not consider their relation to each other in analyzing structure. Consequently much material, especially collections of Plains music, had to be reanalyzed in order to determine the structural types and their distribution.

Under "finals" are indicated the tones which stand at the end of each phrase. Tabulation of such tones as a means of throwing light on the tonal structure of melodies has long been used in the study of European folk song.<sup>5</sup> With certain reservations, it can be used also with primitive songs, in addition to other methods. The tones are indicated by numbers: roman numerals stand for tones below the tonic, arabic numerals for tones above it. The tonic being taken as 1, a second above it is 2, a second below it is VII, etc. The intervals ought to be further specified with sharps and flats, but for the present purposes the procedure may be simplified by dispensing with these signs. Even a much more elaborate table would be a rather rough representation of musical forms. The inadequacies of the present table.

<sup>5</sup> See Ilmari Krohn, *Welche ist die beste Methode, um Volks- und volksmassige Lieder nach ihrer melodischen (nicht textlichen) Beschaffenheit lexikalisch zu ordnen?* (Sammelbande der Internationalen Musikgesellschaft, Vol. 4, No. 4, pp. 643-60, 1903), and Béla Bartók, *Hungarian Folk Music* (Oxford, 1931), pp. 6-8

the discussion of which would again lead too far afield, do not, however, bolster up the evidence; if anything, they diminish it.

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
<b>Arapaho</b>			
M. p. 990, <sup>1</sup> No. 45	5	aabb	1,1,1,1
C. p. 208 <sup>7</sup>	6	aabb	1,1,1,1
M. p. 965	5	aabbcc	1,1,1,1,1,1
M. p. 977	5	aabbcc	1,1,1,1,1,1
M. p. 990, No. 44	5	aabbcc	1,1,1,1,1,1
M. p. 996	5	aabbcc	1,1,1,1,1,1
M. p. 1011	6	aabbcc	1,1,1,1,1,1
M. p. 1066	6	aabbccdd	5,5,1,1,1,1,1
(cf. music example 1)			
M. p. 958	6	aab <sup>1</sup> b <sup>1</sup> b <sup>2</sup> b <sup>2</sup> b <sup>3</sup> b <sup>3</sup>	VII, VII, 1,1,1,1,1,1
F. p. 98-99 <sup>8</sup>	5	aabb ccbb	3,3,1,1 3,3,1,1
S. <sup>9</sup>	8	aabbcd	4,4,4,4,2,1
(C. p. 269-10 <sup>10</sup> )	8	a <sup>1</sup> a <sup>1</sup> bcdef ga <sup>2</sup> cdef	4,4,6,1,3,3,1 4,4,1,3,3,1)
<b>Pawnee</b>			
C. p. 143	8	aabb	1,1,1,1
D. 50 <sup>11</sup>	7	aabb	4,4,1,1
D. 52	7	aabb	4,4,1,1
C. p. 140	5	a <sup>1</sup> a <sup>2</sup> bb	2,2,1,1
(music example 2)			
D. 55	5	aabbcc	3,3,1,1,1,1
D. 58	5	aabbcc	4,4,1,1,1,1
D. 54	5	aabbcc	3,3,3,3,1,1
C. p. 139	8	aabbcc	1,1,III,III,1,1
D. 51	5	a <sup>1</sup> a <sup>2</sup> bbcc	3,3,1,1,1,1
D. 56	6	a <sup>1</sup> a <sup>2</sup> bbcc	5,5,1,1,1,1
D. 57 <sup>12</sup>	5	a <sup>1</sup> a <sup>2</sup> bbcc a <sup>1</sup> a <sup>4</sup> ddee	3,3,1,1,1,1 3,3,1,1,1,1
C. p. 141-42	5	a <sup>1</sup> a <sup>2</sup> b <sup>1</sup> b <sup>1</sup> c <sup>1</sup> c <sup>1</sup> b <sup>2</sup> b <sup>2</sup> c <sup>2</sup> c <sup>2</sup>	4,4,1,1,1,1 1,1,1,1
D. 53 <sup>13</sup>	5	a <sup>1</sup> a <sup>2</sup> x a <sup>2</sup> a <sup>2</sup> x	1,1,1 1,1,1

<sup>6</sup> Reference to the page in Mooney on which the song is found.

<sup>7</sup> Page-reference to Curtis' collection.

<sup>8</sup> Alice C. Fletcher, *Indian Story and Song from North America* (Boston, 1907).

<sup>9</sup> A song in *The Southern Workman*, Vol. 36, p. 111, reproduced there without any comment, taken down probably from the singing of a Hampton student, by N. Curtis. I am obliged to Dr. A. H. Gayton for this reference.

<sup>10</sup> This is a Crow Dance song, cf. Curtis, p. 201.

<sup>11</sup> References to song numbers in Frances Densmore, *Pawnee Music* (Bulletin, Bureau of American Ethnology 93, 1929), pp. 78-86.

<sup>12</sup> The song ends, in the last rendition, without the last two (e) phrases.

<sup>13</sup> This melody, together with Nos. 54-56, was sung to dancing incidental in pauses of the hand game.

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
Caddo			
M. p. 1100	6	aabbcc	2,2,1,1,1,1
M. p. 1096	6	aab <sup>1</sup> b <sup>1</sup> b <sup>2</sup> b <sup>2</sup> b <sup>3</sup> b <sup>3</sup>	1,1,1,1,1,1,1
M. p. 1101-02	6	aabbcc d <sup>1</sup> d <sup>2</sup> eee	3, <sup>2</sup> 1,1,1,1,1 1,1,1,1,1
Kiowa			
M. p. 1088	5	aabbcc	4,4,1,1,1,1
M. p. 1086	7	aabbcc <sup>1</sup> c <sup>1</sup> ddc <sup>2</sup> c <sup>2</sup>	3,3,3,3,3,3,1,1,1,1
Comanche			
M. p. 1046	12	(x)xaabbcc	(8,)8,7,7,4,4,1,1
Teton Dakota			
C. p. 67	5	aabb	1,1,1,1
C. p. 66	5	a <sup>1</sup> a <sup>2</sup> bccdd	2,2,1,1,2,2,1,1
(music example 3)			
C. p. 63-65	5	a <sup>1</sup> a <sup>2</sup> bccdd <sup>1</sup> d <sup>2</sup>	3,4,1,1,1,1,1
Mo. p. 168-70 <sup>14</sup>	5	abccdd	4,1,3,3,1,1
Mo. p. 168-70	7	abcdee	5,3,2,1,1,1
C. <sup>15</sup>	10	a <sup>1</sup> bcc a <sup>1</sup> bcc a <sup>2</sup> de a <sup>2</sup> de	8,5,4,4 8,5,4,4 8,5,1 8,5,1
Yanktonai Dakota			
H.PR 93 <sup>16</sup>	8	abcddee	5,3,1,3,3,1,1
(music example 8)			

The frequent occurrence of paired patterns can be seen at a glance. Simple forms with gradually changing content ("paired progression") are:

<i>aabb</i>	(6 examples)
<i>aabbcc</i>	(11)
<i>aabbccdd</i>	(1)
<i>aab<sup>1</sup>b<sup>1</sup>b<sup>2</sup>b<sup>2</sup>b<sup>3</sup>b<sup>3</sup></i>	(2)

In closely related forms one of the phrases is modified upon its second appearance:

<i>a<sup>1</sup>a<sup>2</sup>bb</i>	(1)
<i>a<sup>1</sup>a<sup>2</sup>bbcc</i>	(2)

<sup>14</sup> Warren K. Moorehead, *The Sioux Messiah* (Archaeologist, Vol. 2, No. 5, pp. 146-49, No. 6: 168-70, 1894)

<sup>15</sup> Pp. 141-42 of L. W. Colby, *Wanagi olowan kin The Ghost Songs of the Dakotas* (Proceedings and Collections, Nebraska State Historical Society, 2nd ser., Vol. 1, pp. 131-50, 1894-95).

<sup>16</sup> Phonograph record 93 of my collection of Yanktonai Dakota songs in the American Museum of Natural History.

One song begins with an introductory phrase of slight melodic importance:<sup>17</sup>

$$(x)xaabbcc \quad (1)$$

Once the progression breaks toward its close and reverts to a previous phrase:

$$aabbcc^1c^1ddc^2c^2 \quad (1)$$

In a few cases the song splits into two sections or "verses" which stand in a definite relation to each other. The second section may substitute new phrases for some of the first:

$$aabb \ cccb \quad (1)$$

$$a^1a^2bbcc \ a^1a^2ddee \quad (1)$$

Or the second section may reproduce the first in a somewhat shortened form, at the same time either modifying phrases or substituting new phrases for old ones:

$$a^1a^2b^1b^1cc \ b^2b^2c^2c^2 \quad (1)$$

$$aabbcc \ d^1d^2eee \quad (1)$$

In the great majority of our cases (thirty out of thirty-eight) the paired pattern is clear and practically unbroken. (The third repetition of the last phrase in the Caddo song M. pp. 1101-2 is a rather minor modification.) Of the eight songs that do not conform to the pattern, six have at least a partial doubling of phrases (Pawnee D. 53., Arapaho S., Teton C. pp. 63-65, Mo. pp. 168-70, Mo. pp. 168-70, and Yanktonai H. PR 93). Moreover, one of them is a variant of a Teton song which is regular: the text of the Yanktonai song is identical with that of the Teton song C., p. 66, and the melody is similar. (Compare No. 8 of the music examples with No. 3.) The text of Moorehead's first Teton song is almost identical with these two, but the melody is quite different. It is justifiable to assume that the Yanktonai melody is a hybrid form. Of the two remaining examples, the Arapaho song C. pp. 209-10 is not a Ghost Dance song proper but a melody from the "Crow Dance," associated with the Ghost Dance.<sup>18</sup> While its scheme in the table suggests no trace of paired patterning, the structure could be re-analyzed without doing it grave injustice, as  $a^1a^1a^2b^1b^2cx \ xa^2b^1b^2cx$ , which shows a more intimate interrelation between the units. Colby's Teton song, finally, shows little of the tendency for doubling, although at least the two verses are repeated. This exception to the musical pattern may be due to

<sup>17</sup> This is the Comanche song, Mooney, p. 1046. On the phonograph record of this song the introduction is repeated in one of the three renditions.

<sup>18</sup> This is a modification of the "Omaha Dance" according to Mooney (pp. 901, 921-22) and not an entirely new growth, which may well explain the "irregularity" of this song.

the poetic form: the text represents a dialogue between the "Great Spirit" and the Ghost dancer, alternately singing two phrases each.<sup>19</sup>

Narrow range and the cumulative use of the tonic for phrase-endings (numeral one) are also fairly frequent in the table. There is a relation, although somewhat tenuous, between these two traits; a relation naturally conditioned also by the number of tones within the range. The gradually descending melodies do not have much freedom of movement when confined to a narrow space, nor much choice where to repose on the way. But to stop consistently on the same level is not obligatory for them and so the two traits may be considered independent, or only indirectly interrelated.

In a respectable majority of songs (twenty-six) the paired pattern is at the same time "paired progression." The structural principles which in the others supersede or obscure this pattern, or are superimposed upon it, are very strong in the music of the Plains.

Plains songs in general exhibit features different from those of Ghost Dance songs. My analysis of material published so far indicates that the most frequent patterns on the Plains are based on a few simple principles such as:

- a) Progression through single phrases, as *ab*, *abc*, *abcd*, etc.
- b) Modified repetition of a section or verse. The two sections then make up the song, which is usually repeated a number of times. The modification may consist of altering one or two phrases of the first section, *a<sup>1</sup>bc a<sup>2</sup>bc* and the like, or of substituting an altogether new phrase: *abc dbc*, etc.
- c) Curtailment of a section by leaving out one or two phrases, very often initially: *abc bc*, etc.
- d) Reversion to a previous phrase: *aba*, *abca*, *abcb*, etc.
- e) In a special type the song or its sections are closed by one or two phrases which have occurred already, but which are now rendered an octave below. Common forms could be noted as *aba<sub>s</sub>*, *abca<sub>s</sub>*, *abc a<sub>s</sub>b<sub>s</sub>*, etc.

It should be kept in mind that in many songs more than one of these principles apply. A structure like *abc a<sub>s</sub>b<sub>s</sub>* for instance, at the same time implies "progression," "modified repetition," and "curtailment," in this case at the end. Thus the frequency of these primary types can seldom be precisely determined by simple counting. For present purposes, however, it seemed more fruitful to indicate the general prevalence of these types, rather than to give a more meticulous but overcomplicated count, even though the simpler method understates the case. In the following tabulation of most of the larger collections of Plains music only the simple forms of the

<sup>19</sup> See Colby, *op cit*, pp. 141-42

types described have been counted and each song is indicated only once, except for the last column. The tabulation ought not to be taken, then, to express much more than the general prevalence of these types on the Plains. Since it is only a general approximation, it would be injudicious to transform these figures into percentages. The heavy proportion of the *aba*<sub>3</sub> type in Omaha and Pawnee material is significant, however.

	<i>Total</i>	<i>Progressive</i>	<i>Initial curtailment</i>	<i>aba</i> <sub>3</sub> , etc.
Mandan and Hidatsa <sup>20</sup>	111	51	15	13
Teton <sup>21</sup>	240	40	62	32
Pawnee <sup>22</sup>	102	12	25	34
Omaha <sup>23</sup>	187	49	30	50

These patterns are not limited to the Plains. Among some tribes of the Northeast, like the Chippewa and the Menominee (whose music must be classed on the whole with Plains music), they are quite as frequent:

Menominee <sup>24</sup>	140	38	8	36
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In the music of other regions in North America these patterns also occur, but except for the very simple and fairly general progressive pattern, they are no longer representative. As random examples:

Creek <sup>25</sup>	119	23	3	—
Pueblo <sup>26</sup>	157	13	4	2

The foregoing indicates that the prevailing structural patterns on the Plains are different from that of the Ghost Dance. As for range and phrase-endings, it may be said that the average Plains song has a range of an octave and above, and that the successive finals tend to form a series descending to the tonic, rather than each one striking it.

<sup>20</sup> Frances Densmore, *Mandan and Hidatsa Music* (Bulletin, Bureau of American Ethnology, 80, 1923).

<sup>21</sup> *Idem*, *Teton-Sioux Music* (Bulletin, Bureau of American Ethnology, 61, 1918).

<sup>22</sup> *Idem*, *Pawnee Music*, and N. Curtis, *op. cit.*, pp. 117-43. The 94 melodies of the Hako (Twenty-second Annual Report, Bureau of American Ethnology, pt. 2, 1904) are omitted here to avoid heavy weighting by so many songs from a single ritual.

<sup>23</sup> A. C. Fletcher, *A Study of Omaha Indian Music* (Archaeological and Ethnological Papers of the Peabody Museum, Harvard University, Vol. 1, No. 5, 1893) and A. C. Fletcher and F. La Flesche, *The Omaha Tribe* (Twenty-seventh Annual Report, Bureau of American Ethnology, 1911).

<sup>24</sup> F. Densmore, *Menominee Music* (Bulletin, Bureau of American Ethnology, 102, 1932).

<sup>25</sup> Frank G. Speck, *Ceremonial Songs of the Creek and Yuchi Indians* (University of Pennsylvania Museum Anthropological Publications, Vol. 1, No. 2, 1911).

<sup>26</sup> Melodies of a collection recorded by me, as yet unpublished.

The question then arises, to what extent are paired patterns found on the Plains at all, outside of Ghost Dance songs proper, and to what extent do they occur in other regions of North America, as far as extant material can show? I have traced these patterns in North American Indian music, analyzing all the larger and many smaller collections of melodies published so far, as well as much unpublished material recorded by myself. The following table gives the results for the Plains:

	<i>Total</i>	<i>Paired</i>	<i>Related or modified</i>	<i>Doubtful</i>
Mandan and Hidatsa <sup>27</sup>	111	—	1	3
Teton <sup>28</sup>	240	—	4	3
Pawnee <sup>29</sup>	90	6	4	1
Omaha <sup>30</sup>	187	1	1	1
Osage <sup>31</sup>	283	1	2	1

The six clear Pawnee cases (and one under "related") are hand game melodies, out of eleven such melodies recorded. Most of them were apparently sung to the game itself, while some of the Ghost Dance songs were sung in the intervals of the game for dancing. The paired pattern as such is not common in songs of hiding games on the Plains, except in a delimited region in the south.<sup>32</sup> All the hand game songs known to me from tribes of this region are incorporated in the following table:

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
Pawnee			
D. 39	3	aabb	1,1,1,1
D. 48	8	aabb	5,5,1,1
D. 45	6	a <sup>1</sup> a <sup>2</sup> bb	1,1,1,1
(music example 4)			

<sup>27</sup> The melodies are, Densmore Nos. 36, 34, 41, 73.

<sup>28</sup> Densmore Nos. 45, 145, 198, 211; 9, 102, 160. No. 45, interestingly enough, is a song received in a vision which made the visionary proof against arrows and bullets (see Densmore, *Teton Sioux Music*, pp. 175-76).

<sup>29</sup> F. Densmore, *op. cit.*, and N. Curtis, exclusive of the Ghost Dance songs. Densmore No. 44 is related and 19, 20, 63 may be considered so; No. 69 is a doubtful case. Of ninety-four melodies of the Hako, one has a regular paired pattern (p. 212) and four may be considered related.

<sup>30</sup> Omaha Indian Music, No. 27, *The Omaha Tribe*, pp. 239, 257.

<sup>31</sup> From the studies of Osage Rituals by F. La Flesche, in the Thirty-sixth, Thirty-ninth and Forty-fifth Annual Reports, Bureau of American Ethnology. The respective melodies are in the Thirty-ninth Annual Report, pp. 102, 231, 233, 315.

<sup>32</sup> Among the eighteen songs indicated in the previous table (exclusive of the Pawnee), two are hiding game songs, one from the Teton out of nine Teton hand game songs recorded, and one from the Omaha out of four that were recorded.

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
D. 42	4	aabbcc	1,1,1,1,1,1
D. 47	8	aabbcc	V,V,V,V,1,1
D. 40	5	aabbcc bbcc	5,5,1,1,1,1 1,1,1,1
D. 44	6	aabcc	4,4,3,1,1
D. 43	8	abc	2,1,1
D. 46	8	abcd	1,1,V,1
D. 41	7	abcd cdd	5,4,1,2 1,1,1
Arapaho			
C. p. 213-14	6	aabbcc aabb	3,3,1,1,V,V 3,3,1,1
Cheyenne			
C. p. 183	8	aabbcc	4,4,1,1,1,1
C. p. 184	9	aabbaa	1,1,5,5,1,1
C. p. 182	11	aabbc	8,8,4,4,1
C. p. 186 <sup>3</sup>	12	aabccx	8,8,4,1,1,1
C. p. 185	8	$a\frac{b}{2}bb$	5,1,1,1
C. p. 187	8	aabcdx bbcd	3,3,1,1,1,1 1,1,1,1

In the light of the connection between the dance and the game among the Pawnee, recently illuminated in great detail by Dr Lesser,<sup>34</sup> the presence of the Ghost Dance pattern in hand game songs of the Pawnee and their neighbors is not surprising. According to him, there are special songs sung before the game proper begins, songs sung in the intervals between the games, and songs sung to the actual playing of the game (Lesser, p. 134). Both old hand game songs and Ghost Dance hand game songs are used in the modified forms of the hand game (Lesser, pp. 210, 235, 310). Furthermore, "Many of the play songs used at the Ghost Dance hand game ceremonies for the play of the game are songs of a revival nature learned in Ghost Dance visions" (Lesser, p. 321). Dr Lesser adds, in a letter, "The Pawnee say that the first songs used for Ghost dancing, both in ceremonies and in hand games, were borrowed, and that then they made up songs themselves." The material referred to in the table is too slight to warrant tracing these distinctions. It is sufficient to note that at least two thirds of these songs classed in the literature as "hand game songs" are unmistakably in the Ghost Dance pattern. Some of them may be new forms, others remodeled old hand game songs.

<sup>33</sup> The phrases marked *c* are actually *a*, phrases.

<sup>34</sup> Alexander Lesser, *The Pawnee Ghost Dance Hand Game* (Columbia University Contributions to Anthropology, Vol. 16, 1933). See also Lesser, *Cultural Significance of the Ghost Dance* (American Anthropologist, Vol. 35, pp. 108-15, 1933).



An analysis of representative collections from other regions in North America gave the following results:

	<i>Total</i>	<i>Paired</i>	<i>Related or modified</i>	<i>Doubtful</i>
Creek <sup>35</sup>	110	3	4	3
Pueblo	157	4	6	—
Papago <sup>36</sup>	169	5	9	—
Navaho "War Dance" <sup>37</sup>	40	14	4	7
Northern Ute <sup>38</sup>	114	9	6	4
Northern Ute Bear Dance <sup>39</sup>	17	8	3	—

The number of paired forms is negligible (perhaps excepting the Papago) until we come to the Navaho and the Northern Ute. In a great mass of Navaho ritualistic music now being analyzed by me, practically no paired patterns as such have been found so far. However, in dancing songs, mostly exoteric, associated with the Enemy Chant or "War Dance," the pattern is apparently quite common. The Northern Ute data probably have some bearing on this frequency. Of the nine clear cut Ute examples given above, eight belong to the Bear Dance; of the seventeen songs recorded from this dance altogether, only six are clearly not to be referred to this pattern at all. The analysis of the Bear Dance songs follows:

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
D. 2	4	aabb	1,1,1,1
D. 13	8	aabb	1,1,1,1
D. 17	6	aabb	1,1,1,1
D. 1	6	aab <sup>40</sup> b <sup>2</sup>	VII,VII,1,1
(music example 6)			
D. 6	5	aabb(x)	1,1,1,1
D. 7 <sup>41</sup>	6	aab <sup>41</sup> b <sup>2</sup> x	1,1,1,1,1
D. 16 <sup>42</sup>	6	a <sup>1</sup> a <sup>2</sup> bb a <sup>1</sup> a <sup>1</sup> bb a <sup>1</sup> a <sup>1</sup> bb abb	1,1,1,1 1,1,1,1 1,1,1,1 1,1,1
D. 9	8	aabb <sup>42</sup> c <sup>2</sup>	VI,VI,1,1,1,1
D. 3	11	a <sup>1</sup> a <sup>2</sup> bb <sup>42</sup> c <sup>1</sup> c <sup>1</sup> c <sup>2</sup> dd	1,1,1,1,1,1,1,1,1,1

<sup>35</sup> Speck Nos. 16B, 19B, Medicine song 2, 10H, 18B, 18C, 20B, 12A, Medicine song 7, 17A

<sup>36</sup> F. Densmore, Papago Music (Bulletin, Bureau of American Ethnology, 90, 1920), Nos. 21, 24, 26, 34, 126, 8, 12, 19, 27, 29, 47, 127, 128, 129

<sup>37</sup> From unpublished material recorded by me

<sup>38</sup> F. Densmore, Northern Ute Music (Bulletin, Bureau of American Ethnology, 75, 1922).

<sup>39</sup> Densmore, *op. cit.*, pp. 58-72.

<sup>40</sup> The x phrase is a connective, omitted the last time

<sup>41</sup> The paired units are exceedingly small. The melody is repeated, beginning with the ninth unit

<i>Song</i>	<i>Range</i>	<i>Structure</i>	<i>Finals</i>
D. 8	8	aabbc	1,1,1,1,1
D. 14	9	(x)aabx	(9)1,1,1,1,1
D. 15	8	abb	1,1,1
D. 10	9	xab <sup>1</sup> b <sup>2</sup>	7,1,1,1
D. 12	9	abcd <sup>1</sup> d <sup>2</sup>	1,VI,V,1,1
D. 5	8	abc <sup>1</sup> dc <sup>2</sup>	5,4,1 5,1
D. 11	9	ab <sup>1</sup> c b <sup>2</sup> b <sup>3</sup> d	1,V,IV 1,1,1
D. 4 <sup>42</sup>	9	abcdcddefghh	8,1,3,1,3,1,2,1,1,1

The comparative frequency of paired patterns in Northern Ute music, in the Bear Dance, supports the information that features of Ute dancing have been incorporated into the Navaho "War Dance."<sup>43</sup> With regard to narrow range and the frequency of phrase-endings on the tonic it should be kept in mind, however, that a scattering of these traits is found in the Northern Ute style as a whole.

Paired patterns are also not exceptional in the music of some of the Yuman tribes:

	<i>Total</i>	<i>Paired</i>	<i>Related or modified</i>	<i>Doubtful</i>
Yuma <sup>44</sup>	82	5	10	6
Southern Diegueño <sup>45</sup>	27	4	—	2
(Mohave) <sup>46</sup>	29	—	1	2)
(Cocopah) <sup>47</sup>	30	—	—	1)

Among these groups, a number of songs show a paired pattern in company with the "rise" characteristic for the Yumans (modification or imitation of the fundamental musical phrase on a higher level somewhere in the body of the song<sup>48</sup>); indicating the interpenetration of two distinct styles or at least, stylistic principles.

Published material from other parts of California is at the present time too scanty for our purposes.

Finally, from the Northern Paiute only the few music examples in

<sup>42</sup> The melody is repeated, beginning with the phrase *c*

<sup>43</sup> I am indebted to Dr W. W. Hill for this information

<sup>44</sup> F. Densmore, Yuman and Yaqui Music (Bulletin, Bureau of American Ethnology, 110, 1932) see especially Nos. 7, 11, 12, 13, and 20

<sup>45</sup> From my collection, partly published in The Yuman Musical Style (Journal of American Folk-Lore, Vol. 41, pp. 183-232, 1928). See especially Nos. 24, 26, and 33 in that article

<sup>46</sup> Four melodies in Densmore, *op. cit.*, the rest in my collection, partly published in The Yuman Musical Style

<sup>47</sup> Densmore, *op. cit.*, pp. 85-98, 168-82, 185-92

<sup>48</sup> See The Yuman Musical Style, pp. 193, 196

Steward's study of the Owens Valley Paiute are available at present.<sup>49</sup> The paired pattern is in evidence, but presumably in a larger collection the proportion would be larger. Of eighteen melodies, one is plainly in this style (No. 12); in two others both paired pattern and Yuman "rise" occur (Nos. 4, 13). The rest are about equally divided between Yuman and other forms. In a larger collection of Southern Paiute songs which I had occasion to analyze, of about 200 melodies some seventy have clearly paired patterns (mostly paired progression), partly with slight modifications, besides a number of possibly related cases.<sup>50</sup> Aside from an approximately equal number of songs conforming to Yuman types, introduced with mourning songs to the Southern Paiute, about fifty remain that do not have paired patterning in one form or another. As it is planned to publish this material after its study has been completed, the distribution of the different types within the various song categories will not be discussed here. It may be added that comparatively narrow range and clustering of the phrase finals on the tonic are not uncommon in Southern Paiute songs. It is safe to infer that when a sufficiently large Northern Paiute collection is studied, it also will show this preponderance of "Ghost Dance patterns."

Even the relatively small body of material on which this study of Plains Ghost Dance music is based, demonstrates beyond doubt that it has preserved a surprising degree of stylistic unity while spreading on the Plains. In structure, comparatively narrow range, and some other features, the melodies are essentially in the style of the Paiute groups. That these patterns are of old standing in the Basin is suggested also by their presence in Yuman and Northern Ute music: both of these groups participated little if at all in the Ghost Dance of 1890.

Observations on the text of the songs, the dance movements, and the instrumental accompaniment, if any, are at present insufficient. It is nevertheless clear that the pattern prevalent in Ghost Dance melodies is predominant also in the song text: of 137 texts given without melodies in Mooney's study, 122 have consistently paired text patterns, every line being repeated. This supports musical evidence and, indirectly, strengthens it. But text and melody are not always parallel; there are examples in which the same line is sung to two different musical phrases, and vice versa.

Ghost Dance songs form a style of their own, embedded within the various local styles of the Plains. That is, Plains music did not strongly

<sup>49</sup> Julian H. Steward, *Ethnography of the Owens Valley Paiute* (University of California Publications in American Archaeology and Ethnology, Vol. 33, No. 3, 1933), pp. 279-85.

<sup>50</sup> This collection is at present in manuscript form, containing transcriptions made by J. Sapir of phonograph records taken by Dr. E. Sapir. For the perusal of this manuscript I am indebted to Dr. Sapir; also for the permission to publish a melody from it.

affect the Paiute musical patterns offered to it in the form of Ghost Dance songs. We have, to be sure, pointed to the occasional merging of Plains patterns with Ghost Dance patterns, which at times make it difficult to say whether a Plains melody became remodeled in conformity with paired patterns, or vice versa. That this process did not progress further may be due in part to the exceedingly quick spread of the Ghost Dance movement and to its brief life in many places. Yet where, as in the case of the Pawnee hand game songs, its life was extended, the style continued to cling to its early form. That it was accepted on the Plains without soon suffering modifications may be explained partly by the analogy between the principle of simple progression in Plains music and the principle of paired progression in the Ghost Dance songs. The modifications of the Ghost Dance pattern on the Plains will be better treated when the Southern Paiute material can be presented as a background. More significant perhaps are the effects on the local music of the patterns introduced into the Plains. In a few tribes, including the Pawnee hand game music became permeated with the new pattern. This bears out Dr Lesser's suggestion that the Ghost Dance represented a revivifying and reintegrating force in Pawnee life.<sup>51</sup> If the Pawnee revival of the old rituals and societies in the spirit of the Ghost Dance<sup>52</sup> had progressed further than it did, the new style might have penetrated into Pawnee music beyond the hand game songs. Indeed, there is clear evidence in the form of certain hybrid melodies that such a process was already under way. Appropriately enough these melodies occur in the Bear Dance, itself, according to Lesser, a Ghost Dance revival in recent years.<sup>53</sup> Musical evidence also strengthens the possibility that the Cheyenne as well have incorporated the hand game into their Ghost Dance, and as a result came to have a modified form of the hand game.<sup>54</sup> We know, further, that the Pawnee Ghost Dance is intimately bound up with the Arapaho Ghost Dance, the Pawnee being the receivers. This tallies well with the tables: the salient features of the style are most frequent among the Arapaho examples, with the Pawnee next, and then the Dakota. Taken in the same order, the correlation of the three features selected also becomes less marked.

It has been shown, in the field of European folk music, that single melodies can be diffused with great ease.<sup>55</sup> The music of the Plains Ghost Dance

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<sup>51</sup> The Pawnee Ghost Dance Hand Game, pp. 106, 116, 117.

<sup>52</sup> Lesser, pp. 106-15.

<sup>53</sup> Lesser, Cultural Significance of the Ghost Dance (*American Anthropologist*, Vol. 35, pp. 108-15, 1933), pp. 113-14, fn. 6.

<sup>54</sup> Lesser, pp. 322, 323.

<sup>55</sup> W. Tappert, *Wandernde Melodien* (Berlin, 1890). See also Erich M. von Hornbostel,

represents a case in which what may be called a tribal or regional style became diffused, as the style of a special ceremonial complex, with continuous distribution, through processes which we know from historical evidence to have been exclusively those of culture contact. While this illustrates the great persistence with which music can adhere to a ceremonial or other complex, it also indicates that similarities of style between two disconnected regions would not necessarily imply that some elements of the population itself were to be considered related. Nor would they necessarily prove long and very intimate culture contacts.<sup>56</sup>

The Ghost Dance songs may also have a bearing on the question whether, and in how far, formal features of a musical style can be explained or derived from their function in social life. The present finding is obviously negative. Through processes of diffusion a local style has become the style of a ceremonial complex in another region. In a third region, among the Yumans, features of the style are found scattered in various types of songs, apparently not restricted to any song group with a specific function. On the other hand, among the Northern Ute the style is concentrated in songs of the Bear Dance, a ceremonial that even by a generous stretch of imagination has little to do with the Ghost Dance—unless one insisted on the common idea of renewal. A more plausible connection might be seen in the styles of dancing which may prove to be related. Features of the style apparently spread from the Ute to the Navaho, who incorporated it, probably together with features of Ute dancing, into the curing ritual of the Enemy Chant; mainly into the exoteric parts. These parts of the Enemy Chant form a pool into which individual compositions also may be deposited with considerable freedom, whereas most Navaho music is ritualistic and is not freely augmented, either by composition or importation.

It should not be asserted that such varied functions associated with the same style, or with the same stylistic features, can never have a more profound remodeling effect on the style itself. But the example under discussion suggests that musical form can weather amazingly well the vicissitudes to which it is exposed, although if it comes from a fundamentally different cultural setting, or from a bygone age, it may receive in the new setting a new life and meaning.

YALE UNIVERSITY

NEW HAVEN, CONNECTICUT

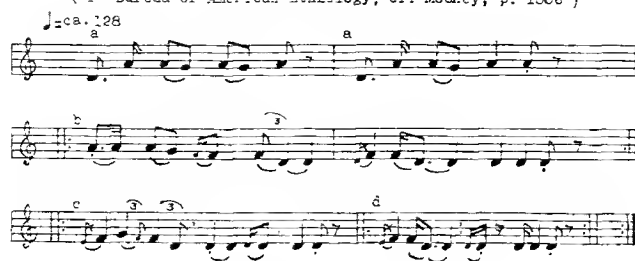
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Notizen über kirgisische Musikinstrumente und Melodien (in R. Karutz, *Unter Kirgisen und Turkmenen*, Leipzig, 1911), p. 214.

<sup>56</sup> I have suggested other examples for the diffusion of musical features in *Special Song Types in North American Indian Music* (*Zeitschrift für Vergleichende Musikwissenschaft*, Vol. 3, pp. 23-33, music pp. 1-6, 1935).

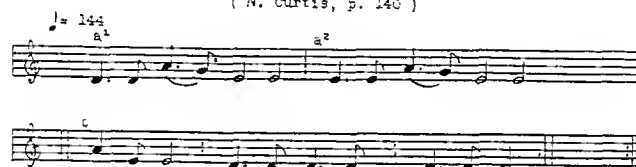
## 1. APAPHO GHOST DANCE SONG\*

( PP Bureau of American Ethnology, cf. Mooney, p. 1006 )



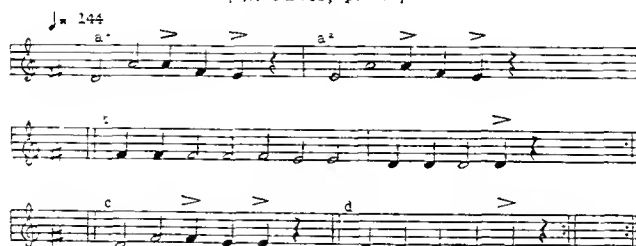
## 2. PAWNEE GHOST DANCE SONG

( N. Curtis, p. 140 )



## 3. TETCH GHOST DANCE SONG

( N. Curtis, p. 26 )



## 4. PAWNEE HAND GAME SONG

( F. Denstore, Pawnee Music, No. 45 )



\* In most music examples taken from the literature I have simplified the signature and the bar lines. The melodies are transposed to the same level in order to facilitate comparison.

## 5. SOUTHERN PAIUTE POAN MOURNING SONG

( E. Sapir, PR 16a )



## 6. NORTHERN UTE BEAP DANCE SONG

( F. Densmore, Northern Ute Music, No. 1 )



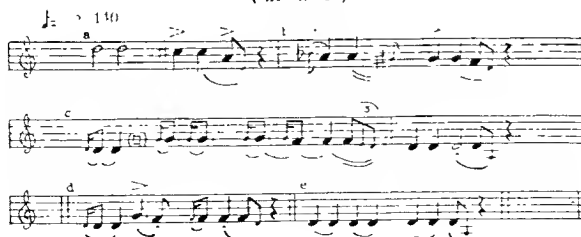
## 7. NAVAHO "WAR DANCE" SONG

( H. Disk 15 B1 )



## 8. YANKTONAI GHOST DANCE SONG

( H. PR 93 )



## THE COMANCHE SUN DANCE

By RALPH LINTON

THE last Comanche Sun Dance was held in 1878, the date being fixed with certainty by an eclipse of the sun visible in Oklahoma in the same year. Even before this time the ceremony seems to have been falling into disuse, due perhaps to the harassed life which the tribe had led during the preceding twenty years. One informant had attended this last dance as a small child, but his memories of it were vague and the account given here must be understood to be based on second hand evidence.<sup>1</sup>

Whether the Sun Dance was ever a regular annual ceremony among the Comanche is uncertain. From at least 1860 on it was held at irregular intervals, whenever some individual received the necessary supernatural sanction. It also seems highly improbable that the whole tribe ever attended a single Sun Dance. The autonomy of the various bands within the tribe was complete and there was little feeling of solidarity among them. Participation seems normally to have been limited to the band of the supervising medicine man with a few others who were in close and friendly contact with it and who came by invitation. Informants frequently described the dance as though only four bands were involved, but this was probably nothing more than a projection of the pattern of using four as a ritualistic number.

Every dance originated in a supernatural sanction received by some medicine man. This was probably in the form of a dream or vision, although informants were not certain. The originator was usually a young man who was just beginning his career. Those who volunteered to dance were also, as a rule, young men who had few war exploits to their credit. It was said that their subsequent careers would be watched to determine whether the sanction received by the medicine man was authentic and that his reputation would be strongly influenced by their success or failure. The medicine man had complete charge of the ceremony, arranging all the details in accordance with his sanction. From the differences in the accounts given by various informants it seems certain that there was a good deal of variation in the procedure, extending even to differences in the form of the lodge used on different occasions.

The ceremony was held approximately in midsummer and continued for eight days, the first four being devoted to the gathering of materials

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<sup>1</sup> The present notice seems to be the first report of the Sun Dance among the Comanche. There are, in fact, specific statements of its nonoccurrence in W. P. Clark, *Indian Sign Language* (Philadelphia, 1885) and James Mooney, *Calendar History of the Kiowa Indians* (Seventeenth Annual Report, Bureau of American Ethnology, Part 1, 1900), p. 322.



and the erection of the lodge and the last four to the dance proper. When the medicine man had selected a place for the dance, invitations were sent out to the various bands. Informants knew of no formality attending this. The bands moved to the place by easy stages, always making four over night halts en route. The camp was pitched in a semi-circle about the site chosen for the lodge. The members of each band camped together, but the bands had no regular order in the circle.

When the camp has assembled, gathering of materials for the lodge was begun. Cottonwood was used for the posts and brush covering and either cottonwood or cedar for the screen inside the lodge. Men and women went out together to cut the smaller posts and brush. They wore wreaths of sage and joked and sang. When the materials had been cut they were tied to ropes and hauled back by groups of men on foot. These men usually halted four times on their way back to rest and sing.

The cutting and transportation of the center pole was left until the last. There seems to have been no ceremony attending its selection. It was cut, in various instances, by a virtuous Comanche woman (i.e., one who had always been faithful to her husband), a virtuous captive woman, and a captive man who had a number of war deeds to his credit. Although informants could give no explanation of these selections, the use of captives for this important office probably reflects their importance in the social and economic life of the tribe. The Comanche captives, who were mostly Mexicans, tended the horse herds and practiced most of the specialized industries such as gun repairing and saddle making. They were also encouraged to accompany their masters on war parties and, if brave fighters, could rise to any position open to a full blood. The captive woman who was faithful to her master or the captive man who had won war honors had behaved in a commendable way and deserved to be honored by the whole society.

The center pole itself was a tree trunk about fourteen feet long and one foot in diameter with a fork at the upper end. After it had been felled and peeled, it was carried bodily to the site of the lodge, twenty-five or thirty men bearing it by means of ropes passed under it. The bearers were changed frequently so that everyone would have a chance to help. Four stops were always made en route.

The construction of the lodge was never begun until all the materials were on the spot. The center pole was erected first, being planted in a hole two to three feet deep. The upper end was supported and guided by means of a forked sapling. Three unsuccessful attempts were made to lift it, it being set on the fourth attempt. At each attempt the medicine man blew

on his whistle of eagle bone. After the center pole had been set, the outer circle of short posts and stringers was erected and rafters run from these to the fork in the center pole. In one case the entire lodge was then covered with cottonwood brush. In another only the walls were filled in with brush, the roof being left open. A single opening was left on the east side to serve as a door. Across the west side of the lodge a row of small cedar trees or cottonwood branches was set up, screening off an alcove into which the dancers could retire to rest and sleep.

While the lodge was being constructed, hunters were sent out to kill a three year old buffalo bull. At the dance where the lodge was fully roofed, the head and fore quarters of this animal were skinned and the skin stuffed with willow twigs, making a sort of manikin. This was set on top of the lodge with its head toward the east. In the dance where the open lodge was used the skin was simply placed in the fork of the center pole. During the time that the materials were being collected and the lodge erected ordinary rules of conduct were relaxed and there was a good deal of sexual license, although this apparently did not apply to close relatives.

When the lodge was finished or nearly finished, four or five clowns appeared. The number of these performers does not seem to have been fixed and anyone could adopt the rôle who wished. These clowns wore conical helmets of willow twigs with the leaves and bark left on, the lower edges of the helmets coming down over their shoulders. They carried circular shields roughly woven of split willows, also with the leaves and bark, and long swabs or clubs made from willow saplings with the leafy twigs at their ends woven into a ball. They had large false noses of mud and their whole bodies, equipment and even their ponies were liberally daubed with mud. They road into camp at a gallop, chasing the people and dogs, striking anyone they could catch with their mud soaked swabs, and performing a variety of amusing antics. Those who were struck could not take offence, but people who did not want to take part in the fun would run into the lodges, where the mounted men could not reach them.

After the completion of the lodge, and probably after the clown's invasion, there was a sham battle. While the finishing touches were being put to the lodge, some of the men had retired to the neighboring creek bottom and built a rough enclosure there, "like a rail fence." In this they established themselves, sometimes having women and children with them. A man who had served as a scout in actual combat came out from the main camp, located this "fort," and returned to report the presence of an enemy. All the warriors in camp then donned their full regalia and went out to attack it, followed by a crowd of spectators. During the attack each man reenacted

his actual war deeds. When the "fort" had been taken, the whole group returned to camp singing victory songs. This episode symbolized the triumph of the Comanche over their enemies.

On the evening of this day the crier rode through the whole camp calling on the people to get ready. The members of each band assembled in front of their tipis and at a given signal all the bands began to move toward the lodge. Each band was marshalled and driven along by its War Whip Bearer. This functionary was chosen for bravery, being presumably the most important warrior in the band. He directed all dances and was allowed to strike any one who refused to dance, or who seemed backward, with his war whip, a long flat club, notched along one edge. Even to point this club at an individual was enough to bring him into the dance since he would fall ill if he refused.

The various bands moved toward the lodge obliquely, avoiding a direct approach to the entrance but marching in such a way that they would all finally congregate in front of it. As they moved each band did its favorite dance and sang a song of its own. When the first song was finished, the War Whip Bearer called on the band to halt, saying that he had something with which to quench the people's thirst. He then recounted some of his war deeds, assuring the people that they were not true, although they actually were. At the mention of each man whom he had killed the drum was struck. After this coup counting another song would be begun and the people would dance forward again. There were four halts of this sort, the last one finding them in front of the door of the lodge. Here the whole group danced for some time with a slow stamping step. Finally, the camp crier announced that the real dance would begin in the morning, saying that the herd of buffalo had been located by the scout sent out to find them. The people then dispersed.

The dance proper began the following morning with the buffalo round up. Four men, all brave warriors, had been selected to play the part of the buffalo. They were disguised in buffalo robes, one always being that of an old and scabby bull. These men were hidden at some distance from the camp and an especially brave warrior was sent out to find them and drive them in. He carried a bow and arrows in his right hand and a torch in his left, these being the means for killing and cooking the buffalo. The warrior came back driving the buffalo before him and into the lodge. Three of them went in at once, but the old bull ran around the lodge four times before he entered. After this ceremony, which symbolized the bringing of food to the people, the "buffalo" removed their costumes and left the lodge quietly. The same person might act both as a buffalo and as one of the regular

dancers, but this was rarely the case since the buffalo were older men of established reputation while the dancers were usually young.

Early on this same morning the wives of the dancers, or, if they were unmarried, any of their close female relatives, made beds for them in the screened alcove at the western side of the lodge. These beds consisted of heaps of sage with robes over them. A large rounded heap of moist sand was also built. When the dancers were exhausted they would retire behind the screen and throw themselves upon this so that they would be revived by its dampness and coolness. After the beds had been made and the sand heap built no one was allowed behind the screen except the medicine man and the actual dancers.

The crier warned everyone who wished to attend the dance to bathe that morning. The dancers also bathed. This was a common preliminary to seeking to establish relations with the supernatural, as when a man was going to a medicine man's grave to get power from his ghost. It was a form of purification or consecration, although in this case it was not attended by any ceremony.

After the noon meal the people went into the lodge and the dance began. The drummers, old men who also led in the singing, seated themselves near the door or about the fire place. Unfortunately, the location of the fire place in the lodge was not ascertained. They had a large circular drum or drums of the sort used in ordinary dances. These drums were beaten by several men simultaneously. They also carried rattles which apparently could be of any sort. The spectators, both men and women, seated themselves around the sides of the lodge three or four deep.

While the audience and orchestra were taking their places, the dancers went behind the screen with the medicine man and painted themselves. Often they helped to paint each other. This painting seems to have been rather varied. In the ceremony in which the roofless lodge was used all the dancers painted solid yellow. In that with the closed lodge each dancer was painted differently and there was a variety of colors and designs. All informants agreed that, although the paint was renewed each morning of the ceremony, each man wore the same designs throughout. The dancers were stripped to breech clout and moccasins and carried no paraphernalia except their eagle bone whistles. None of these were seen, but they are described as identical with those used by other Plains tribes in the Sun Dance.

They were usually eight to twelve dancers. When they first emerged from behind the screen they danced with their arms stretched toward the sun, but as they became exhausted they dropped their arms and merely moved their shoulders with a shrugging motion, stamping at the same

time. Each time they shrugged and stamped they blew a short blast on the eagle bone whistle. When certain special songs were sung all the spectators would join in, rise and dance too to encourage them. The medicine man stayed behind the screen most of the time, but when the dancers seemed to be flagging he would come out and reinvigorate them. He would do this by taking his fan, made of the tail feathers of the road-runner, and, while holding it horizontal, sweep it around in a circle from left to right. As he swept it around he blew along it and each dancer, as it came in line with him, began to dance hard again.

From time to time, apparently whenever interest was flagging, one of the spectators would come forward and hang a gift of some sort on the center pole. While doing so he would pray for a long and happy life. Small boys were encouraged to shoot arrows into the fork of the pole as their offerings. These offerings were sacred and could not be touched. When the people went away at the end of the dance the things were simply left there to be destroyed by the elements. However, visitors from other tribes who happened to be present during the dance could put an offering on the pole and take down something else in exchange. One informant, when a small boy, had visited a Kiowa Sun Dance lodge after the dance and had, on the advice of his grandfather, exchanged his old breech clout for a new one hanging on the pole. When they got back to camp their own people said that this was dishonest.

According to Post Oak Jim, the informant mentioned above, the medicine man in charge might come out from time to time and do sleight of hand tricks to show his power. He might put a knife down his throat and bring it up covered with blood. Then he would blow on the blade and the blood would disappear. Sick people might also call on the medicine man for special help during the dance, although whether he treated them on the spot was not ascertained. No other informants mentioned these features, but healing is such a frequent element in Comanche ceremonies that they may well have taken it for granted. All the spectators were believed to benefit by the ceremony and to go away improved in health and spirits.

On the first day the dance ran from early in the afternoon until sunset. There was then a halt of about two hours to rest and eat, after which the dance began again and continued until midnight. After the evening session had ended, the dancers went down to the creek under guard of a man chosen by the medicine man and bathed. They sat and splashed in the water but were forbidden to dive in or to drink. They then returned and slept behind the screen on the beds prepared for them. They danced the next morning, afternoon, and evening, with intervals at noon and night for them to rest

and for the spectators to eat. This continued for three days. All the people had to bathe every morning before entering the lodge.

The dancers underwent no torture other than exhaustion and abstinence from food and drink. Even the abstinence was not complete. Dancers were allowed to chew slippery elm bark during the rest periods, also to drink the mucilaginous fluid obtained by scraping the inside of strips of this bark. They were allowed to eat the dried wild plums which would be found lying on the ground under plum bushes at this season of the year and possibly other dried fruits as well.

The condition of the dancers was closely watched and if it seemed that any dancer would be unable to last out the full period his father would arrange for his release. The father went to the medicine man and without making him the formal smoke offering required when his aid was asked in other matters, gave him a gift and asked that the young man be excused. If the medicine man accepted the gift, the young man was told to go behind the screen, take up his bed and leave. His father went with him from the lodge to the creek, where the young man washed his mouth out, dived in four times against the current and then washed his mouth again. After this he was free to go home. Diving four times against the current was also a part of the ritual used in relinquishing any supernatural power which had become burdensome. Sometimes another man would take the place of the one who had been released and finish the dance for him, but informants did not know how this was arranged. Actually, such releases were very infrequent, as they were considered disgraceful.

The dance ended at midnight on the fourth day. Informants were not sure whether the dancers underwent any ceremony of secularization at the end of the ceremony. The next morning camp was broken and the various bands moved back to their own ranges. Four over night halts had to be made on the return trip. Even the band in whose territory the dance had been held would move away, leaving the lodge with its offerings deserted. Shortly after the dance a series of small feasts would be given, one in honor of each dancer. To these feasts only the dancer, his relatives, the medicine man, and the old men of the Sun Dance orchestra would be invited.

Very little regarding the symbolism or purpose of the dance is remembered at the present time. The whole lodge is said to have represented an eagle's nest while the buffalo manikin or pelt placed on top of the center pole is said to have been food for the eagle. In their movements and whistling the dancers were supposed to be imitating young eagles not yet able to fly.

Apparently the purpose of the ceremony was partly to increase the importance of the medicine man, who remained the central figure throughout, partly to improve the well being of the whole group, especially their physical health. The dancers, as a result of their participation, received an increase in their supernatural power, but they seem to have drawn this from the medicine man rather than directly from the Sun, the Eagle, or any other supernatural being. This would be in accordance with the general Comanche pattern of obtaining power from other individuals more frequently than they obtained it by direct contact with supernatural beings. Visions might come to the dancers during the dance, but these were in the nature of unusual benefits not specifically sought for or expected. The only instance of such a vision which informants remembered was not that of a dancer but that of a man who impersonated the old bull in the initial ceremony of driving in the buffalo. At the end of the chase this man was so exhausted that, when he started out of the lodge, he partially lost consciousness and had to cling to the center pole. When he came to he told the people that he had had a vision of a white man with long white curly hair. The Comanche believe that this was Jesus, although it happened before missionaries had come among them. As they had had Christian Mexican captives living among them from at least 1750 on, they were probably familiar with Christian ideology even at that time. Since the Comanche are not secretive with regard to their visions or other supernatural experiences, the fact that this is the only Sun Dance vision remembered would indicate that such visions were very rare.

There is documentary evidence that the Comanche did not reach their historic territory until about 1700 and it seems highly probable that they had emerged from the northern plateau area only a few generations before. It is certain that during the historic period they were actively engaged in borrowing elements of Plains culture from their neighbors and that certain of these elements had not been perfectly assimilated at the time of their final subjugation. I believe that the rather wide range of variation in certain details of the Sun Dance, such as the form of the lodge, can best be explained by the assumption that the Comanche had not had the Sun Dance long enough to establish a complete and integrated pattern for the ceremony. If so, many of the details of particular Sun Dances might be determined largely by the officiating medicineman's knowledge of the Sun Dance rituals of neighboring tribes, especially the Kiowa and Cheyenne. Most of the older informants were familiar with the Kiowa Sun Dance and considered their own as very much the same. The use of a fan of roadrunner feathers to invigorate the dancers was cited as a specific identity. At the same time,

they stressed the fact that the Comanche did not use a Sun Dance image while the Kiowa did.

The material on which the preceding report is based was collected by the Ethnological Field Party of the Laboratory of Anthropology, Santa Fé, New Mexico, while working with the Comanche during the summer of 1933.

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## BASIC CULTURES OF THE MISSISSIPPI VALLEY<sup>1</sup>

By THORNE DEUEL

PROBABLY the majority of those who have dealt with the broader aspects of Mississippi Valley archaeology have been struck by the fact that sites in the region show a tendency to group themselves into two large divisions on the basis of the artifacts, burials, and the circumstances of occurrence.<sup>2</sup> These two divisions are, in the main, rather distinctly differentiated one from the other, especially at the extremes of their geographic range. For this discussion, I shall term the one more generally familiar in the southeastern United States as the Mississippi Basic Culture; that better known in the upper valley as the Woodland Basic Culture. This paper is an attempt to outline our knowledge of these basic cultures in the light of a four year survey of the region for the University of Chicago. It is to be remembered that the classification is in no sense dependent upon genetic relationship or spatial distribution; the units are classified as suggested by McKern on "trait complexes" alone.<sup>3</sup>

For convenience in handling certain apparent exceptions, it may be

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<sup>1</sup> In determining the characteristic traits of the two cultures, I am indebted for suggestions and criticisms to Messrs Fay-Cooper Cole, W. C. McKern, Carl E. Guthe, W. M. Krogman, George K. Neumann, and others.

While conducting the Pictorial Survey of the Mississippi Valley for the Department of Anthropology, University of Chicago, I have had an invaluable opportunity to study in detail a large amount of material from many sites in the Mississippi Valley, north and south, and on the Atlantic seaboard.

<sup>2</sup> It should not be assumed that this paper suggests the limitation of the basic cultures in the Mississippi Valley to two types. The cultures of some sites and of some historic groups do not readily fit into either classification. It may be necessary to introduce one or more main divisions to take care of them. On the other hand, it is not to be expected that all cultural manifestations will fall unquestionably and definitely into one and have no traits in common with the other - any more than in zoological classification (or ethnological groups sharing in more than one culture). The determinant complexes selected may be too inclusive; their simplification may solve the difficulty.

<sup>3</sup> Unpublished paper by W. C. McKern, "Culture-type Classification for Midwestern North American Archaeology." This classification as modified by a committee representing the Milwaukee Public Museum, University of Chicago, University of Illinois, and the University of Michigan was issued as a circular from the Office of the Chairman, Committee on State Archaeological Surveys, National Research Council.

It will be recalled that the most inclusive grouping, the *Basic Culture*, is subdivided into *Phases*, *Aspects*, *Foci*, and *Components*. A *Component* is the total complex of traits occurring in a single level at any site. (See also "Certain Culture Classification Problems in Middle Western Archaeology" by W. C. McKern in Circular Series, National Research Council, Committee on State Archaeological Surveys, No. 17, August, 1934.)

well to indicate a further subdivision of the basic cultures into phases. The details by which the smaller units are determined are omitted here for lack of space. The Woodland may be separated provisionally into (1) the Red Ochre phase (including the Adena of Ohio and West Virginia as well as the more widely spread and simpler components of Wisconsin, Illinois, Indiana, Michigan, and New York); (2) the Central Basin phase<sup>4</sup> (comprising the richly developed Hopewell of Ohio and the so-called Hopewell variants of Iowa, Wisconsin, Michigan, Indiana, and Illinois;<sup>5</sup> and (3) the Tampico<sup>6</sup> (less spectacular and less well-known than the two preceding). The pottery of the Tampico phase includes a ware similar to that of Aztalan called "transitional" by S. A. Barrett.<sup>7</sup>

It is to be understood that the list of determinant traits is tentative, that fundamentals have been sought, but some minor diagnostic traits have also been retained, and that exceptions exist in certain phases, although the general complex in each phase of the basic culture is the same.

#### WOODLAND BASIC CULTURE

In detail, the phases of Woodland culture differ greatly from each other, but fundamentally they have more in common than any one of them has with any phase of the Mississippi culture.

*Dwellings.* Houses, round in section and hemispherical or conical in shape were probably an early type that persisted to post-Columbian times. The rectangular house was probably known also and used by some groups a portion of the time. All house types seem to have been generally less permanent than those built in the Mississippi culture.

Mounds were not used as domiciliary bases.

*Ornaments.* Personal ornaments, at least in the more durable media, are

<sup>4</sup> The Hopewell culture of Ohio seems to be a brilliant elaboration of the Central Basin phase, communities of which had extended trade relations (whether organized or not) with groups having cultures related to or very similar to Etowah and Moundville. From these southern neighbors the Hopewellians borrowed liberally in certain materials and forms. A large number of these forms occur rarely or not at all outside the Scioto Valley region. Central Basin signifies a complex of traits more widespread and, in general, simpler than the high specialization in industry and art generally called Hopewell. A simpler aspect of Central Basin may have existed in Ohio prior to the higher development.

<sup>5</sup> The materials found in the "black sands" beneath a mound at Liverpool, Illinois (F<sup>277</sup>) seem to belong to an early, simple aspect of this phase.

<sup>6</sup> The Tampico phase is named from a site opened by Mr Robert Gooden on his farm seven miles east of Lexington, later investigated by the University of Chicago. The cultural manifestation called Lake Michigan by McKern probably constitutes an aspect of this phase.

<sup>7</sup> Ancient Aztalan (Bulletin, Public Museum of the City of Milwaukee, Vol. 13, 1933), pp. 336-42.

not abundant. Those of shell, excepting perhaps in the Central Basin phase, are confined to necklaces. Gorgets of polished stone are moderately common.

*Burial Customs, etc.* Flexed burials are most common although bundle and extended burials sometimes occur.<sup>8</sup> Both the fully flexed and semi-flexed varieties are found, with the former probably the most common.

The dead were interred in highland cemeteries which in many cases developed into mounds as the earth was piled up over successive burials. In the Central Basin phase and in certain aspects of the Red Ochre, it became customary to erect a definite monument in the form of a mound over the important dead.

Burials are characterized by the complete absence or by a very small number of grave offerings. Of such articles, the most common are stone objects, often chipped rather than polished, while pottery is rarely found.<sup>9</sup>

*Work in Stone.* Judging by the number of objects left behind, stone, both chipped and polished, was the material used as a rule for making tools and weapons.

Chipped stone tools were generally fashioned from the core itself, or from flakes struck off directly by a single blow. Secondary flaking was by pressure and in general coarse, the flake often extending from the edge to the median line of the blade. In spite of the coarseness of the flaking, some of the pieces are skillfully and regularly executed and call to mind the Solutrean technique.

The comparatively large size of the projectile points may indicate their use as throwing weapons, or for stabbing. Possibly the bow was unknown in the earlier periods; certainly slight skill could have been attained if the arrows were tipped with such unwieldy stone heads. The use of small flakes and the patent imitation of the small Mississippi projectile forms is seen in certain sites of the Tampico phase. These points are often curved or

<sup>8</sup> In Illinois mounds, definitely identified as belonging to the Central Basin phase, the extended burial is the most common type, with the bundle burial second in frequency, and the flexed type the least common of the three.

<sup>9</sup> The profusion of material associated with the dead in some Hopewell and Adena mounds in Ohio seems a contradiction to this statement. Nevertheless, in Illinois and Wisconsin at least, many mounds belonging to the Central Basin phase have few grave offerings, or none. F 188 in the Ogden group, probably of this phase judging by total burial complex, contained thirty-six skeletons but not a single artifact (Univ. of Chicago field notes, 1931). See also McKern, "A Wisconsin Variant of the Hopewell Culture," pp. 246-66. F 11, the only mound of the Red Ochre phase explored by the University of Chicago, yielded many artifacts—chiefly of chipped stone—with its burials. On the other hand, in the Ulrich and Western-haver mound groups (Ohio), probably of the same phase, the majority of the skeletons had few or no offerings (W. C. Mills, *Certain Mound and Village Sites in Ohio*, Vol. 2, pp. 250-58, Vol. 3, pp. 153-66).

in other ways readily distinguishable from the more slender southern ones.

The polished stone forms include most of the problematical types,<sup>10</sup> the celt and its specializations—the adze and the gouge—and the grooved axe with its numerous variants. The last is foreign to Mississippi phases. There seems to be no generalized pipe form, the types being phasic traits.

The discoidal seems to be generally lacking in the Woodland culture.

*Work in Shell, Bone, Mica, Copper, and Hematite.* This type of industry was comparatively rare. The products of wood-working and the textile industries appear even more rare, probably due to the impermanence of the material. Cords and netting were probably in use in all phases. Some of the earliest sites disclose pottery with imprints of basketry and coarse cloth on it, although this is rare. Wood was probably used extensively for tool handles.

Necklaces of shell, awls of split deer bone, and hematite celts occur occasionally. Axes, knives, and other implements of copper appear more frequently in the Central Basin phase.

*Pottery.* Pottery-making was probably general, except possibly in the Red Ochre phase, but was not elaborated nor skillfully fashioned. Grit-tempering was commonly employed, although shell and vegetable fibre may have been utilized too in certain areas. Pottery seems to have been subjected to prolonged but uneven and poorly controlled firing, as indicated by the color and its variation in the same sherd. Surfaces were generally smoothed to a degree, and unslipped. Cord-roughened exteriors were common. The chief pottery shape was the elongated globular form, with a slight constriction between lip and shoulder, and with a conoidal (or truncate) base. Effigy pots were not made, and handles, flanges, and legs were lacking or very rare.

Decorations, when present, were confined for the most part to the area adjacent to the lip and rarely extended below the shoulder (or line of greatest expansion of body). Decorations, when they occur, are chiefly incising, punctating, and toothed stamps.<sup>11</sup> The simple punctate and stamped elements are often arranged in straight lines paralleling each other and the lip of the vessel.

*Art.* The art objects most commonly preserved from the Woodland basic culture are the forms in chipped and in polished stone.

The Woodland projectile-maker frequently turned out beautiful forms with a flaking pattern resembling the Solutrean, e.g., the leaf-shaped blades of the Red Ochre phase and the notched forms in the grave offerings of

<sup>10</sup> Problematical forms are relatively infrequent in Illinois as compared to certain areas in the eastern United States.

<sup>11</sup> This should not be confused with the technique of paddle stamping with its more elaborate patterns, used by the Cherokee and others

Central Basin sites. Banded slates and other varicolored stones were fashioned into beautifully symmetrical shapes known as bannerstones, birdstones, amulets, etc., probably best designated problematical forms.

Both the Central Basin and the Tampico phases borrowed liberally from the Mississippi groups in the South or those who invaded the northern valley along the principal rivers in pre-Columbian times. Nevertheless, these phases appear fundamentally Woodland.

#### MISSISSIPPI BASIC CULTURE

In general, it may be said that this culture is more elaborate than the Woodland culture.

*Dwellings.* The houses may have been commonly rectangular in plan, although round as well as oblong types were noted by early travelers in certain southern villages. The roofs (at least in historic times) were arched or gabled. In some regions the wall structure consisted of a network of poles and cane (or brush) plastered over with clay, as recorded by Du Pratz and others for the Natchez and their neighbors. Briquettes or masses of burned clay enclosing casts or impressions of grass, reeds, or cane are generally accepted as fragments of house walls.

*Ornaments.* Objects for personal adornment were numerous and seem to have graded imperceptibly into insignia of rank or birth. Necklaces, armlets, belts, leg bands, anklets, ear spools, ear plugs, headdresses, gorgets of copper and shell, and pendants were common at least for important personages. Materials employed included shell, bone, stone, copper-jacketed wood (shell, stone, etc.), sheet copper, mica, and pottery. Pearls were popular for necklaces, and for pendant strings associated with ear spools and gorgets of copper and shell.<sup>12</sup>

*Burial Customs, etc.* Bodies were usually interred in the extended position on the back, although bundle, flexed, sitting, and urn burials, and cremation were not unknown.

Interment was made in cemeteries, although important individuals were buried in the tops of temple (and domiciliary) mounds even in historic times. Mounds were not in general erected as monuments over the dead.

In many instances numerous objects were associated with the dead, the most common being pottery, ornaments in shell and sheet copper were also frequent, and more rarely, stone objects.

Mounds were used as substructures for temples and houses of important officials. They were situated in or adjacent to the village and in certain historic instances, at least, were associated with the chunky

<sup>12</sup> Ornaments, including pearls and ear spools in considerable quantities in some sites, and headdresses of antler have been found in the Central Basin phase (Woodland Culture), chiefly in the Hopewell of Ohio.

ground. The chunky game may have had a position of ceremonial importance somewhat similar to the ball game of Middle America.

*Work in Shell, Bone, and Copper.* The work in shell, bone, copper, and pottery was highly developed. Artistic and ornamental as well as utilitarian forms were produced.

Shell was cut, ground, perforated, and engraved. Bead shapes were numerous; including the globular, globular flattened at the poles, cylindrical, flattened cylindrical, elongated cylindrical with tapering ends, barrel-shaped, olive-shaped, annular, disk and discoidal, and phalangiform or spool-shaped. Ear spools, disks and plugs, spoons with cut handles, perforated hoes, ladles, drinking cups, gorgets, and pendants were made from river or marine shells. The marine species commonly employed were the *Busycon perversum* and *B. contrarium*.

Bone appears to be a favorite source of material for implements of certain kinds, for example, the piercing implements, needles, bodkins, awls, hairpins, and fishhooks. Awls of deer ulna and the tarso-metatarsus of the wild turkey are common in this culture and rare in the Woodland culture. Antlers served for handles, socketed projectile points, and probably for flakers. Bone, less generally, became the medium for pendants, rings (used in hair or with headdress), and bracelets.

Certain regions yield flutes, whistles, and musical rasps of bone. Engraving on bone is rare.

Copper was used, chiefly in sheet form, for making ornaments and insignia. Beads of rolled sheet copper are common. Celts and copper tools are, in general, rare.

*Textiles.* Cords, woven textiles, feather cloth, matting, etc. preserved by copper salts or by charring, and impressions of fabric on pottery give us a hint of the progress in the weaving art. Occasionally pottery indicates a knowledge of basketry. Nettle fibre, bast, and split cane were employed perhaps generally.

*Agriculture.* Most groups seem to have depended upon the cultivation of maize. This gave rise to comparatively extensive and populous villages. The use and culture of tobacco was known by many groups. Whether or not some of the earlier component groups were acquainted with either maize or tobacco constitutes an interesting problem.

*Fishing.* The recovery of fishhooks (unbarbed) of bone in different regions indicates a rather general use of this implement in catching fish. The use of poison, resorted to among historic southern tribes, may have been practiced.

*Pottery Vessels.* Shell seems to have been the most common material used for tempering. Grit, vegetal fibre, and crushed sherds were also used.



Woodland Basic Cultures. A, A series of natural hillocks on a ridge near Lewistown. These hillocks are capped with low rounded mounds that are scarcely discernible and whose artificial nature can be determined only by excavating. Mounds of the Central Basin phase occur more frequently on the river flood plain and are usually conspicuous. B, Fully flexed burial lying on right side (F<sup>o</sup>12, Lewistown, Illinois). C, Semi-flexed burial (F<sup>o</sup>13, Lewistown, Illinois). D, A smaller vessel found with burials in a rock-walled tomb near Quincy, Illinois. It belongs to the Tampico phase. E, F, G, Conical based pots from the Trempealeau Mounds, Wisconsin, Central Basin phase. (Courtesy of the Milwaukee Public Museum.) H, I, Two truncated conoidal vessels of the Central Basin phase. (H is from a mound on Rock River, near Milan, Rock Island County, Illinois. I from a village site, F<sup>o</sup>49 near Lewistown, Illinois.)



Mississippi Basic Culture. A, Mounds of the Cahokia group near East St. Louis, Illinois. The one at the left is the typical flat-topped pyramid. B, A briquette or piece of burned clay with reed impressions and casts, probably a fragment from a house wall (from the Hazel site, Arkansas). C, Water bottles. D, A comparatively rare though widely distributed Mississippi cultural form (found near Lewistown, Illinois). E, Effigy and effigy-head water bottles (from New Madrid County, Missouri). F, Most common type of pottery vessel, the wide-mouthed olla (1934, Lewistown, Illinois). G, Effigy-head shallow bowl. H, Fragment of cloth preserved by contact with a copper gorget from a mound near Spiro, Oklahoma. I, A skeleton lying on back, in the fully extended position from F<sup>14</sup> (1916, Mound 14, Fulton County, Illinois) near Lewistown.



Much excellent ware exhibits no tempering. The paste and surfaces are in general gray or grayish brown, probably due to a low and uniform temperature of firing. In the Middle and Lower phases, two or three wares may be encountered in the same component, that is, they existed contemporaneously. Surfaces are generally smoothed and the application of slip and subsequent polishing are not uncommon in the two phases just mentioned. On the whole, cord-roughened exteriors occur only in limited areas.

Vessel shapes exhibit considerable variation, the predominating form being the flattened globular and its variants. Among the subtypes may be mentioned the wide-mouthed olla with outcurving rim, the shallow bowl with straight or incurving rim, the water bottle, the beaker, the cup, the plate, and toys in all shapes. Handles are common, and nodes, lobes, flanges, tripod legs, and effigy forms are of frequent occurrence, for utility purposes, for decoration, or for both.

Decoration of pottery was characteristic of all phases. The body was the favored area for ornamentation, more rarely the "neck." The decoration of inner surfaces was confined, in the main, to the flaring rims of plates and shallow bowls and to the interiors of slipped and painted bowls. The techniques of decoration in vogue were many, and varied to a greater or less degree according to focus and region. The employment of incising and trailing before firing, scratching and engraving after firing, punctating, paddle-stamping, modeling, appliqué, notching by pressure or filing, lobing, slipping, polishing, and painting were widespread if not general.<sup>13</sup> The effigy pot, the effigy head shallow bowl, the scroll, meander, cross, swastika, and cross-lined triangles constitute motifs occurring over wide areas.

*Pottery Objects Other Than Containers.* Disks made from sherds, perforate and imperforate, are common to all phases. Numerous implements and ornaments modeled in clay, among which might be mentioned trowels and pipes, are found in all phases.

Pipes of clay are more numerous than those of stone.<sup>14</sup> The prevailing type is the "equal-armed" elbow pipe, one in which the stem is about as long as the bowl portion or a trifle shorter. In the more generalized variety, the outer ends of both bowl and stem flare. Some of the phases yield the effigy pipe in massive stone.

*Work in Stone.* The objects in chipped and ground stone form relatively a much smaller proportion of the cultural remains than in the Woodland culture.

The projectile points are small isosceles triangles in outline. They are

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<sup>13</sup> The pottery complex of the Upper phase is somewhat simpler and less elaborated than the other phases

<sup>14</sup> Recent studies indicate that this is probably true for the Lower phase only.

fashioned from thin parallel-faced flakes, which appear to have been struck from a core by indirect percussion (that is, by the use of hammer, punch, and rest). The side-notched isoscles triangle and the small willow leaf shape occur less generally.

The celt and the discoidal were the only generally occurring polished stone forms. The specialized adze form is found frequently. Although the celt was common to both Woodland and Mississippi, the grooved axe was practically unknown in the latter.

Tools made of rough stone included hammer-stones, grooved abrading stones, "arrowshaft polishers," whetstones, and hones.

*Woodworking.* The fashioning of forms in wood seems to have reached a high point of development. Wooden forms included hafts for celts, axes, and hoes; cores for earspools, rattles, and other objects are preserved through the presence of copper salts or charring. The specimens recovered are evidence of skill and artistic ability, indicating there was considerable familiarity in producing symmetrical forms in wood.

*Art.* Among the Mississippi groups, art had reached a high degree of perfection. Gracefully proportioned and highly decorated pottery vessels, representations of human and animal figures, and geometric designs skillfully engraved on shell plaques or embossed in sheet copper, realistically and artistically modeled pottery forms of birds, animals, shells, gourds, melons and, less frequently, human heads; while figurines in clay, stone and bone are far from rare. Artistic productions are less numerous in certain aspects of the Middle and Upper phases.

#### SUMMARY

	MISSISSIPPI	WOODLAND
I	House type	
	(1) Rectangular, semi-permanent	(1) Round, temporary
	(2) Mounds primarily substructures, pyramidal	(2) Mounds not used as substructures
II	Burial customs and ceremonials	
	(1) Predominantly extended	(1) Predominantly flexed
	(2) Simple interment in cemeteries	(2) Burials generally in or under mounds constructed solely for funereal purposes
	(3) Grave goods profuse	(3) Grave goods lacking or few in number
	(4) Grave goods usually pottery, copper, and shell	(4) Grave goods chiefly of stone when present; very rarely, pottery
III	Industries and art forms	
	(1) Pottery, shell, copper, bone most highly developed	(1) Stone most highly elaborated

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| <p>(2) Stone</p> <p>(a) Projectile points: simple small isosceles triangle from thin flat flake technique</p> <p>(b) Discoidals common</p> <p>(c) No grooved axes (or rare)</p> <p>(d) Grooved abrading and whetstones common</p> <p>(e) Problematicals in polished stone very rare</p>  | <p>(2) Stone</p> <p>(a) Notched and stemmed types, medium to large in size, from core, or thick coarse flake technique</p> <p>(b) No discoidals (or rare)</p> <p>(c) Grooved axes</p> <p>(d) Grooved abrading stones and whetstones rare</p> <p>(e) Problematicals more common than in Mississippi</p>   |
| <p>(3) Articles of bone commonly occurring</p> <p>(a) Fishhooks</p> <p>(b) Awls from ulna of deer and awls from tarso-metatarsus of wild turkey</p>  | <p>(3) Bone</p> <p>(a) Generally bone fishhooks lacking</p> <p>(b) Split bone awls from metapodial of deer and wapiti</p>  |
| <p>(4) Textiles: evidence of woven material frequently found</p>   | <p>(4) Textiles: evidence of cords common, of netting scant; of other fabrics even scarcer.</p>  |
| <p>(5) Pipes: clay pipes common, equal-armed pipes. Massive effigy of stone in some phases.</p>  | <p>(5) Pipes: vary according to phases but are generally unlike Mississippi forms. Possibly forms in stone more common than in clay (?)</p>  |
| <p>(6) Pottery</p> <p>(a) Tempering predominantly shell</p> <p>(b) Generally low but fairly well controlled firing temperatures</p> <p>(c) Wide variety in shape and decorative technique</p> <p>(d) Round to flat bases; flattened globular shape</p> <p>(e) Handles, flanges, lugs, and feet</p> <p>(f) Effigy forms common</p> <p>(g) Objects other than containers</p> | <p>(6) Pottery</p> <p>(a) Tempering predominantly grit</p> <p>(b) Prolonged but uneven, poorly controlled firing</p> <p>(c) Slight variation in form</p> <p>(d) Conoidal (and truncated conoidal) base; elongated globular (or globular)</p> <p>(e) Secondary features very rare</p> <p>(f) No effigy forms</p> <p>(g) Containers and pipes only</p> |

*Explanation of Figure 1.*

## POTTERY SHAPES: WOODLAND BASIC CULTURE

The Woodland forms shown here occur in the northern United States, east of the Mississippi river as far south as Tennessee and Georgia. Many of the forms occur in Iowa and apparently in Minnesota, and southern and eastern Canada as well.

In some of the earlier archaeological reports, it is often impossible to secure a complete pottery description or to determine the artifacts in association. Wherever information is afforded, the fragmentary complex accompanying these Woodland pottery forms appears to belong fundamentally to the Woodland basic culture. Moreover, they do not occur on sites known to be "pure" Mississippi.

No complete forms pertaining to the Red Ochre phase have been identified and consequently none appear in the accompanying outlines. Even the sherds found in Illinois have been too small to warrant any trustworthy restoration.

The outlines of Woodland and Mississippi pottery forms indicate roughly the relative sizes in which they usually occur.

1. Central Basin Phase. From a vessel found in central fireplace of Mound 1, Turner Group, Hamilton County, Ohio. Outline drawn from Figure *i*, Plate 22 (opposite page 90), Charles C. Willoughby, *The Turner Group of Earthworks*, Hamilton County, Ohio, (Papers Peabody Museum, Harvard University, Vol. 8, No. 3, 1922).

2. From a vessel discovered in village site on Pocatusquing Creek three miles south of Trenton, New Jersey. Outline taken from Figure *c*, Plate 158 (opposite page 176), W. H. Holmes, *Aboriginal Pottery of the Eastern United States* (Twentieth Annual Report, Bureau of American Ethnology, 1903). Phase undetermined.

3. Central Basin Phase. From a vessel (restored from fragments constituting from one-third to one-half of the pot including entire base) found on F-49 (i.e., village site 49 of Fulton County, Illinois).

4. Central Basin Phase. Vessel found with two others in a mound near Laporte, Indiana. Drawn from Figure 74c, page 191, W. H. Holmes, *op. cit.*

5. Central Basin Phase. Pot with truncated conoidal base found in Adler mound near Joliet, Illinois. Presented by George Langford to Department of Anthropology, University of Chicago.

6. From kitchen midden in Wilkes County, South Carolina. Drawn from Figure *e*, Plate 130 (following page 148), W. H. Holmes, *op. cit.* Almost identical forms are reported by Holmes from mounds near Naples, Illinois, Milledgeville, Georgia, and by others from Wisconsin and New York. Phase unidentified.

7, 8. Tampico Phase. Two vessels from a village at Mossville, Peoria County, Illinois, in the collections of the Peoria Academy of Science.

9, 10, 11. Tampico Phase. These are somewhat more specialized forms occurring in the Dick mound south of Quincy, Illinois. They were secured by the University of Chicago expedition of 1928. The "coconut shell" type has been found near Washington, D. C., on Pocatusquing Creek near Trenton (mentioned above), from Tennessee, and from Missouri.

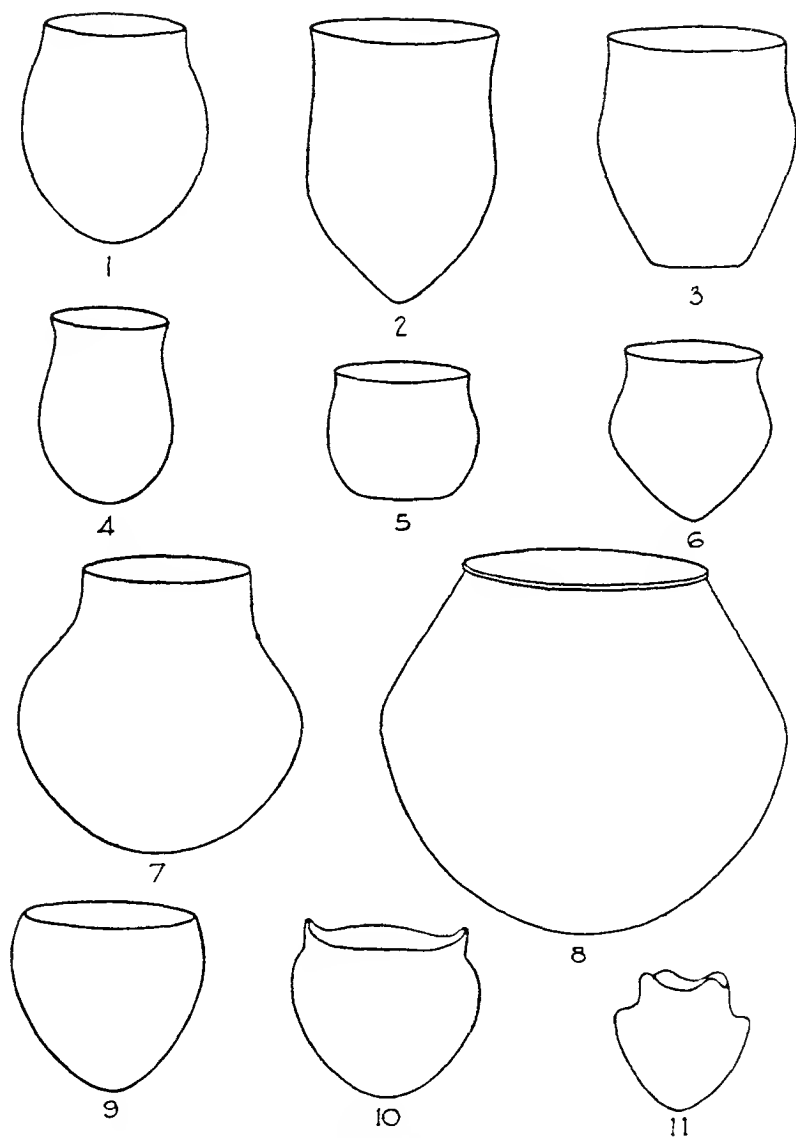


FIG. 1. Pottery shapes. Woodland Basic Culture.

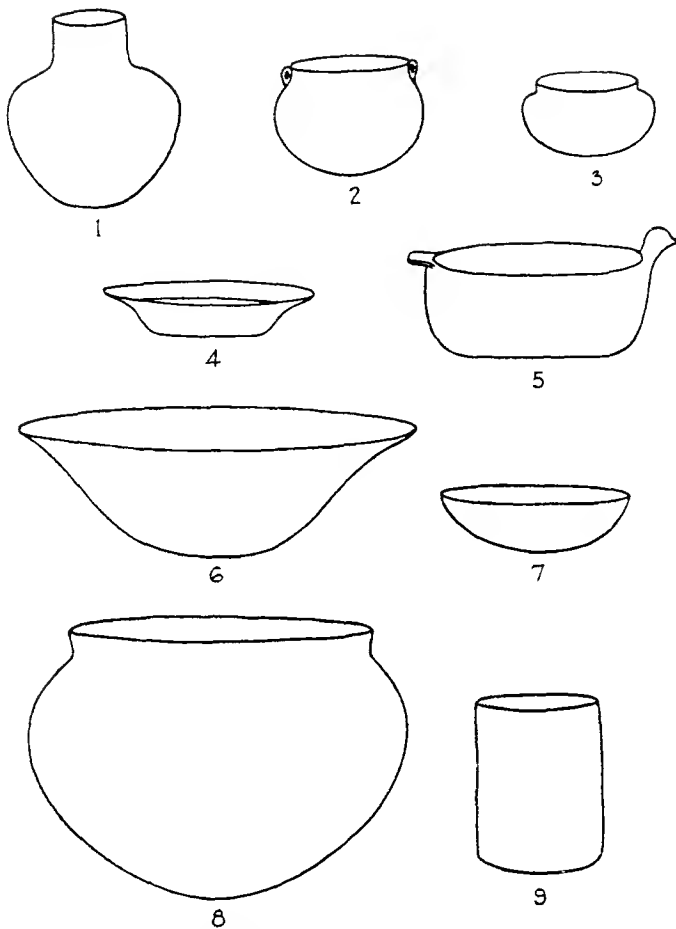


FIG. 2 Pottery shapes Mississippi Basic Culture.

The Mississippi forms selected are, with the exception of 6, 8 and 9, from F<sup>34</sup> (Dickson mound) near Lewistown, Illinois. They are generalized types of this basic culture.

1. A water bottle with medium width and height of neck.
- 2, 3. Wide mouthed ollas, the most common form in the Mississippi culture.
4. A plate form
5. An effigy head shallow bowl.
6. A deep plate (or shallow bowl with flaring platelike rim) from Taskigi village and burial site, Montgomery, Alabama. Collection of P. A. Brannon.
7. Plain shallow bowl from F<sup>34</sup> (the Dickson mound)
8. A wide mouthed olla. A burial urn from the Taskigi site near Montgomery, Alabama, from collection of Alabama Anthropological Society. Note tendency toward pointed base while body is of flattened globular type
9. Beaker type. A generalized shape from Moundville, Alabama, in collection of Alabama Museum of Natural History

*Explanation of Figure 3.*

## POTTERY RIM SECTIONS

The rim sections taken from actual vessels of large sherds belonging to Lower and Middle phases were chosen as representative of general types. As mentioned before, the wares of the Upper phase are somewhat less varied.

Rim sections are oriented with the horizontal as they were in the original vessel when it sat upright.

The interior of the vessel is always to the right. Sections are one-half actual dimensions (one-quarter actual area).

## MISSISSIPPI BASIC CULTURE

1. Beaker from F°34 (Dickson mound), Lewistown, Illinois.
- 2, 3. Wide mouthed olla from F°34.
4. Wide mouthed olla from F°34. The angularity of shoulder is emphasized in the Middle phase more often than in the other two.
5. Shallow bowl from F°34.
6. Plate fragment from Moundville village site in collections of Alabama Museum of Natural History.
- 7, 8, 9, 10. Large wide mouthed ollas with thin walls from F°14 (Morton mound group), Lewistown, Illinois. Closely similar sections are found in similar ware on the Moundville village site.
11. Water bottle from F°34.

## WOODLAND BASIC CULTURE

These sections are from vessels and large rim sherds chiefly from Illinois.

12-18 represent the coarser Central Basin ware. A large proportion of it is embossed near the lip as shown in Nos. 13, 16, and 18, and (or) have the exterior or inner rims notched by stamping or otherwise as in 13 and 14. From F°49 (Whitnah village site) near Lewistown, Illinois. University of Chicago expeditions, 1930-32.

19. This is an example of the "collar and channel" rim variety having approximately the same distribution as the Central Basin phase. This ware occurs in a relatively small amount on the larger village sites of the phase in Illinois and not at all on many others. The thin ware to which it belongs has a somewhat wider distribution in this state than this specialized variety of rim. F°49, University of Chicago expedition, 1930-32.

20. Central Basin phase probably from F°88, near Liverpool, Illinois.

21. Tampico phase. Found near surface in F°85 (Gooden mound group), Maples Mills, Illinois.

22. Village site near Mossville, Illinois. In collections of Peoria Academy of Science.

23. Tampico phase. Rim sherd found in F°85. Lip is somewhat squarer than represented in the figure.

24, 26. Tampico phase. From vessels from A°28 (Dick mound group) near Quincy, Illinois. University of Chicago expedition, 1928.

25. Tampico phase. From a mound on the Hess farm, Jo Davies County, Illinois, excavated by University of Chicago expedition, 1927.

27. Tampico phase. From F°85. Note resemblance of No. 10, Mississippi culture, and this figure. An unusual Woodland section.

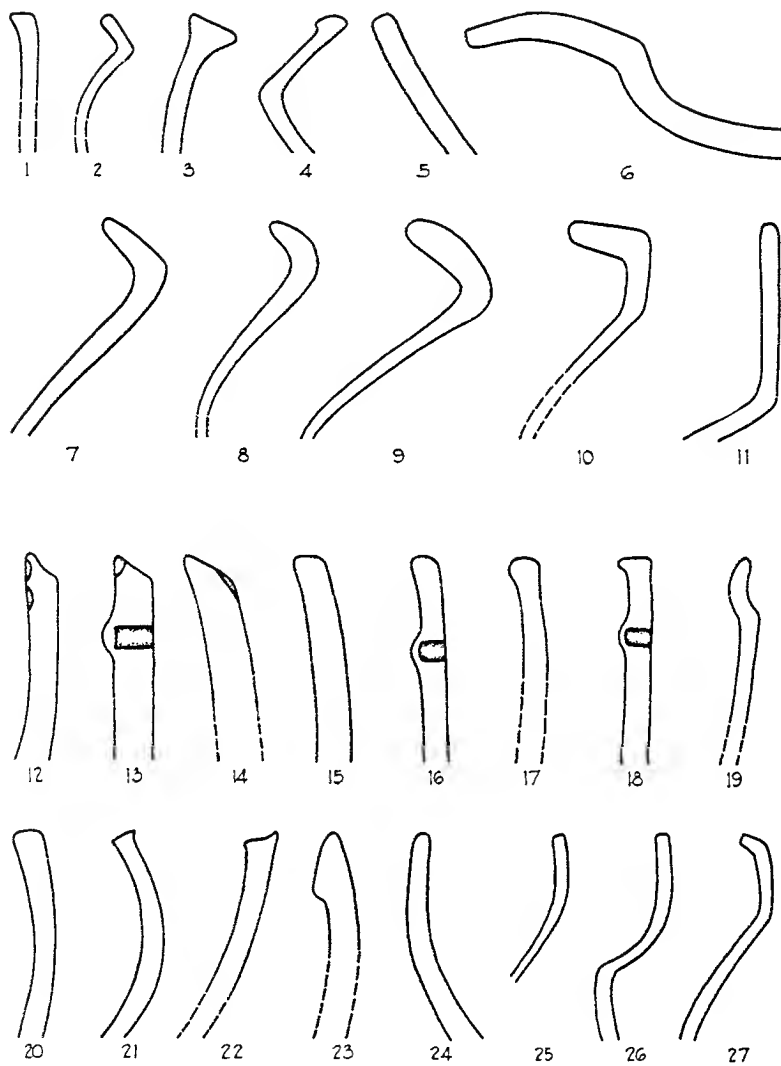


FIG. 3. Pottery rim sections. Mississippi and Woodland Basic Cultures 1-11, Mississippi, 12-27, Woodland.



*Explanation of Figure 4.*

## ARTIFACTS. WOODLAND BASIC CULTURE

These forms illustrate general Woodland forms although many of the specimens shown here cannot be identified by phases.

The leaf- and truncated leaf-shaped forms of Nos. 1 and 2 pertain to the Red Ochre phase. No. 8 is a type often constituting a part of the grave offerings in Central Basin mounds. The stemmed and notched types of medium to large size occur in all Woodland and rarely in Mississippi phases.

The grooved axes (15-17) are found in the Woodland but are lacking generally from the Mississippi basic culture.

The celt occurs in Woodland and Mississippi phases. The one shown here (18) of rectangular section is from F<sup>o</sup>11 (Morton mound group) of the Red Ochre phase. This particular variety is probably not a general Woodland one.

The gorgets and problematics (19-22) may belong only to definite phases, as for example 21 which seems to be identified with the Adena aspect (?) of Red Ochre phase.

The pipes differ generally from the Mississippi forms and do not resemble each other. No. 23 is drawn from one figured by W. H. Holmes, *op cit*, Plate 160, Figure g and is of clay. Stone tubes, possibly pipes, occur in the Adena aspect (?). The platform pipe of stone with straight or curved base appears peculiar to the Central Basin phase. The long-stemmed elbow pipe usually of clay is widespread in the Tampico phase.

*Explanation of Figure 5.*

## ARTIFACTS. MISSISSIPPI BASIC CULTURE

1-4 The small simple triangular projectile points appear to be present in all Mississippi phases. The side-notched and its variations do not occur in Upper Mississippi or at least rarely.

5-7. Discoidal and biconcaves of stone.

8 Pottery trowel of clay.

9. Clay pipe of equal-armed variety.

10. A celt of polished stone which may not be confined to Mississippi cultures but is, in shape, a form commonly met with in Middle and Lower phases.

11. A marine shell (*Busyon*) with groove for suspension.

12. A bone awl from the ulna of deer.

13. A bone awl from the tarso-metatarsus of the wild turkey.

14. A grooved abrading stone (sandstone).

15. A socketed and barbed projectile point of antler.

16. A bone fishhook.

17. A *Unio* shell hoe.

18, 19 Shell spoons found in pottery vessels.

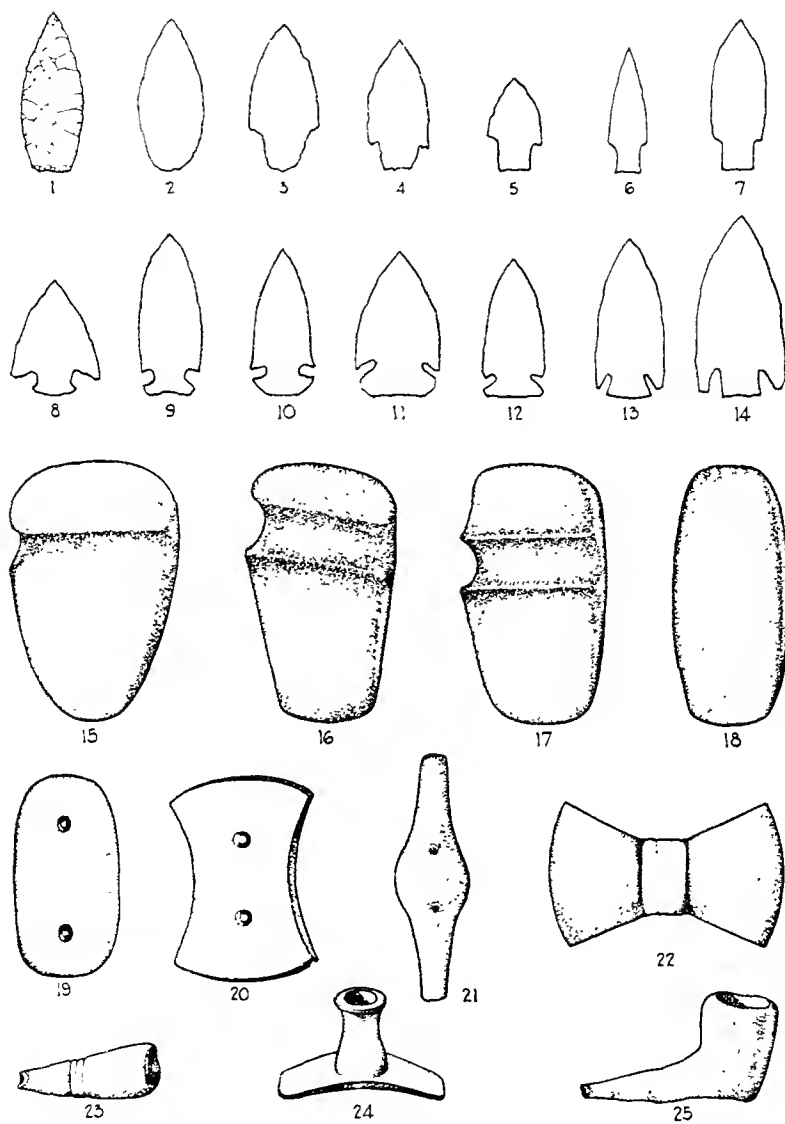


FIG. 4. Artifacts Woodland Basic Culture

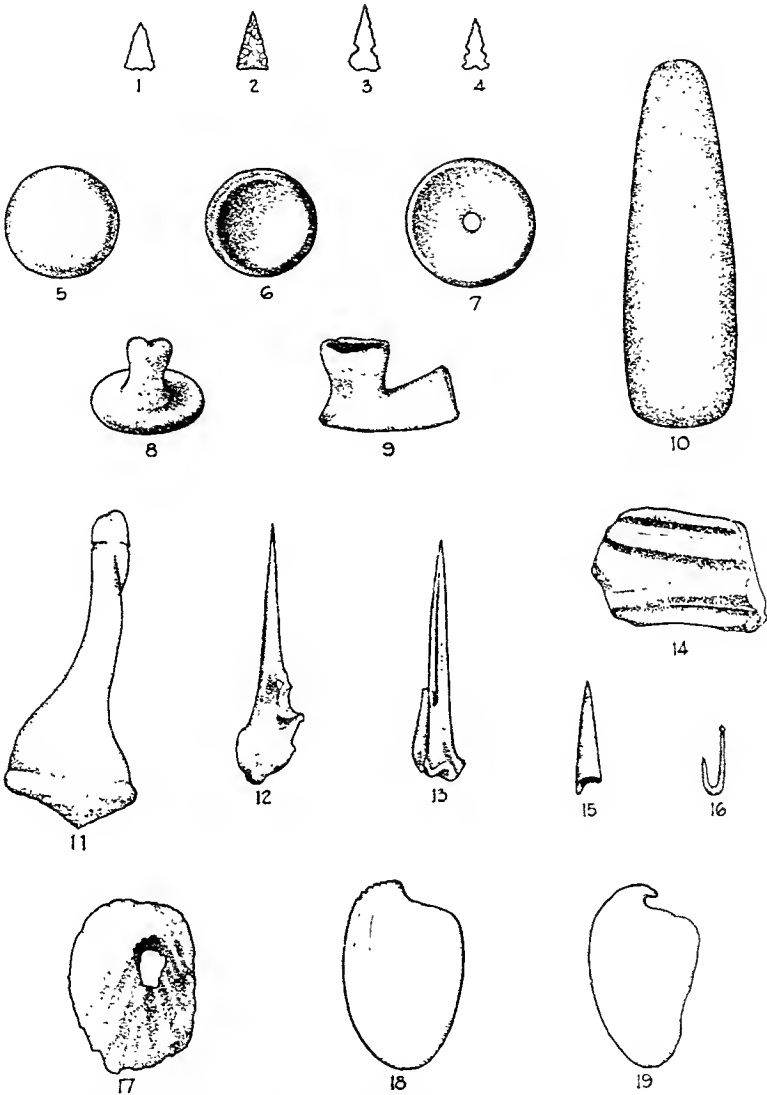


FIG. 5. Artifacts: Mississippi Basic Culture.

SOME NOTES ON WINNEBAGO SOCIAL  
AND POLITICAL ORGANIZATION<sup>1</sup>

By TRUMAN MICHELSON

THE easily accessible accounts of Winnebago social organization are Morgan's *Ancient Society*,<sup>2</sup> J. O. Dorsey's *Siouan Sociology*,<sup>3</sup> the article *Winnebago* in the *Handbook of American Indians*, initialed by James Owen Dorsey and Paul Radin,<sup>4</sup> and Paul Radin's *The Clan Organization of the Winnebago*,<sup>5</sup> his *Winnebago Tribe*,<sup>6</sup> and lastly his *The Social Organization of the Winnebago Indians. An Interpretation*.<sup>7</sup>

None of these authors apparently were aware that a number of important statements on this and kindred topics are to be found in Caleb Atwater's *Remarks made on a Tour to Prairie du Chien; thence to Washington City, in 1829*.<sup>8</sup> I quote from this as follows.

Carrymaunee (walking turtle) a Winnebago chief carries a large Tortoise, fully extended, and beautifully painted perfect in all its limbs, on his back as he marches onward at the head of the turtle tribe. In the same manner, Snakeskin marches at the head of the snake tribe, with the skin of a large snake tied around his neck. In fine, every tribe has its standard bearer, with appropriate emblems (p. 87).

Their form of government is aristocratical, and the whole structure of their society is equally so. The Winnebagoes are divided into seven tribes, or bands, some of these are named after animals, such as the Turtle tribe, the Snake tribe, wolf tribe, &c., and others are named after inanimate things, as the Thunder tribe. These tribes dwell in different places, in towns or villages; in each one of which, there are two Civil Chiefs, who govern that town—for instance Du Corri and Winneshiek are at the head of the government, of the Le Croix village situated near the Mississippi river, on its eastern bank, 80 miles north of Prairie Du Chien. So of all the other tribes, each one of which, has its town or seat of government, and is governed by its two civil chiefs. The civil government of the Winnebagoes, is in the hands of fourteen civil Chiefs, and when they are assembled in one council, it is the grand national council.

<sup>1</sup> Printed by permission of the Smithsonian Institution.

<sup>2</sup> Page 157.

<sup>3</sup> Fifteenth Annual Report, Bureau of American Ethnology, pp. 240-41.

<sup>4</sup> Bulletin, Bureau of American Ethnology, 30, Pt. 2, pp. 958-61.

<sup>5</sup> *American Anthropologist*, Vol. 12, 1910, pp. 209-19.

<sup>6</sup> Thirty-seventh Annual Report, Bureau of American Ethnology, p. 181 *et seq*

<sup>7</sup> Museum Bulletin, Canada Department of Mines, Geological Survey, No. 10 (Anthropological Series, No. 5)

<sup>8</sup> Published by Isaac N. Whiting, Columbus, Ohio, 1831. I have not seen the edition (of the same date) published by Jenkins and Glover. Atwater's *The Indians of the Northwest*, etc., Columbus, 1850, apparently has the same contents and pagination as his *Remarks* which I have seen. So we may presume that the 1831 edition (which I have not seen) of his *The Indians of the Northwest* is the same also. "The Writings of Caleb Atwater" (Columbus, 1833) has apparently disappeared from the Library of Congress. The proofsheets have been verified by consulting the 1850 edition, differences in capitalization being ignored.

In each village, the two civil chiefs, appoint all the officers, deemed necessary, civil and military, who obey them implicitly. There are two ways of arriving at these high stations, by birth and by election. When the father dies, if he has a son, who has arrived at the age of manhood, and who bids fair to make a good chief—that is, if he possesses a good form, has good bodily powers and mental faculties—is brave, sedate, wise and prudent, he generally succeeds his father in the government, on his father's demise. If the chief, at his death, leave no son who is qualified for the high office of chief, but wills it to some other person, he succeeds to the government. If the chief has no son at his death, it is commonly the case, that his brother's son succeeds him. The line of succession may run out for want of a lawful heir, which is always supplied by an election. It may be changed too, where the heir is unqualified for the station. Great deference is always paid to the will of the dying chief, but every such case is always laid before a full national council, whose decision is final (p. 97).

A chief may be degraded from his rank for bad conduct. . . .

There is in every tribe, what answers to a standing army among us. The profession of arms holds out, to the great mass of the common people, the only road to the temple of fame. . . . The young man, who aspires to the honorable distinction of a "brave" or warrior, must exhibit such traits of character, as are deemed necessary for a soldier to possess, before he can be admitted into the army. When admitted, he wears on his head just as many feathers of the bald eagle, as he has slain human beings, and the size of the feathers indicate the size of his victims. . . . If the warrior has taken a captive, he has a human hand as large as life, painted either on his face, or on some part of his body, or on his blanket. some individuals have several such hands painted upon them (p. 98).<sup>9</sup>

At the head of the army, belonging to each tribe, there is a person who occupies the same station as a General does with us, and he appoints all the inferior officers. The chiefs when met in council, call into it, their warriors, with whom they consult, but frequently they are called to receive the orders, which are obeyed to the letter. This council call before them, persons who are interested in the trial . . . (pp. 98, 99).

If the Senate of the United States is the mildest, the most patriotic and wisest legislative assembly in the civilized world, as it truly is, the Winnebago council, is decidedly at the head of the savage world. . . . The great body of the people have very little influence, almost none, with this council, and they never appear before it unless they are summoned to attend it.—They have no voice in electing the chiefs and in fact no political influence. The civil chiefs and the chief warriors, have in their hands, the whole government of the community, and they govern as they please. Disobedience to the orders of the rulers is punished with death, though, like the British nation, the Indians are not savages enough to cut the traitor into quarters after hanging him until he is dead (pp. 99, 100).

The female part of it, exercised the same influence among the women, that the chiefs did among the men, and the presents they expected from us, had to be better, than those given to the common people. The daughter of a chief, never mar-

<sup>9</sup> Quite different from the data given by Radin, *The Winnebago Tribe*, pp. 161, 162.

ries into a family below her's in dignity. The pride originating in birth, is as deeply seated in the hearts of those who are nobly descended, among the natives of the Northwest, as it is among the petty princes of Germany.

It is customary for the chiefs to appoint two soldiers, in each village to keep order in it, and they faithfully do so (p. 100).

In each tribe, some one man acts as a divider, by order of the civil chiefs, and by general consent. Whenever we made the men any presents, of pipes, paints, tobacco, or any thing else, the self same men, if Winnebagoes were the Indians receiving the presents, appeared, took charge of the property, and divided it in the most equitable manner, among all present at the time. They reserved nothing for themselves, generally, but held up their empty hands to show their disinterestedness.

If the Sauks and Foxes received presents, two men, one for each tribe, acted as dividers, and the same individuals always performed the same duty (pp. 100-101).

... "The rolling Thunder," "the Yellow Thunder," "the Distant Thunder," &c., are individuals among the Winnebagoes, who possess great weight of character; and that tribe to which belong men, who stand high on their roll of fame, for their distinguished valor in War, for their consummate prudence, experience and wisdom in the national councils, is emphatically called, "the thunder tribe" (p. 124).

There are other items appurtenant to Winnebago ethnology in Atwater's work; some to that of the Sauks and Foxes; some to tribes of the upper Mississippi valley in general: there are some acute statements regarding the chronology of the populating of America.

A "turtle" gens does not occur in the lists of Morgan, Dorsey, and Radin, and Radin specifically denies that there is such a Winnebago gens. He also states that almost all informants claimed that the Snake and Fish clans (*gentes*) were recent to the tribe. However, as special names "go with" fixed *gentes*, by examining the lists of the signers of the treaties between the Winnebago and the United States, it becomes clear that a member of the Snake gens signed the treaty of August 19th, 1825 as well as the treaty of August 25th, 1828. And a Snake clan exists among the Ponca, Iowa, Oto-Missouri, Quapaw, as well as some non-Siouan tribes.<sup>10</sup> Also it may be observed that among the Osage and Quapaw we have a Fish clan (used rather loosely). So too "Turtle" as the name of a gens (sub-gens) among the Siouan tribes Omaha, Kansa, Quapaw, and Osage,<sup>11</sup> and elsewhere. And Carrymaunee (Karamanu, etc.), "Walking Turtle,"<sup>12</sup> signed more

<sup>10</sup> See the important theoretical discussion by Lowie in *American Anthropologist*, Vol. 36, 1934, pp. 327-28; cf. also Radin, *The Winnebago Tribe*, p. 182; Fletcher and La Flesche, *The Omaha Tribe* (Twenty-seventh Annual Report, Bureau of American Ethnology), pp. 38, 39.

<sup>11</sup> Among the Omaha and Osage "the turtle carriers or bearers" and "Turtle-carriers" respectively; "Carries-a-turtle-on-his-back" among the Kansa. observe that according to the above Carrymaunee "carried" a large painted tortoise on his back.

<sup>12</sup> The phonetic restoration is presumably Kéramani; mani means "to walk," as can be seen from old vocabularies, etc., as well as the personal names given by Radin in his *The Winnebago Tribe* and the Winnebago text by Radin in the Siouan sketch in *Handbook of Ameri-*

than one treaty with the United States. Similarly the lists of signers of the treaties of June 3rd, 1816, August 19th, 1825, etc., between the Winnebago and the United States indicate the existence of gentes not in the published lists, but which have analogues among other Siouan tribes. I feel certain that a study of the names of the signers of treaties between United States and such Siouan tribes as have distinctive clan-names (and similar non-Siouan tribes) would yield valuable results. And a study of the personal names of such tribes in older writers would also pay. In this connection I may say that from Atwater's book, using this technique, it can be readily determined that the Eagle and Elk gentes were in existence among the Winnebago in addition to those specifically mentioned by him. The treaties furnish a few others, e.g., Earth, Sky. In this connection I may say that it is clear from the names of the signers of the treaty of May 13th, 1816 that at least the following Sauk gentes were in existence: Sturgeon, Eagle, Thunder, Bear, Swan, Wolf, Fox, Partridge, Sea. It is not without interest to note that all these save Partridge and Fox are on Forsyth's list of 1827.<sup>13</sup> If we count Partridge as the equivalent of Grouse, all occur in Jones' list printed in the Handbook of American Indians in the article *Sauk*. Sturgeon, Swan, and Partridge are not in Skinner's list. Sturgeon, Eagle, Sea (= Great Water), Bear, Thunder, Partridge (= Pheasant), Wolf, and Fox occur in Galland's list. Wolf, Bear, Eagle, Sturgeon, Fox, Sea, and Thunder occur in Morgan's list. If we pool M. R. Harrington's two lists we find Thunder, Fox, Wolf, Bear, Swan, Sturgeon, Eagle on them. A knowledge of the names on the tribal rolls from the earliest times would doubtless shed much light on the subject.

It should be mentioned that Radin argues for a priority of village groups, but states that according to all informants each Winnebago village was inhabited by members of various clans, and that in no case did a village merely include members of the same clan. Observe the contradiction with Atwater's data.

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can Indian Languages (Bulletin, Bureau of American Ethnology, 40, Pt. 1). There are equivalents in several Siouan languages. In the Siouan sketch Radin gives *ke'c'ú'ngēgā* as a personal name, "the tortoise," and *ke'c'ú'ngēra* as "you, tortoise," and in note 38 of the Winnebago text we are told that *ke'c'ú'ñk* or *ke'c'ú'ngē* is a large species of turtles, and that *kē* also means "turtle." La Flesche gives *ke* (in my transcription) as the Osage for "turtle, tortoise, terrapin," but Dorsay gives *k'e* (in my transcription) for both Osage and Quapaw, but *ke* (in my transcription) for Omaha; *k'eya*, "tortoise," is given by Deloria for Dakota. I am not sufficiently acquainted with the formation of personal names in Winnebago to be positive whether *ra* is the pronoun "thou" (vocative; see notes 39 and 46 to Radin's Winnebago text) or whether *kera* may be an archaic correspondent to Dakota *k'eya* with the recognized shift of *y* to *r*.

<sup>13</sup> They are in his Fox list.

THE discussion of whether or not marriage is reflected in kinship terminology has been going on since the time of Lewis H. Morgan. One way of approaching the problem is to logically construct theoretical kinship systems on the basis of types of marriage.

Let us assume that we have a case where sororate marriage and the marriage of a man to his wife's brother's daughter function together. From these marriage practices we can construct a theoretical kinship system. Since a man may marry his wife's sister and his wife's brother's daughter (then he may marry the wife's brother's daughter of his secondary wife, that is, his wife's brother's son's daughter), he is able to equate these women terminologically and call them something like "substitute wife" or "women whom I may marry." The brothers of these women will be called "brothers of my substitute wives." Thus this man has equated terminologically his wife's sisters, wife's brothers' daughters and wife's brothers' sons' daughters; he has also equated terminologically his wife's brothers, wife's brothers' sons and wife's brothers' sons' sons. In this way all of the wife's brothers' *male descendants through males* are equated terminologically, and all of the wife's brothers' *female descendants through males* are also equated. By following his father's identifications, a child of this marriage will terminologically equate his mother's sisters, mother's brothers' daughters and mother's brothers' sons' daughters, and call them something like "substitute mother;" the child will also equate his mother's brothers, mother's brothers' sons and mother's brothers' sons' sons, calling them something like "brothers of my substitute mothers;" that is, all of the mother's brothers' *male descendants through males* and all of the mother's brothers' *female descendants through males* are equated, according to sex. Furthermore, this child will call his father's sister's husband and his sister's husband by the same term because his father's sister's husband, by marrying a wife's brother's daughter, is marrying this child's sister. The children of his father's sister would be possible step-children to his sister, and he would equate them. Thus the cross cousins are arranged so that his matrilineal cross cousins, his mother's brothers' children, are raised a generation; and his patrilineal cross cousins, his father's sisters' children, are lowered a generation.

If we have another case, where levirate marriage and the marriage of a woman to her husband's sister's son (a man to his mother's brother's wife)

<sup>1</sup> Read at the joint session of the American Anthropological Association and the American Folk-Lore Society, Pittsburgh, December 27, 1934.



are practiced simultaneously by a people, we find a parallel, but reversed condition. Since a woman may marry her husband's brother and her husband's sister's son (then she may marry the husband's sister's son of her secondary husband, that is, her husband's sister's daughter's son), she is able to equate them terminologically and call them something like "substitute husband" or "men whom I may marry." The sisters of these men will be "sisters of my substitute husbands." Thus this woman has identified her husband's brothers, husband's sisters' sons and husband's sisters' daughters' sons; and she has identified her husband's sisters, husband's sisters' daughters and husband's sisters' daughters' daughters. In this manner all of the husband's sisters' *female descendants through females*, and all of the husband's sisters' *male descendants through females* are equated, according to sex. A child of this marriage will, by following his mother's identifications, equate terminologically his father's brothers, father's sisters' sons and father's sisters' daughters' sons, and call them something like "substitute fathers;" the child will also equate his father's sisters, father's sisters' daughters and father's sisters' daughters' daughters, and call them something like "sisters of my substitute fathers." Thus all of the father's sisters' *female descendants through females*, and all of the father's sisters' *male descendants through females*, are equated according to sex. Furthermore this child will call his mother's brother's wife and his brother's wife by the same term because his mother's brother's wife, by marrying her husband's sister's son, is marrying his brother. The children of his mother's brother would be possible step-children to his brother, and so he would equate them with his brother's children. Thus the cross cousins are arranged so that his matrilineal cross cousins, his mother's brothers' children, are lowered a generation, and his patrilineal cross cousins, his father's sisters' children, are raised a generation.

Suppose we construct a theoretical kinship system on the basis of the two preceding. We would have the marriage of a man to his wife's sisters, wife's brothers' daughters and wife's brothers' sons' daughters (the wife's brothers' daughters of his wife's brothers' daughters). A woman would be able to marry her husband's brothers, husband's sisters' sons and husband's sisters' daughters' sons (the husband's sisters' sons of her husband's sisters' sons). Thus a man would equate all *male descendants through males* and all *female descendants through males* of his affinal family. A woman would equate all *female descendants through females* and all *male descendants through females* of her affinal family. Now we have placed the child in a position where he may equate terminologically his mother's brothers' children with his mother's siblings, as he does in the first example, or he may

equate his mother's brothers' children with his brothers' children, as he does in the second example. On the other hand, this child may take over the identifications of his mother, as he does in the second example, and equate his father's sisters' children with his father's siblings, or equate his father's sisters' children with his sisters' children.

Which will he take over—his mother's or his father's identifications? His parents do not have this problem because the terms which they use for their own family members are different from the terms which they use for members of their affinal families (there are two persons using these terms and there are two sets of terms used); but a child (one person) has the possibility of doing a number of things.

1. He may take over all of his father's identifications. In doing this he could equate the members of his mother's family as his father does, and identify the members of his father's family as his father does also. Marriage would be reflected only in the terms used for the mother's relatives.

2. He may take over all of his mother's identifications. In doing this he could equate the members of his father's family as his mother does, and identify the members of his mother's family as his mother does also. Marriage would be reflected only in the terms a person used for relatives on the father's side.

3. He could equate his father's relatives as his mother does and his mother's relatives as his father does. Here, marriage would be reflected in all the terms, but not all marriages would be reflected.

4. He could equate his father's relatives as his father does and his mother's relatives as his mother does. This would give us a kinship system where the cross cousins did not reflect any marriage customs.

5. Another possibility would be for this child to use terms which had no relation to any which his parents used.<sup>2</sup>

6. He could take over both sets of identifications and call certain relatives by two or more terms.

While each community may select its own pattern, the people who actually become married have these terms for their affinal relatives. Affinal relatives of one generation become consanguineal relatives of the next generation.

We may say that mechanically there are four possibilities: 1. Taking over the father's identifications, and not the mother's. 2. Taking over the mother's identifications, and not the father's. 3. Taking over parts of both

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<sup>2</sup> A man may use terms based upon his possible marriage to his mother's brothers' wives, a woman on the possible marriage to her father's sisters' husbands, or a man or woman on the possible marriage of his or her siblings.

identifications. 4. Taking over neither the mother's nor the father's identifications.

Thus we see that in wife's brother's daughter marriage or in husband's sister's son marriage, we may not have the marriages reflected at all in the terminology which a person uses for the members of his mother's and father's families, but a reflection of the terms which are based upon the biological connections within the family; that is, the taking over of both parents' identifications for their own families.

From the above we can see that systems basically the same may be classified into different categories if only the cross cousin terminology, lineages and terms for the first ascending generation are used. In a kinship system the important terms which may reflect sociological conditions are those used for one's own consanguineal and affinal relatives on the same generation level as the speaker and their descendants. If we use the terms of the first ascending generation for classifying kinship systems we may be misled because it is absolutely impossible, where there is more than one type of marriage practiced, for the terms to reflect the marriages in all particulars, while the terms used for the members of a person's own affinal family are able to reflect all the marriage practices.

Some of the questions which naturally arise from this discussion are:

1. Has the interrelationship expressed above been found in any society?
2. Have these four forms of marriage been found functioning together?
3. If so, were the marriages reflected in the kinship terminology?

Last summer I was sent to California by Columbia University to investigate the kinship and sociology of the Pomo Indians. They practiced the marriage of a man to his wife's sister (sororate) and wife's brother's daughter, and of a woman to her husband's brother (levirate) and husband's sister's son simultaneously, and the marriages are reflected in the terminology to a surprising degree.<sup>3</sup>

The kinship systems of the Pomo Indians were reported by E. W. Gifford in *Californian Kinship Terminologies*. When Mr Gifford collected the kinship systems of the Pomo he was interested primarily in obtaining the terminology and not the marriage practices and sociology of the people. At the time I was with the Pomo, Mr Gifford was also there, working on material and other phases of culture. We both found that the

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<sup>3</sup> These people also practiced exchange, modified group, polyandry and other forms of marriage, all of which are reflected in the terminology, but do not interfere with the reflection of the marriages under discussion.

Pomo were divided into valley groups, each valley being an autonomous unit; and that although all of the Pomo were fundamentally a group having many traits in common, each valley had its own variants.<sup>4</sup>

I have selected the kinship systems of three valleys (Hopland, Ukiah, and Rincon) to illustrate three of the possibilities in taking over of identifications from generation to generation.

The Hopland valley people have what has been called an Omaha type system.<sup>5</sup> The Hopland women equate their husband's brothers, husband's sisters' sons and husband's sisters' daughters' sons terminologically. They also equate their husband's sisters, husband's sisters' daughters and husband's sisters' daughters' daughters. The Hopland men equate their wife's brothers, wife's brothers' sons and wife's brothers' sons' sons; their wife's sisters, wife's brothers' daughters and wife's brothers' sons' daughters. The terminology used for members of the first ascending generation reflects the four marriages in part. Father's sister's husband is called by the same term used for sister's husband. Mother's brother's wife is called by the same term as brother's wife. The cross cousins follow the reflection of the wife's brother's daughter marriage; that is, the matrilineal cross cousins are called mother's siblings, and the patrilineal cross cousins are called sister's children.

The Ukiah valley people have what has been called a Dakota type system. They identify the members of their affinal families exactly as the Hopland people do. But their children call their cross cousins by sibling terms, taking over the biological (own family) identifications of their parents, and not the sociological, since their parents call their own siblings' children as they do their own children. If the child called these cross cousins by uncle-aunt or nephew-niece terms as the Hopland people do, he would be taking over the sociological identifications. The mother's brother's

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<sup>4</sup> The Hopland valley people speak the Central Pomo dialect, the Ukiah valley people the Northern Pomo dialect, and the Rincon valley people the Southern Pomo dialect.

"Seven principal dialects of the Pomo language are distinguishable. Perhaps these should rather be called languages, since their differences seem approximately commensurate with those of the Romance tongues, though somewhat other in kind" (A. L. Kroeber, *Handbook of the Indians of California*, Bulletin, Bureau of American Ethnology, 78, 1925, p. 226).

<sup>5</sup> The problem of *marriage and kinship terminology* has been considered by practically everyone interested in kinship. Kinship systems have been classified and categorized on the basis of various parts of the systems by many persons. Some authors have tried to tie the classification up with a dichotomous society, some with marriage, some with the sib, and so on. It would lengthen this paper too much to acknowledge the author's indebtedness to and departures from the previous writers on the subject. A short bibliography is given at the end of this paper for reference.

wife is called by the same term as the brother's wife; the father's sister's husband is called by the same term as the sister's husband.

The Rincon valley people have what has been called a Crow type system. They identify the members of their affinal families as do the Ukiah and Hopland people. But a child of a marriage calls his matrilineal cross cousins by the terms he uses for his brothers' children, and calls his father's sisters' children by terms he uses for his father's siblings. Again, the terms used for mother's brother's wife and father's sister's husband reflect the wife's brother's daughter and husband's sister's son marriages.

Thus we see that in all three cases my mother equates my father's sisters' children with my father's siblings, and my father equates my mother's brothers' children with my mother's siblings, but I do not follow them. In the Hopland case, I equate my father's sisters' children with my sisters' children, and my mother's brothers' children with my mother's siblings. In the Ukiah case I equate my father's sisters' children and my mother's brothers' children with my siblings, taking over the identifications of my mother for her own siblings' children whom she calls by the same terms she uses for my brother and sister, and my father's identifications for the children of his siblings whom he calls by the same terms he uses for my brother and sister. (My father and mother call all children of members of their generation by the same terms.) In the Rincon case I equate my father's sisters' children with my father's siblings, and my mother's brothers' children with my brothers' children.

Here we have the Hopland people with an Omaha type system, the Rincon people with a Crow type system, and the Ukiah people with a Dakota type system, when all three are basically the same, being based upon the same forms of marriage and containing approximately ninety-five percent of the same terminological equations in their entire kinship systems. The only difference is the cross cousin terminology, and the descendants of the cross cousins who follow a linear pattern in two of the cases.

Since it is possible to take over the identifications of both parents for their own families, only a few of the actual conditions may be reflected in the terminology a person uses for his uncles and aunts, and so a classification on the basis of the terminology used for members of the first ascending generation may also prove insufficient in the categorization of kinship systems.

Since the marriage of a man to his wife's sister and his wife's brother's daughter may cause an alignment of relatives similar to that in which the marriage of a man to his wife's sister and wife's brother's daughter,

and a woman to her husband's brother and husband's sister's son to occur, it is also possible to place in the same category two or more kinship systems based upon different combinations of marriage practices. In the terminology used for the cross cousins and their descendants, or for the uncles and aunts and their spouses, none, or only one of the marriages may be reflected.

From these examples we may venture some generalizations. It seems to me that the approach to kinship should be first by a study of the terminology used by a person for members of his affinal family. Then we must determine whether or not the terminology reflects the marriages practiced by the particular group of people; then see which identifications, if any, are taken over by the children of a marriage, i.e., by members of the next lower generation. Then, if the identifications are taken over, the next step is to find out why the choice has been made. Here there must be some influence which has caused the choice to be what it is. It may be that the marriage of men is of more importance in the community, but why is that marriage more important? It may be due to religion, economics, residence, inheritance, borrowing of kinship patterns from another people, or to any other reason.

We have seen that it is impossible to have all the marriage practices reflected in the terminology unless more than one term is used for certain relatives.

After finding that there was a selection in the identifications passed on from generation to generation among the many Pomo valley groups, I tested the principle with the Hupa and Yuki to the north and the Chukchansi (Yokuts) to the south. I found it occurred with them also. In all the cases I investigated the terminology showed a selection of identifications. The people, when questioned as to why they used certain terms for definite relatives said, "Because my mother calls them so and so" or "Because my father calls them by such and such terms." When I said "Your other parent calls them by other terms," they answered "I follow my mother and not my father," or vice versa. Thus we have empirical substantiation of the mechanical principle arrived at deductively.

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SLIT TAPESTRY FROM THE  
UPPER SALT RIVER VALLEY, ARIZONA    By CHARLIE R. STEEN

**D**URING the summer of 1931 several rooms of the north cliff dwelling at Tonto National Monument, in the upper Salt River Valley, were excavated. Several pieces of cotton fabric were recovered, among which was a portion of a head band for a tump line. The weaving of this strap is the type known as slit tapestry, and is interesting because of its rarity in the southwestern United States.



FIG. 1. End of a head band for a tump line, with slit tapestry weave (ca. 2 3).

The fragment has a cotton weft over a yucca fibre warp and is woven in white, red, and brown. The maximum length of the piece is 11.5 cm., maximum width 4.0 cm.; the thickness through the body is 0.3 cm., and thickness at the end 0.7 cm.

The yucca fibre was twisted into a thick, even cord, and the weft coiled back and forth over this foundation. At the end of the band the warp cords were gathered into four bundles and paired, these pairs being braided together to form a loop which was probably used for fastening the load cords.

The peculiarity of the fragment, i.e., the slit tapestry weave, occurs in the division of colors. Instead of the conventional interlocking of weft strings of different colors, the string was carried over two warp cords in a simple figure-eight three times, then dropped a cord and the process re-



peated. This method of separating colors forms a natural stepped or terraced design with the triangle and rectangular block forming the principal elements. A major slit occurring three and a half centimeters from the end of the band gives a false impression of being a second tying loop: this, however, forms a line of demarcation between a block of brown weft and the white.

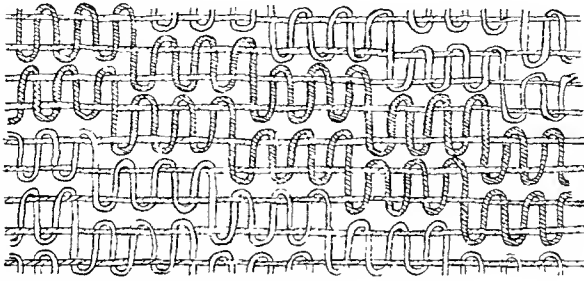


FIG. 2. Detail of slit tapestry weave.

Since coming into possession of this fragment I have seen a photograph of a second piece of slit tapestry taken, I believe, from a ruin on the middle Verde River, Arizona. To my knowledge no other example of this type of weaving has been reported from the southwestern part of the United States.

ROOSEVELT, ARIZONA

THE JOKING RELATIONSHIP AND ORGANIZED  
OBSCENITY IN NORTH QUEENSLAND<sup>1</sup> By DONALD F. THOMSON

AMONG the native tribes of Cape York Peninsula, North Queensland, the use of swearing and obscenity is of frequent occurrence and plays an important part in social life. In each of the tribes discussed in the present paper there is a well defined joking relationship: by which is understood an organized type of behavior between relatives of a certain order in which it is customary, even obligatory, to make use of, or to exchange, obscenities of a set and restricted nature, generally in public. As far as I am aware, the existence of this organized obscenity, which is well known in African, Melanesian, and many other primitive societies, has not been recorded hitherto in Australia.

Cape York Peninsula, by reason of its position on the threshold between Australia and Papua, is a region of great ethnological interest. It is a transitional area in which the typical Australian culture and social organization, although still dominant, have been modified by infiltrations of Papuan culture; especially by virile hero cults with masked dancers,<sup>2</sup> that have entered by way of Torres Straits and swept down both coasts of the Peninsula.

THE TRIBES OF CAPE YORK PENINSULA

The territories of the native tribes discussed in the present paper lie in tropical Queensland, north of the 15th parallel of latitude. This region is occupied by a great many tribes—about forty in number—of which the most important are shown on the map, Figure 1. Although only five representative tribes will be discussed here, the joking relationship is found throughout the area.

Physically, culturally, and in their intellectual equipment, the natives of Cape York Peninsula are of a fine and outstanding type. In their social organization and their material culture they differ in important respects from the typical Australian aboriginal of the inland country. The people of the coast tribes are fishermen, and particularly on the east coast, many of them are seafarers, building seagoing dugout canoes, generally fitted with two outriggers, in which they make voyages far out to the reefs. In

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<sup>1</sup> The expedition to North Queensland in 1932-33, on which this work was carried out, was financed by a grant from the University of Melbourne.

<sup>2</sup> See Donald F. Thomson, *The Hero Cult, Initiation and Totemism on Cape York* (Journal, Royal Anthropological Institute, Vol. 63, 1933, pp. 453-537), and *Notes on a Hero Cult from the Gulf of Carpentaria, North Queensland* (Journal, Royal Anthropological Institute, in press).



typical of the Australian mainland aboriginal. A glance at the map will show that these bush tribes, such as the Wik Monkan<sup>3</sup> of the Archer River district, the Kanju of the central range country, the Koko Ai'ebadu whose country lies to the south of the Kanju on the headwaters of the Holroyd River, and the Olkolo, whose territory extends from the Alice River to the Upper Coleman, generally occupy much larger territories than the seafaring peoples of the coast. This does not necessarily mean that they are numerically stronger than the sandbeach tribes (although in the case of the Wik Monkan and Kanju tribes it is so). Among the seafaring people who obtain much of their food from the sea, the density of the population per square mile is much greater than in the bush tribes where the natives are living as hunters and collectors and where the individual clan territories must therefore be greater in extent.

It was noted above that the Wik Monkan and the Kanju tribes were numerically stronger than their neighbors, and in fact they are the largest tribes in the entire region. Again, this does not mean that these tribes are dominant: for in this region, as in other parts of Australia, the tribe is generally merely a linguistic unity, and is not the war making group. The localized totemic clans with patrilineal descent, that form the basis of the organization of these tribes, were self-governing units, and the fighting that took place intermittently, generally having its origin in blood feuds, or in expeditions organized for the purpose of capturing women, were carried out, not as a rule by the tribe, but by its component clans, or more accurately, by its hordes<sup>4</sup> acting independently, sometimes among them-

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<sup>3</sup> The system employed for the transliteration of vowel sounds is:

a as in "father"	o as in "no" in open	i as in "feet"
a as in "come"	syllables and "not"	u as in "fool"
ɜ as in "cat"	in close syllables	ɪ as in "fit"
e as in "fetch"	as in English.	ʊ as in "full"
é as in "fate"	ɔ as in "nor"	

<sup>4</sup> A clan, in a patrilineal society, consists of a man and all his relatives in the male line, that is his father, his father's brothers and sisters, his own brothers and sisters, and his sons and daughters, and all the children of the male members of the clan only. All these men, however, marry women who are members of other clans, and again their sisters and daughters marry men of other clans, so that the group of people popularly spoken of as a "camp," that is found at any time within a clan territory, really consists of members of many clans, and for this group the word horde will be used. Thus a horde consists of all the male members of the clan whose territory it inhabits, with their wives, who, though they are members of the horde, are not members of the clan (since entry to a clan is by birth alone), and less those women of the clan who have married into other hordes. But while they may change their hordes by marriage, they can never change their clans. It is clear, therefore, that although the horde is the war-making group, the clan, and not the horde, is the land-owning group; a clan is a

selves, at others against hordes of neighboring tribes. Clan totemism and other elements of social life tended to maintain the solidarity of these clans at such a level that a minimum of contact took place between distant clans even of the same tribe, and therefore, when the territory occupied by a tribe was extensive, the clans at one side of the tribal territory frequently had more in common with the clans of neighboring tribes, than with those of their own tribe situated on the far side of the territory. Thus in the Wik Monkan tribe the clans impinging upon the territory of the Kanju, had much more in common with the members of the Kanju clans with whom they came into contact almost daily, than with members of their own tribe who belonged to distant clans localized on the lower Archer River.

Government in these groups is vested in a loosely organized council of old men, and sometimes a man may, by virtue of exceptional experience, especially in matters of initiation or on account of special prowess in fighting and hunting, carry more weight in the deliberations of these old men, but here, as elsewhere in Australia, there is nothing approaching chieftainship. It is necessary to stress this point since references to a Mamoose appear frequently in the accounts of early travellers, and the term is much used to-day on certain mission stations on Cape York Peninsula. Nevertheless the very name is foreign: an unhappy importation from the islands of Torres Straits, where according to Haddon<sup>5</sup> it owes its origin to one of the misinterpretations of native words that are the peculiar gift of the white man.

Most of the tribes of Cape York Peninsula are divided into two exogamous moieties, but the class system found in most other parts of Australia is conspicuous by its absence. In the Kōkō Ya'ō and Ompela the moieties are called Kōiyān and Karpéya; in the Kanju and Kōkō Ai'ebadu tribes the word is slightly altered and becomes Kōiya and Karpi. In the Wik

stable, permanent, structural unit of society; but the horde is unstable; it is a sociological entity the membership of which is constantly changing. I do not propose to go more fully into this subject here, but brief mention must be made of the bond that unites members of the two groups. It may be noted that solidarity within the clan is maintained by the bond furnished by (1) common descent, (2) the possession of common totems, (3) the possession of a common territory. Solidarity within the horde rests upon none of these permanent foundations. It depends solely upon the cohesive force supplied by such social institutions as marriage and the bond set up between a man and a woman (who are members of different clans) by the family, centered in their children, and by the sharing of normal activities of everyday life, by fighting with other hordes—in all of which the bond of solidarity within the horde is affirmed and strengthened by collective ceremonies such as dancing, especially war, funeral, and vengeance dances.

<sup>5</sup> A. C. Haddon, Reports, Cambridge Anthropological Expedition to Torres Straits, Vol 5, 1904, p. 266.

Monkan tribe, however, there are no moieties, although these appear again in the tribes immediately to the south, and thence occur right down to the Mitchell River.

The kinship system is of the classificatory type, and in many of the tribes a man marries an "outside" cross cousin, i.e., a classificatory mother's brother's daughter, with whom no blood relationship can be traced. That is, he marries a woman who is not a blood relation but who stands in the same relation to him as an actual mother's brother's daughter. Several other types of marriage are practiced however, and as this, too, is out of the scope of the present paper, a brief note only on the marriage system will be given in speaking of the joking relationship of the individual tribes.

All the tribes on the Peninsula are patrilineal, tracing their descent in the moiety and clan through the father. In the Kōkō Ya'o, Ompela, Yint-jingga and Kanju tribes there is also a curious form of personal totemism, which is entirely absent from the Wik Monkan. This personal totemism is anomalous, for although these tribes are patrilineal in all other institutions, the clan totems being taken from the father, the personal totem always comes from the mother's moiety and sometimes even from her clan, by divination, at the rite of tooth avulsion. Tooth avulsion (the removal of the right or left upper central incisor) which takes place in both sexes after puberty, is not an initiation rite, but is associated with dream life and life after death.

Finally, in order that significance of the allusions discussed in this study of organized obscenity be understood, it must be stressed that the members of most, if not all, of these tribes, are aware of the fact of physiological paternity; that is, they are aware that sexual intercourse and pregnancy are related. In another communication<sup>6</sup> I have adduced the evidence for this statement at some length as far as the Kōkō Ya'o, the Ompela, the Kanju and allied tribes are concerned, and I have obtained subsequently from the Wik Monkan tribe a mass of evidence still more striking, that will be published in due course.<sup>6a</sup>

In former times Cape York Peninsula supported a very dense native population, but today the members of all the tribes mentioned are greatly reduced; a number of tribes are already extinct, and a few are represented by one or two members only.<sup>7</sup> The policy of gathering these people into

<sup>6</sup> See *The Hero Cult, Initiation and Totemism on Cape York Peninsula*, pp. 505-10; this paper also contains an account of the rite of tooth avulsion and the belief in a life after death.

<sup>6a</sup> *The Father in the Wik Monkan Tribe*. ms.

<sup>7</sup> E.g. the Wik Tinda of the Archer Bay district and the Addedjin'nit and Latum'nit whose territories lie to the northward of the mouth of the Archer River.

camps on mission stations, or in the vicinity of white camps and settlements, of working them on cattle stations, and of recruiting a very large proportion of the young and able bodied men each year from camps of the coast tribes to work on trochus and bêche-de-mer vessels operating from Thursday Island, has proved fatal, and the natives are today dying out at an appalling rate. A few institutions have shown in their official figures an increase in the natives under their charge, but these are due to the bringing in of fresh individuals from the bush, and not to an actual increase in the population.

#### SWEARING AND ITS PLACE IN NORTH QUEENSLAND SOCIETY

Although the licensed swearing and obscenity permissible or obligatory under the joking relationship is the only swearing that falls under a social sanction, it forms but a small part of the swearing that is heard in a native camp, and is by no means the only bad language or obscenity that plays a part in native sociology.

Except in the presence of relatives of certain orders to whom it is *kintja* (tabu) to speak, or with whom it is obligatory to exercise restraint, or to whom deference must be shown (which include all those to whom the kinship terms are extended under the classificatory system) there is no restriction upon reference to the *genitalia* or to the physiological functions of reproduction, defecation and micturition, and references to these normal phenomena occur in many myths and legends which may be repeated at mixed gatherings, and even before children. Children grow up with no inhibitions in these matters, except towards certain relatives to whom any mention of these things would be incompatible with the set pattern of behavior. In a camp of the *Yintjingga* tribe on the estuary of the Stewart River, a child about two years of age that was being suckled at its mother's breast, dropped the nipple to glower at me and exclaim in the *Ompela* language of its mother, *Awu! kuna kätta! kuna kätta!*, "Devil! excrement foul! excrement foul!" I did not understand the language at that time, but nobody expressed concern or corrected the child.<sup>8</sup> When a man swears, it is not a question of what he says so much as to whom he says it, and an offense against etiquette, against the customary norm of behaviour, even if unintentional, as in a *lapsus linguae* or a *faux pas*, in the presence of a *kintja* relation, necessitates ritual purification (*vide infra*, page 468).

<sup>8</sup> In the *Wik Monkan* tribe however, there is a special expression, which is regarded as a stigma, for one who *habitually* uses foul or obscene language, as against the ordinary word used for swearing, and in the *Wik Monkan* tribe a child is corrected for using bad language; see page 470.

The behavior that is obligatory not only towards certain relatives, but sometimes, even in their presence, amounts in certain instances to the use of an almost completely different language that must be spoken in an unnatural voice, and *at*, rather than *to* the person really addressed, i.e., to a third person, generally a child, and sometimes to a dog, within the hearing of the first. This behavior becomes extremely complicated, and since it is inseparably linked with the kinship organization, a full discussion is outside the scope of the present paper: a short note and a number of words for comparative purposes will be given later (page 481).

It should be noted, however, that there is a very definite etiquette in these matters that is rigorously observed. In most of these languages there are at least two, often a number, of words for each object, or for parts of the body (in addition to the special and restricted language mentioned above). One of these words is generally considered to be the proper term to be used in ordinary polite conversation, the other is, in the words of my informant, "half swear." In the Koko Ya'o language the word *tykin* (copulated) occurred in a myth that I was writing down. It was this word that my informant characterized as "half swear," i.e., having the force of raped. The more polite or more genteel word is *matjin* (seize, catch hold), i.e., a euphemism. Similarly in the Wjk Monkan tribe the word for copulate is *etjin*, and *etj'wun*<sup>9</sup> is copulation, sexual intercourse, literally "lie down and copulate," and while I have heard this word used frequently, even by women, in recounting myths, it would be a breach of good taste to employ it in ordinary conversation. Between husband and wife such an expression is gross or "rough" as my informant expressed it: a man who wishes to have intercourse with his wife says simply *ɳäll känwi*, an idiomatic expression the literal translation of which gives no indication of its real meaning (i.e., *ɳäll*, you and I; we two [dual person], *känwi*, stop, leave off). Apart from its special use as a euphemism in this expression, *känwi*! used alone is a mild and jocular way of saying "don't" or "stop it!" to a person who playfully catches hold of one, and is normally in itself an absolutely innocuous word. A stronger word than *känwi* in the ordinary, non-idiomatic, sense is *kana*, "stop," "finish," implying a definite request to desist. The natives of these tribes place great importance upon the observance of etiquette, and considerable significance is attached to what we should regard as a trifling shade of difference in the meaning of the words used, and even to the tone of voice, in gauging the "atmosphere" of a group or the attitude

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<sup>9</sup> *Wun* is lie, lie down, hence *etj'wun* is copulate, *werp* is sleep; hence to lie down and sleep is *werp wun*.



of an individual. To express many of these feelings the natives often employ words that have no exact counterpart in English.

An appreciation of the importance of this etiquette will contribute much to an understanding of the facts here presented, and will help to show that the norm of behavior can be dictated both by a nice reserve and by a sense of "good taste" that we are always apt to claim as our special prerogative. Before leaving the subject I shall cite an example from the Tjuṇundji tribe that will show how easy it is for a white man to make a faux pas. In the language of this tribe a woman is *n'dwämrä*, and she is spoken of as such as long as she is not close at hand, when the term *mätäṇamaräṇo*, "yam stick belonging to," *mätäṇa*, yam stick, *maräṇo* belonging to) is used instead. *N'dwämra* has really the force of "a female," i.e., as my informants have it, a "woman one." The euphemism "the bearer of the yam stick," the gatherer of the food, is the correct term in "polite" conversation.

When an aboriginal of any one of the tribes discussed below, inadvertently strikes his toe against a stump or root he does not break out with an oath or obscene expression, but calls upon the name of a relative long deceased. Similarly, when a weapon upon which he is working, or a canoe lashing, breaks, he calls upon the name of a dead relation whose name has outlived the *kintja* (tabu) period following death. An aboriginal of the Koko Ya'o tribe who was closely associated with me for many months, and with whom I could converse in his language, invariably exclaimed *Tjäkkamyl!*—the personal name of a woman who died a very long time before and who stood in the relation of *ṇämi* (mother's elder brother's daughter) to him. When a canoe lashing that he was tying parted in his hands he cried *Tjäkkamyl tankin!* or *Tjäkkamyl naiyen*, "Tjäkkamyl, (it is) broken!" And when a nail that he was hammering doubled up, he exclaimed *Tjäkkamyl toiyi!*, "Tjäkkamyl, (it is) crooked (bent)."

This practice of calling upon the name of a deceased relation, a mild and innocuous oath, is general among the native tribes of Cape York Peninsula, and is found in tribes in widely separated localities. Thus, in the Tjuṇundji tribe of the lower Batavia River, that differs greatly from the tribes of the eastern seaboard, and also from the Wik Monkan and other peoples to the southward, the same custom prevails. An old Tjuṇundji man who was almost constantly with me when I was in this area, frequently exclaimed *Yändäpunyu!*, the personal name of a man who stood in the relationship of mother's father (*taiyi*) to him and who had died a very long time before.

If a Wik Monkan man accidentally injures himself, he may make an exclamation and call upon the name of a deceased relative, e.g., *Waiya*

Kutakan!, "Kutakan, (it is) bad," the equivalent of "bad luck, Kutakan!" Generally, the name called upon will be that of a relative long deceased, but if he should inadvertently mention the name of a close friend (pām kāmpan)<sup>10</sup> or a bystander, it may make trouble or ill feeling (i.e., kul kentan, literally, "anger arouse"). Everyone laughs except the offended one, who remains silent but kakul,<sup>11</sup> "sulky," or kul puntj, "angry inside." If, however, the offence is purely an unintentional one the speaker exclaims at once Äpa, ta-kättiti<sup>12</sup> ñai'ya! A similar custom exists in the Kōkō Ya'ō tribe, and if a man swears in the presence of a relation who is kintji to him, he exclaims kama kätta! or ñāta'kama kätta!, "my mouth is foul," and sometimes takes a lighted firebrand and passes it backwards and forwards in front of his mouth as a purification ritual. At a ceremony at which I was present in an Ompela camp I became aware that I had made a *lapsus linguae*; I exclaimed kama kätta! and one of the old men who was sitting with me at once passed a hurricane lamp close in front of my mouth, in lieu of fire.

If a Wik Monkan man misses a fish that he thinks he should have speared, he may exclaim waiya! ñai'ya yapen!,<sup>13</sup> "No good (here an exclamation of disgust), I do not know how (to catch fish)." Then he jerks his arm outwards from the elbow, crying ma'a punta waiya, "my forearm no good."

When he receives a minor shock or surprise, a native will exclaim almost involuntarily yākkai! (Kōkō Ya'ō, Ompela, Kanju, Wik Monkan), or ākkai! (Līn'ñitti and Tjūñundji), an exclamation expressive of pain or

<sup>10</sup> Pām kāmpan, literally "man friend," "friend person," is a phrase meaning something like "one's own people" as applied to the closer relations within the kinship system.

<sup>11</sup> Kakul is idiomatic; the etymology is interesting. The usual word for face is ñurp, but the word ka, properly "nose," is frequently used idiomatically for the face, especially in compound words. Hence, the literal translation of the words would be nose (ka) angry (kul) ñurp kul ("face angry") does not appear to be used in this idiomatic sense.

<sup>12</sup> Kat (katta of Ompela and Kōkō Ya'ō) means "bad, putrid, stinking, decayed," but it may, as in this instance be used simply for "old, dead." Hence yuk kāt, a dead or dry tree (A dead man, however, is pām myl, he would be pām kat, or pām myl kat, only if stinking.)

<sup>13</sup> In the Wik Monkan language a poor hunter is spoken of contemptuously as ma'a yapen, but a good hunter, one who shows special prowess, particularly in killing emu (*Dromaeus novaehollandiae*), native companion (*Megagloris rubicundus*); jabiru (*Xenorhynchus asiaticus*) and other large and wary birds, is distinguished by the name ma'a tup, a title that is much coveted. Tup may be applied also to a woman, as for example, a woman who excels at the task of gathering vegetable food (mau), etc. Such a woman is mai'tup, or wantj (woman) mai tup, not wantj tup (*vide infra*). A man renowned for his fighting prowess is kek (spear) tup, and a "woman hunter," a man celebrated for his conquests, is wantj tup, a title that most men would deem it politic to disavow in public.

surprise. Less frequently he may exclaim *yākka papa!* (*Kōkō Ya'o*, *Om-pela*), *yākkaté!* or *yākkatändäm!*, mild expressions used frequently by women and children, and exactly equivalent to the English exclamation "Oh mother!" The expression may be varied by the substitution of any other term of relationship, e.g. *yākka pipi!* (father).

The expressions recorded above are frequently heard and are in everyday use, but in three years spent among these natives I never knew them to depart from their traditional behavior and to use foul or obscene expressions under stress of pain, fear, or surprise, although these are freely used under other circumstances. It has been stated, however, that swearing and obscenity is not only often heard but that it plays an important part in social life. How then is this swearing employed and what functions does it serve? Swearing is of two main types:

1. Unorganized swearing and obscenity, falling under no sanction and used by both sexes in quarrels, and as taunts to goad an enemy to fight. This type of swearing is known to the *Wik Monkan* people as *kul kentānāk*, "anger make-for," i.e., "for the purpose of arousing anger," for which purpose it is deliberately employed. Under this category fall all the worst expressions in the language: deadly insults that it would be intolerable for any native to receive in public.

2. Organized or licensed swearing and obscenity that is not only permissible, but obligatory, between those who stand in certain relationships under the classificatory system. It is carried out in public, and falls under a definite social sanction. This organized license is in direct contrast with the extreme tabu (*kintja*) of certain relationships, e.g., with the wife's mother, the wife's father, and the wife's brothers. It is supposed to induce a state of euphoria: in the words of my informants, to "make everybody happy." This falls again into two distinct types:

- a) Obscenity pure and simple, consisting of more or less stereotyped references to the pudenda, and permitted between a very few relatives; frequently in the grandparent-grandchild generations, and almost always between persons of the same sex. Certain relatives are also permitted to snatch playfully at one another's genitalia, and even to handle these organs in public.

- b) Bad language, consisting chiefly of references to the anus and to excrement, permissible or obligatory between a number of relatives, generally, distant,<sup>14</sup> with whom license of a restricted type is permitted. The use of obscenities, or any sexual behavior is prohibited.

<sup>14</sup> It is still a frequent error to suppose that an aboriginal does not distinguish clearly under the classificatory system between own and distant relatives. Every shade of difference

## TAUNTS, SWEARING AND OBSCENITY

It will be evident from the foregoing that while swearing is of frequent occurrence it is used only within well-defined limits. The word for an oath in the Kòkò Ya'ò, Ompela and Wìk Monkan tribes is *ākān*. I joked frequently with an old man of the Kòkò Ya'ò tribe who stood to me in the relation of *mimi* (≡<sup>15</sup> *kamijo*), mother's mother's brother, and once remarked to him *nono nani nātan' ākāna?* ("You what for mine swore?"), "What did you swear at me for?" He replied at once *naiyu nāmpa ākāna, naiyu mimi tjlbo!*<sup>16</sup> ("I did not swear, I [am] *mimi* [to you] old man") "I did not swear at you, I am your *mimi* and an old man," a grey headed one, i.e., with the inference "and it would be incompatible with my dignity."

In the Wìk Monkan tribe a person who is always swearing is *ākān wenta*, literally "swearing mad," or idiomatically, *ta kōnn*, *ta kōnnitti* or *ta kōnn auwa* (*ta*, "mouth," *kōnn*, "ears," *kōnnitti*, an emphatic derived from *kōnn* [pages 471-72]; *auwa*,<sup>17</sup> the name for any totem center, also occurs frequently in idiomatic expressions for a source of plenty, any thing or place that is always giving rise to something else). When a Wìk Monkan child makes frequent use of bad language, a relative, particularly the father's elder sister, smacks its lips and cries *ta kāt*, "mouth foul." This is in contrast with the behavior in the Ompela tribe, noted on page 465, where the child was uncorrected.

Before proceeding with the study of the joking relationship I shall give an account of the taunts, swearing, and obscenity that are in general use, particularly those of the *kyl kentānak* type swearing.

Although swearing and obscenity hold an important place in all these tribes it will be found that the exact implication of the insult in swearing varies considerably from tribe to tribe, and bears a direct relationship to the social life. Thus, in the Kòkò Ya'ò and Ompela tribes the grossest insults are accusations of incest, the most serious of which is maternal incest; in the

can be expressed by an aboriginal In the Kòkò Ya'ò tribe a man may say *pipi*, father; *pipi natan*, father mine, or *pipi talli*, father distant or "outside;" *talli* is literally leg (in anatomical sense) and is employed to express "distant" only in kinship; distant (of place) is *katji*

<sup>15</sup> This symbol indicates reciprocal usage of the kinship terms.

<sup>16</sup> *Tjlbo*, "the grey one," is a term of veneration in the Kòkò Ya'ò and neighboring tribes. It is used as a noun as well as an adjective and may be employed as a term of address. To say *Tjlbo* is in the Kòkò Ya'ò tribe a term of respect and is very different from the ordinary use of the equivalent in English *Tjilbobinda*,—*binda* a suffix meaning "the bearer of," i.e., "the bearer of the grey hair," may also be used.

<sup>17</sup> *Auwa* appearing after a name, except in idiomatic expressions, always signifies a totem center, e.g., *kaŋ'kaŋ* is the white-bellied sea eagle; *kaŋ'kaŋ auwa* the totem center of this bird. Similarly *patj* is a shooting star, *pātjauwa* the shooting star totem center.

Wìk Monkan tribe these are generally associated with the genitalia, and in the Tjùṇdji, the two most serious insults are to accuse a man in public, on the one hand, of having eaten food bitten by a dog, and on the other to reflect upon the chastity of his mother. These expressions are insults that no man could endure and are therefore goads that invariably result in a fight.

In all the tribes referred to in the present communication, humorous, or even ribald, remarks on parts of the body are used in a jocular fashion, and most of these are innocuous unless they are addressed to a person who stands in a forbidden relationship. Thus, in the Wìk Monkan tribe, while these terms may be used freely between father's father and son's son (pola and poliäṇ), a fight would almost certainly result if they were used by an elder sister's son (tuwa) to his mother's younger brother (kala). A series of expressions from the Wìk Monkan language, relating chiefly to parts of the body, are set out below, showing a gradation from merely jocular references to the hands, face, and legs, to obscene references to the pudenda, that constitute the grossest insults in the language.

meritti	big eye <sup>18</sup>
yäṇäntti	plenty hair (yäṇän is hair of the human head)
konn werrä	ears wide, i.e., protruding ears
yänk onk	legs long
ka onk	nose long
ma'a punta wäkk	arm like grass: ma'a, hand, punta, arm (literally "hand arm," i.e., the forearm or brachium), wäkk, grass; hence, "your arm is like a stem of grass"
tump mäny'	legs little, thin legs, thin calves: tump, leg, mänyä, little
kutjäketti	big head <sup>18</sup>
kutjäk onk	head long

In an Archer River camp a woman was often addressed playfully as pap mer onk, lit., "breast eye (nipple) long," "the one with the long nipple," and an old woman whose hair was straight and sparse was called yäṇän wäkk, "grass hair," hair thin and sparse like grass. When I applied the term to her she retorted at once Yäṇän yark, "hair open and sparse." Yark is the term applied to a grass basket of very open coarse texture.

Most of the foregoing expressions are innocuous except where they are

<sup>18</sup> Mer, "eye," -itti, a suffix frequently used to give strength to an expression, and sometimes carrying the force of a superlative, e.g. kutjek or kutjak, "head," kutjäketti, "big head;" kät, "bad," kätitti "most bad;" yaṇän, "hair (of head)," yaṇanti, "having plenty hair"

incompatible with the obligatory norm of behavior, but *mer pot*, "eye white," "eye opaque," is generally regarded as an offensive term. Even the most innocent sounding expressions may also be used in earnest. Thus a woman may say to a man who is looking hard at her, *merritti*! "big eye," i.e., equivalent to saying "What are you staring at?" He would reply at once, knowing that his advances were repelled and desiring to avoid a clash with the woman's husband: *ŋaiya ke'a tättāŋ n'yintaŋ*, "I (am) not looking you-at," and she might reply again: *Yé!mer punkätti*! "Yes! eyes big round," i.e., "staring eyes."

When a strong or offensive term is intended playfully, the word *puk*, really "child," "baby," is sometimes prefixed. When used to an adult it has the force of a diminutive, a term of affection that takes the sting from the word that it accompanies. Thus, *waiya*, "bad," "a bad one;" *puk waiya*, in its literal sense "a bad child," is much the same as "you little devil."

A number of taunts are used, but always to express ridicule, contempt or defiance, and are not as a rule employed playfully among adults. The taunts most frequently used among the *Wìk Monkan* are:

*Tjìn'tjìn!* a taunt and untranslatable, used especially by women and cried in a derisive tone and sing song voice calculated to annoy. It is however, a taunt that few women would dare to throw directly at their husbands. My informants declared that if a woman did so aggravate the man he would at once seize her by the throat and half throttle her, i.e., *man intan*, lit. "neck seize."

*Tät*, another taunt used especially to defy a man who has threatened to throw a spear.

Of the more serious swearing in the *Wìk Monkan* language, the following obscene references to the pudenda are typical.

To a man:

<i>po'q wantj wenta</i>	vagina woman mad; lit. <i>po'q</i> , female pudenda, <i>wäntj</i> , woman, <i>wenta</i> , mad
<i>kuntjitti</i>	big penis; <i>kuntj</i> , penis (- <i>itti</i> suffix, see fn. 18)
<i>kuntj onk</i>	long penis
<i>kuntj täntjitti</i>	penis fatty; <i>kuntj</i> , penis; <i>tänt</i> , fat
<i>kuntj mánkatti</i>	penis root big
<i>untjitti</i>	big scrotum; <i>unt</i> , scrotum
<i>otjumti</i>	plenty urine

To a woman:

<i>pam wenta</i>	man mad
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nok werräm	nok, inguinal fold, werräm wide, hence equivalent to "you are exposing yourself"
pə'o pätj	no pubic hair; pə'o, pudenda, pätj, bald, (pubic hair is yi pätj)
pə'o ka onk	long clitoris; pə'oka, clitoris; lit. "vagina nose"
pə'o kati	enlarged clitoris
pə'o tantitti	vagina fatty
pə'o konnitti	labia minora enlarged; lit. "vagina ears big"

With the exception of one or two of the terms applied to the male pudenda which may be used in one of the joking relationships, none of these terms is used jocularly; all are employed only in the serious quarrelling that is the prelude to fighting and are spoken in a sharp tone of voice. Thus the term *maritj tup*;<sup>19</sup> *maritj*, one given to stolen intercourse with the opposite sex, or even *maritj wenta* (*maritj* mad) may be used jocularly, but such a term as *päm wäntj wenta* (man woman mad) is never used except as a deadly insult. These, then, are the expressions used when all restraint is cast aside: the expressions, called *kul kentanak*, to which reference has already been made. When a man is really angry he seizes his spear and spear thrower and bites the handle of the spear as he advances, before hooking it on his spear thrower. Real anger in a woman however, is expressed by a gesture called *änkäm ompän* the literal translation of which means "elbow cut," in which the elbows are flexed and the arms are flapped against the sides of the body. If the woman is carrying a yam stick (*kätjin*), she holds it stiffly in front of her at arm's length, and flapping the other elbow against her side, she flexes her knees slightly and leaps up and down, drumming with both feet on the ground. In this way she works herself into a passion of anger while she shrieks abuse and obscene taunts at her adversary.

It has been noted that in the *Kokö Ya'o* and *Ompela* tribes the most serious insults are those concerning incest. Of these expressions, *yanta papa gobi* ([you] like [your] mother's vagina) equivalent to "have connection with thy mother," an expression so grave that it is never used except in deadly earnest, as in serious quarrels that generally result in fighting. I heard it used in this way only once; by a woman to her husband during a serious domestic quarrel. The same expression may be used with other relationship terms which may be substituted for that of the mother (*papa*), such as *ya'a* (elder sister) or *kulnta* (wife, *Ompela*), hence *yanta-ya'a-gobi*, or *yanta-kulnta-gobi*, but the suggestion of incest with the mother is the gravest insult of all. This is in contrast with the observations of Professor

<sup>19</sup> *Tup* is a term denoting the possession of special prowess. see footnote 13.

Malinowski,<sup>20</sup> in the Trobriand Islands: he states that "the maternal incest is absolutely and completely out of the question, yet it is the mildest abuse." In a well organized North Queensland community incest of any kind was punished by death; it was not a personal matter, a matter for individual action or private vengeance, but fell under an organized social sanction; the people were "shamed," a ritual state of sin, of disphoria, prevailed, for which there was no expiation. The punishment of incest was death.<sup>21</sup> The closer the relationship the graver the incest; it is evident therefore that to charge a man in public with maternal incest, with a crime against the society for which there was no expiation, is to challenge him to fight.

### THE JOKING RELATIONSHIP

It has been stated that the joking relationship is found in all the tribes of Cape York Peninsula. This joking relationship is of two distinct types:

- 1) With a member of the grandparents' generation, frequently an "outside" father's father, and
- 2) With certain relatives by marriage, generally where an incompatibility in the obligatory pattern of behavior has been set up by the marriage.

In each instance the freedom in language and the license in behavior permitted between those within the joking relationship—in the presence of other members of the horde—is the antithesis of that obligatory to those who stand within certain other degrees of the kinship system, such as the wife's mother, the wife's father, and the wife's brothers, the behavior towards all of whom is governed by the most severe code of restraint with many inhibitions. In the case of the first this consists of an absolute prohibition on speech of any kind, and in the second and third the use of a special language, called in the Ompela tribe *ɲɔrŋki*, e.g., *ɲɔrŋki pɪloba-go*,<sup>22</sup>

<sup>20</sup> *Sex and Repression in Savage Society* (New York and London, 1927), p. 106

<sup>21</sup> In another place (*The Hero Cult, Initiation and Totemism on Cape York*, p. 511) I have shown that even where the natives were so much under the influence of the white man that they were unable to carry out their own law, they would not tolerate an incestuous marriage, but compelled the offending couple to live apart at some distance from the camp of the horde to which they belonged, and from the social life of which they were cut off.

<sup>22</sup> *Kokɔ* in the Ompela and allied languages is "talk" or "speech" and is the word used in ordinary conversation. It appears frequently as a prefix before the name of a tribe, e.g. *Kokɔ Ya'o* which means literally *kokɔ*, "speech," *ya'o*, "this way." A separate vocabulary, amounting almost to a separate language, is also employed in speaking *at* *kintja* (tabu) relations of certain orders. This is known as *ɲɔrŋki*, not *kokɔ*. The meaning of *ɲɔrŋki pɪloba-go* is literally *ɲɔrŋki*, "speech," *pɪloba*, "wife's brother," *-go*, suffix meaning "to and for;" "speech for wife's brother," hence "talk for a tabu person."

Similarly, in the Wik Monkan tribe, the name of this speech for tabu relations is *ɲonk wonka tonn*, literally, "speech side another" (see pages 485-86).



and in the Wik Monkan, *nonk wonka tonn*, which may be used only indirectly, that is *at*, and not *to*, the person addressed. In the apt phrase of my informant, they may talk "one side." I shall have occasion to speak more fully of this "one side" talk when discussing the joking relationship in the Ompela and Wik Monkan tribes. The joking relationship of the first type, between members of the grandparent's and grandchildren's generations, is the more widely distributed type, and the one in which the greatest license is permitted. It is only in this relationship between individuals two generations removed, e.g., between the father's father and the son's son (in certain of the tribes of this area), that organized obscenity and behavior of a sexual type, is permitted, and even becomes the norm of behavior. In the Ompela and Koko Ya'o tribes the relation of the father's father (*pola*) and his classificatory son's son (*poladu*) is an extraordinary one, and is characterized by extreme freedom and license both of speech and behavior, in the presence of other members of the horde, that is permissible with no other individual. It is the *pola* and *poladu* who pursue one another and snatch at one another's genitalia.

In all these tribes—the Koko Ya'o, Ompela, and Kanju—the name *pola* is applied not only to the father's father but also to the clan totems.

In the joking relationship of the second type, while a great deal of freedom is permitted, this generally takes the form of bad language rather than of obscene expressions, and references and behavior of a sexual nature are excluded.

In each case greater freedom is permitted between relations that a man calls *talli* (distant) than with *yintju* (close) relations, and as a rule restraint must be exercised with blood relations, to whom a man applies the term *ñätaño*, "my own."

When I asked two men of the Koko Ya'o tribe who stood in a joking relationship and who were perpetually exchanging terms of abuse, why they swore, one of the men replied "Yor'yor (nothing), *ati* (friend), make happy little bit, no swear proper!"

It has been noted that all this organized swearing and obscenity in the joking relationship, that is, all the swearing that falls under a social sanction, is carried on in camp in the presence of other members of the horde, and it fulfils a definite function. Just as the proper observance of the tabus governing behavior towards the wife's mother and certain other relations, maintains a condition of euphoria, the joking relationship induces a state of ritual well-being: in the words of the natives themselves it "makes everybody happy." For it must be remembered that the proper observance of the formal pattern of behavior under the kinship system is not a matter

of individual choice or caprice; it is laid down by tradition and governed by organized social sanctions. A serious breach of etiquette under the kinship system "makes everybody 'shamed,'" a state of disphoria results; hence any breach of etiquette must be expiated by ritual means (pages 467-69). In the Kòkò Ya'ò tribe this ritual cleansing is effected by washing the mouth with water.

A short account of the joking relationship in each of the tribes under discussion follows.

#### THE JOKING RELATIONSHIP IN THE KÒKÒ YA'Ò TRIBE

The joking relationship in the Kòkò Ya'ò tribe is well developed, and plays an important part in social life. It falls into the two main types noted above, but as it is inseparably linked with the kinship organization, some knowledge of this system, especially of the type set up by marriage, is essential to an understanding of it.

In the Kòkò Ya'ò tribe two types of marriage are permitted, with a classificatory cross-cousin, that is with a mother's brother's or father's sister's daughter, and with a classificatory sister's son's daughter. Marriage with the actual mother's brother's or father's sister's daughter, that is with an actual cross cousin, is prohibited: the relationship must be *talli* (distant) not *yintju* (close or actual). Swearing and obscenity, whether used within the privileged relationships or as taunts or goads during quarrels, resemble in import those in the Wik Monkan language. Among the most common are *ɲono wakkan ontan*, "you scrotum burned;" *tall'al woipa* or *tall'al*, "(your) semen (is) close." To a woman the most deadly insults are *pinyoi!* (*pinyu*, "vagina," -oi, a suffix used in hailing a person from a distance), hence the expression is equivalent to "Hey! vagina!" and *yini myk-kân*, "enlarged clitoris;" as in quarrels between men, references to the genitalia are frequent, especially such expressions as *ɲono yini ontan*, "you vagina (is) burned." The men, especially the single men, are ashamed when they hear these words exchanged between the women in public, and bow their heads.

The three principal joking relationships in this tribe are:

(1) *Pola* (father's father) and *poladu* (son's son);

(2) *Yami*=*yami*, a joking relationship that is set up between two individuals who stood at first in the relationship of elder brother (*yapu*) and younger brother (*ya'adu*), after one has married a classificatory sister's daughter (*mampa*) of the other.

The term *yami*, applied to the actual mother-in-law and to her brothers and sisters, is thenceforth used reciprocally, with a pattern of behavior in strong contrast with the extreme tabu of the usual *yami* relationship.

(3)  $\text{alma} \rightleftharpoons \text{alma}$ , a joking relationship established between two individuals who were at first yapu and ya'adu, when one has married a classificatory mother (papa) of the other.

The type of behavior obligatory is very different in these two main types. Thus the greatest freedom is permitted between pola and poladu, and behavior of an obscene or sexual type, characterized by the natives themselves as a'inyu woŋa-go is permitted. This is contrasted with the behavior in the yami and alma relationships, which is characterized as a'inyu kuna-go.

(1) A poladu may borrow the spears or other personal possessions of his pola during his absence without asking his permission and the pola must show no resentment. This fact was brought home to me by a little comedy that was enacted in a Koko Ya'o camp near Lloyd Bay. I had picked up a puppy belonging to an old man who was standing some distance away and was pretending to carry it off. He was really concerned, and I was about to set his mind at rest by putting down the puppy, when the bystanders, who were much amused at the incident, told me in an aside, to address him as my pola—with the obvious desire to prolong the amusement of the incident at the old man's expense by adding to his embarrassment, while rendering him powerless to object.

This obligatory type of behavior is carried out in the presence of the entire camp and is often given free rein on the  $\eta\text{artji kintja}$ —the tabu ground, or sacred initiation place during the prolonged preparations for the ceremonies of the Okainta,<sup>23</sup> when the  $\eta\text{artji kintja}$  becomes a kind of men's club. On this  $\eta\text{artji kintja}$ , during preparations for a ceremony, I have heard two men, pola and poladu, who were squatting on the ground engaged in making spears and ornamenting a drum (waiyuba), exchange a running fire of obscenities for hours on end—each preserving throughout the greatest good humor in the face of insults that in other circumstances would have been intolerable. Among the expressions used were woŋanto mukkan! "penis—yours big:" the reply to this was ulmpe!  $\eta\text{aiyulla tjo-a-tji!}$  "Nothing! mine is a little one:" woŋa kätta! "penis (is) rotten." At intervals, especially after one of these exchanges of obscenities, the men may snatch at one another's pudenda. A poladu may even say to his pola, pa'il matjatjina! "Your wife (actually my father's mother), I shall rape!"<sup>24</sup> an unheard of joke with any other relative. There is no doubt that the exchanges under the joking relationship do provoke genuine mirth, as well as a ritual state of

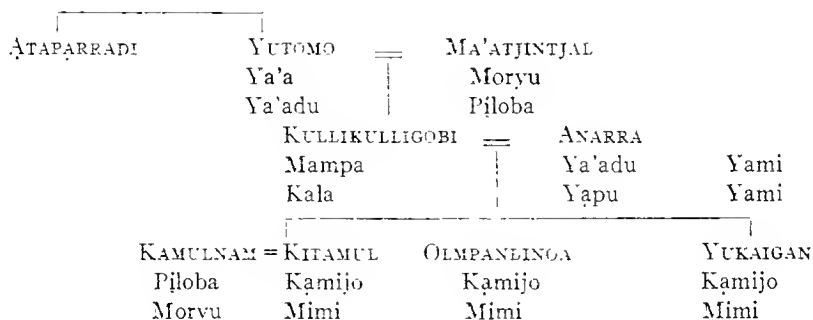
<sup>23</sup> See The Hero Cult, Initiation and Totemism on Cape York, pp. 473-82.

<sup>24</sup> Matjatjina is a euphemistic expression for rape, its literal meaning is "I shall catch hold," tykin ("to copulate with") really has the force of rape; see page 466.

well-being, that counterbalances, relieves, and gives point to, the austerity and restraint that characterize much of the behavior under the kinship system.

(2)  $yami \rightleftharpoons yami$ . Although the extreme license of the previous relationship is here considerably restricted—references to the anus and to excrement alone being permitted—the obligatory behavior presents the strongest contrast with the usual behavior to the wife's mother ( $yami$ ), and the sisters and brothers of this woman. For not only is it inconceivable that he should swear at or in the presence of any one of these people, but he may never speak directly to them nor even approach them or any group in which they are standing.

The relationship that was set up when a Kókò Ya'ò man Anarra married the mampa (sister's daughter) of his yapu (elder brother) is as follows.



It should be noted, however, that the individuals were all distant kin, and that the relationship shown in the table is classificatory, not actual. Yutomo was a *ya'a talli* ("outside" elder sister) of Ataparradi and not an actual blood relation. Moreover as Anarra and Ataparradi were brothers it will be evident that both should have applied the term *mampa* to the woman Kullikulligobi, whom Anarra married. Incompatibilities in relationship terminology were, however, of frequent occurrence, especially when individuals from widely separated clans came together, and not only gave rise to these joking relationships with a reversal of the customary pattern of behavior, as in the *yami* relationship, in which extreme restraint normally observed gave place to license, but were actually provided for in many of the kinship systems by the use of special terms employed only to overcome incompatibilities in terminology, and which do not necessarily indicate incestuous marriage.

Anarra applied the term *yami* to Yutomo, his wife's mother ( $pima \rightleftharpoons piado$ , before betrothal, in the case of a classificatory cross-cousin marriage) and therefore extended the same terminology to her brother, who would normally, as the brother of a woman to whom he applied the term *pima*, have been his *pipi* (father) and not his *yapu*.

Examples of the organized swearing employed freely within this relationship, and known as a'inyu kuna-go are kuna katta! "excrement stinking;"  $\eta'$ kän' kuna katta! "your excrement stinks;"  $\eta'$ kän' kuna kama tonko! "your anus (is) (kama kuna, literally "mouth of excrement") black;" and endless variations of these. No sexual references are permitted, nor does this license extend to other forms of behavior, as, for example, to the handling of the genitalia.

(3)  $\mathfrak{a}$ lma $\rightleftharpoons$ alma: The behavior within this relationship is similar to that between yami.

#### THE JOKING RELATIONSHIP IN THE OMPELA TRIBE

The joking relationship in the Ompela tribe is of the same type as that of the Kōkō Ya'o tribe, and as the languages and general culture of the two tribes differ only in minor details, the joking relationship extends to neighboring hordes over the boundaries of the tribes. The kinship system of the Ompela tribe, however, differs slightly from that of the Kōkō Ya'o, and only one type of marriage, a bilateral cross-cousin type (with the classificatory mother's brother's daughter and the father's sister's daughter) is permitted; marriage with the sister's son's daughter is here prohibited.

The yami and  $\mathfrak{a}$ lma joking relationships occur in the Ompela tribe as in the Kōkō Ya'o, with the same restrictions as in the latter tribe, and again, the greatest amount of license is permitted in the  $\mathfrak{p}$ ola $\rightleftharpoons$ poladu relationship, where sexual references, as distinct from references to the anus and to excrement form the pattern of behavior.

In addition, a joking relationship, with considerable freedom in behavior, exists between the mother's father ( $\eta$ atjimo) and his daughter's son ( $\eta$ atjijo), distant, not actual. I had two men who stood in this relationship to one another with me in a boat crew on the Gulf of Carpentaria, and they maintained an almost incessant exchange of ritual abuse, even when traveling with the launch at sea. The behavior in this relationship is very similar to that between  $\mathfrak{a}$ lma and yami, and although a good deal of license is permitted, definitely sexual references are excluded. One of the most frequent sallies that passed between the two individuals mentioned above was the following:  $\mathfrak{k}$ opi  $\eta$ āno wāntil  $\eta$ ātagān? (Sweetheart you what time marry?), "When are you going to marry your sweetheart?" and the reply was  $\eta$ ola, "bye and bye." Both men laughed, and each stretched his arm out towards the face of the other as if to chuck him under the chin, at the same time exclaiming Eeh! in a hoarse voice. This exclamation, and the accompanying gesture, were repeated after each fresh sally.

Alma. On another occasion I overheard one of these men exchanging

the following expressions with an individual whose relationship to him I did not then know: *ṁāno intjai'yu kantai'ya!* (You wild banana eat), "Eat a wild banana." To this he retorted at once *āpāti! kuna<sup>25</sup> ṁātagān'*, (Don't anus mine-nothing), "Don't: my anus will be shut!" This sally, which terminated with the exclamation noted above, was repeated, with slight variations, at intervals throughout the day, "to make fun." On enquiry I found that these two men were *alma* to one another. No blood relationship existed between them, and in fact they came from hordes far separated. At first they had stood in the relationship of *yapu*=*ya'adu* (elder brother=younger brother), such relationship being established, as a rule, between strangers after lengthy kinship discussion, during which a mutual relationship to a third person is eventually discovered. This establishes a kinship bond between the two men which may then be extended to all the members of the hordes to which they belong. In this way, incompatibilities in kinship terminology are largely avoided, but the rigidity of the marriage rules are often relaxed when a man comes "from long way," and incompatibilities may then arise. One of these men married a classificatory mother (*papa*) of the other, who then married the younger sister's daughter (*mukadu*) of the first. Thus, while in the beginning they were brothers, after the first marriage one should have stood in the relation of father=son (*pipi*=*piado*) to the other, and the *alma* joking relationship was set up. As a result of the second marriage they would have stood in the relationship of *yami* to one another.

I have stated above (footnote 22) that in certain of the relationships established by marriage, the pattern of behavior is one of restraint so severe that a special vocabulary, amounting in certain cases almost to a separate language, is employed. This language is well developed in the *Koko Ya'o*, *Ompela*, *Kanju*, and *Wik Monkan* languages. In the *Ompela* tribe, the behavior between individuals who apply the term *yami* (wife's mother=daughter's husband) reciprocally is normally one of absolute avoidance. Not only may they never speak, but they must not approach, or even look directly at, one another, and the woman may carry a palm leaf shade or a sheet of tea tree (*Melaleuca*) bark, with which she covers her face when she passes in the vicinity of her *yami*. It will be evident, therefore, that the license permitted in the *yami* joking relationship constitutes a simple reversal of this behavior.

The behavior obligatory between father-in-law and son-in-law is only less severe. A father-in-law, i.e., the husband of a *yami*, is *armpai'yi*. This

<sup>25</sup> *Kuna* is really "excrement," *kuna kama*, lit. "excrement mouth," is the anus, but *kuna* alone is sometimes used

man may speak to his daughter's husband ( $\eta$ artjam $\eta$ o), but the latter may not reply directly. The son-in-law may talk "one side," that is, while he may not address his elder in ordinary speech ( $k\acute{o}k\acute{o}$ ), he may speak in the language known as  $\eta$ ornki. Even in this language, however, he may not address his remarks in the first person directly to his armpai'yi, but to his child, or even to his dog, to which he speaks as to a son,<sup>26</sup> and not directly to the person for whom the remark is intended. A similar type of behavior exists between a man and his wife's brother ( $\pi$ iloba $\rightleftharpoons$ moryu). A moryu may speak to his piloba, that is to his sister's husband, but the latter may not reply directly; he "talks one side," that is he uses  $\eta$ ornki, not  $k\acute{o}k\acute{o}$ .  $\eta$ ornki does not comprise a complete language, but a set of names for the most important objects and articles of everyday life, as well as certain verbs. It is a skeleton language only, but it must be remembered that this is probably correlated with the type of behavior obligatory between those by whom it is employed, among whom communication, especially verbal communication, is reduced to a minimum. The existence of this language, and of special terms for many objects, the names of which may be tabu ( $k\acute{i}ntja$ ) during the "period of the separation" during mourning, is probably responsible for the suggestion that sometimes has been advanced that languages in Australia are unstable and that the name of an object obtained at one time may be entirely different from that obtained at another. In spite of the fact that this dual language occurs in most, if not all, the tribes of Cape York Peninsula, and its obvious importance to an understanding of these people, as far as I am aware its existence has never hitherto been recognized or recorded.

For comparative purposes, a list of the names of some of the common objects in the language of the Ompela tribe are set out below in  $k\acute{o}k\acute{o}$  and in  $\eta$ ornki

<i>English</i>	<i>k\acute{o}k\acute{o}</i>	<i>\etaornki</i>
water	$\pi i'i$	tulnk\~{a}n
meat	m\~{i}nya	yantai'yi
(animal food in general; also generic term for any animal, game, or fish)		
vegetable food	mai'yi	mamp\~{a}ndji
(food in general; also generic term for vegetable food or food of vegetable origin)		
fire (also firewood)	yuma	m\~{a}nto

<sup>26</sup> In these tribes a dog has a place in the kinship system; it is piado (son or daughter, man speaking) to a man, and mampa (son or daughter, woman speaking) to his wife.

camp	ɲartji	manko
dog (domesticated dingo)	ko'aga	ɲonkɔŋo
tobacco	keni	operri
(originally <i>Derris trifoliata</i> var <i>macrocarpa</i> , a fish poison)		
firestick ( <i>Premna acuminata</i> )	tɪkki	po'anyimo
palm leaf basket	olko	wokidji
(water vessel)		
grass basket	pontɔi'yu	muntamo
string "dilly bag"	pikon	yolun
canoe	wornyo	ɲa'alɔdi
spear (generic term)	yan'ka	panko
spear (multiple pronged)	tai'yä	paityä
spear thrower	yuli	tanti
		put'tjuga
tea tree bark	ontji	poimi
yam ( <i>Dioscorea sativa</i> )	dampu	mɔi'yuno
yam ( <i>Dioscorea sativa</i> , var <i>rotunda</i> )	ka'ata	ɪntani
hand	ma'a	pulomo
foot	ta'o	owul
nose	né'yi	qtjal
eye	tuntɔi	ɲulmpai
mouth	kamä	ta'alläga
ear	yämpä	ɲätjäppi
big	mukkän	mäntéyatti
little	tjo'a'tjo	imil
cuscus ( <i>Phalanger maculatus</i> )	ampoiyu	atanyu
bandicoot ( <i>Isodon peninsulae</i> )	kulpa	pinyi pinyi
echidna	ka'oma	ɲintai'yi
wallaby ( <i>Macropus agilis</i> )	piwo	pikoni
tabu	kɪntja	wotji

It will be evident from the above vocabulary that a great deal may be expressed with a minimum of words. Thus all animal food, all plant food, water, camp, fire and firewood may be expressed by five words—mampändji, yantai'yi, tulnkän, mänko and mänto—which again may be supplemented by the uses of the gesture language that is used freely in all kɪntja relationships. Moreover, without an appreciation of the uses of these two vocabularies, and of the kinship system and the behavior within it, a great deal that passes in a native camp must be incomprehensible. If two men stood in the relationship of moryu ⇌ piloba, the moryu would say to his piloba, yuma kälówé! "fire bring-up;" the piloba on the other hand would say manto palonolla! "fire bring."



## THE JOKING RELATIONSHIP IN THE KANJU TRIBE

The joking relationship is also found in the Kanju tribe of the central highlands of Cape York Peninsula. It differs in no important respects from the relationship in the Ompela and Kōkō Ya'o tribes that occupy the territory to the eastward, and to which it is culturally and linguistically much more closely allied than to the tribes of the west coast.

## THE JOKING RELATIONSHIP IN THE WĪK MONKAN TRIBE

It has been shown above (p. 469) that in the Wĭk Monkan tribe swearing is of two distinct types. Apart, however, from the swearing of the *kūl kentānāk* type that has already been discussed in some detail, a well defined joking relationship, resembling closely in all essential features those that have been described from the Kōkō Ya'o and Ompela tribes, also exists in the Wĭk Monkan. The marriage system in the Wĭk Monkan tribe differs from that of any of the tribes hitherto discussed and is of a second cousin type.

Āk is the Wĭk Monkan term for an oath, and organized swearing of the type found in the joking relationship is *ākwunāk ke'ātnāk*, "swear-for play-for," or *pām kāmpān ākwunāk ke'ātnāk*<sup>27</sup> "man friend swear-for play-for," i.e., "swearing for play among relatives."

Again in this tribe, the greatest license is permitted between members of the grandparent-grandchildren generations, usually distant, and called by the Wĭk Monkan *mäll tonn* (*mäll*, "right hand," *tonn* "another"). For a man the principal joking relations are:

1) *mukka* ( $\rightleftharpoons$  *mukkaiya*), a mother's elder brother. Some decorum must be observed in the behavior to the actual (own) *mukka*; and to the mother's younger brother (*kala*), own or classificatory, extreme deference must be shown. A *kala* is *ṇaintja taiyīn*, my informants stated (*ṇaintja*, "tabu," *taiyīn*, "hard" or "severe"). Considerable interest attaches to this relationship and to the unusually sharp distinction in behavior between the mother's elder brother (*mukka*) and the mother's younger brother (*kala*).

2) *ṇātja*  $\rightleftharpoons$  *ṇātjiyāṇ* (mother's father  $\rightleftharpoons$  daughter's son or daughter), also father's *mukka* and *kala*.

3) *pola*  $\rightleftharpoons$  *poliyaṇ*, father's father, classificatory or "outside."

4) *kōrpān*: a name used reciprocally between two people who stand in either of the following relationships with one another: mother's elder brother (*mukka*) or mother's father (*ṇātja*), whose wives, instead of being, in the first case *pīnya*, and in the second *kemā*, are placed in another rela-

<sup>27</sup> *Pām kāmpān*, the literal translation of which is "man friend," is the term for relatives who are not *ṇaintja* (*tabu*) under the kinship system: -*ak* is a suffix meaning to or for

tionship which makes the obligatory behavior incongruous or incompatible, through a "crooked" marriage "from first somewhere." These people call one another *ma'a korpän*, and between them a joking relationship is set up, in which considerable license is permitted.

It should be noted that in each case it is an outside or classificatory (*mäll tonn*), not an actual, relation who stands in the joking relationship. With the actual relations in each case, some restraint is required.

The greatest freedom is permitted in this tribe between *ɳätjä* and *ɳätjiäɳ* and between *pola* and *pöliäɳ*, but much license, and a sexual type of joking, is permitted also in each of the first three relationships above. Individuals in any of these three relationships may grasp one another's genitalia and exclaim: *n'yinta ɳätärrä kuntj pi'in-eh?* (you [thou] your penis big-eh?), "You have a big penis, eh?" *Ya'a we'<sup>28</sup> ɳaiya mäny'*! "Not at all I have small one." A *pöliäɳ* may say to his *pola*, *unt titätäm*, "scrotum eggs with;" in free translation "Your scrotum is like a bag with eggs!" And the *pola* will reply *n'yinta titä kenkänäké?* (you eggs cook, eh?), "Would you like the eggs to cook?"

After every sally of this kind each man may hold his hand out towards the other with a laugh, and each cries *Eh!* just as in the terminal rite of an initiation ceremony or other ritual performance.

For a woman the chief joking relationships are:

1) *mukka*, but not *kala*, who is *ɳaintja taiyìn* (severely tabu) and to whom she may not speak at all.

2) *ɳätj wqt* and *ɳätj waiyo<sup>29</sup>* mother's father and mother's father's sister respectively, *mäll tonn*, i.e., classificatory, not actual.

3) *pola* (*pöl wqt*), classificatory father's father, and father's father's elder brother.

4) *kat kall'n* (literally "mother carries") is the term applied to a man who through an irregular or "crooked" marriage with the mother has come to stand incompatibly in the relationship of father (*pip*) to the person speaking.

5) *korp'n*. Mild swearing is permitted in this relationship, but definitely sexual references between individuals of opposite sex are usually excluded from the joking relationship (i.e., from *äkwunäk ke'ätnäk*), and

<sup>28</sup> *Ya'a*, "no, nothing;" -*wé*, a termination carrying emphasis. It must not be confused with -*wi* which, used as a suffix, means something very like please; an expression of entreaty or pleading rather than of emphasis.

<sup>29</sup> *Wqt* is really an old man; with kinship terms it often indicates elder, i.e., *pola*, father's father; *pöl wqt*, father's father's elder brother, as well as a *pola* who is an old man. *Waiyo*, used with terms of relationship applied to women, indicates an elder or more important person, e.g. *mykk' waiyo*, the mother's elder sister.

are generally but not always restricted to times of real anger. The strongest type of joking, usually of a coarse, rather than of an obscene nature, is *otjäm*ti (*otjäm*, "urine"), hence, "plenty urine."

Nevertheless, just as extreme license is permitted between a *ḡätjä* and his *ḡätjiäḡ*, between a *ḡätjianḡ* and his *ḡätjä waiyo* (father's mother's elder sister) a great deal of license is permitted, and references of sexual character, which, as was noted above, are usually excluded from the joking relationship between individuals of opposite sex, are sometimes employed. Thus a *ḡätj'waiyo* may say to her *ḡätjiäḡ*, *kuntj mäny'*, "little penis!" and he may retort *pō'ō mäny'*, "little vagina," a type of jest that is unthinkable in any other relationship.

Behavior within the kinship system of the Wik Monkan tribe is extremely complex, and here again as in the Ompela and allied tribes, a special vocabulary is employed for talk "one side" between individuals who stand in certain relationships within the kinship system. The word for talk or speech, corresponding with *kōkō* of the east coast, is *wik*, and again, as in that area the word for "speech" appears as a prefix to the names of many tribes, e.g. Wik Monkan, Wik Tinda, Wik Älkän, Wik ḡätärrä. Normal speech is *wik kōi'um*, which means literally "talk straight," while "one side talk" is *ḡonk wonk tonn* "speech side another," so that the natives' distinction between "straight talk" and "one side talk" are almost exact translations of their own phrases. While *wik* corresponds with *kōkō*, *ḡonk* corresponds with *ḡornki*, and there is little doubt that it has the same derivation.

The function served by this language is exactly similar to that in the Ompela tribe. When a relation who is *ḡaintja taiyḡn* talks "one side" through a child or a dog, he or she addresses it by the term denoting the relationship in which it stands to the speaker. A dog is *nenk* (son or daughter, man speaking) to its master, who is *pip* (father) to it, while his wife is *kat* (mother) to the dog and it is *tuwa* (son or daughter, woman speaking) to her.

In this tribe, although the pattern of behavior to the mother is still one of avoidance, she may speak "one side" to her son-in-law, using *ḡonk won'k tonn*. Thus, if she wishes to ask her son-in-law for tobacco (normally *mai ken*), she may not use direct talk (*wik kōi'um*) which is: *ḡaindäḡ ḡaiya mai ken ya'a*, "Son-in-law I tobacco nothing," but in *ḡonk wonk tonn*, speaking to her daughter's dog: *kemiäḡ mampi ḡaiya kōn katume*,<sup>30</sup>

<sup>30</sup> *Mai*, the generic name for any and all vegetable foods, is prefixed to the names of foods, etc., of vegetable origin, hence tobacco is *mai ken*. The *ḡonk won'k tonn* for *mai* is *mampi* (cf. *mampandji* of Ompela) and for *ken*, *kōn*, i.e., *mampi kōn*.

"Daughter's son (i.e., the dog is the child of her tuwa), I tobacco nothing." If he has none, instead of replying directly *ɲaiya ya'a*, he again addresses the dog: *ɲaindān ɲaiya katum*, "Son (to his dog) I (have) nothing," or if he has a small piece only he may say *inwé ɲainda wettä*, "Here (is) son no good." Freely rendered "Son, here is a little no good piece."

The following is a list of words in *wik*<sup>31</sup> for comparative purposes:

<i>English</i>	<i>wik koi'um</i>	<i>ɲonk won'k tonn</i>
water	ɲäkk	tjäŋ
vegetable food	mai	mampi
animal food	mīna	nenk
fire	tum	opärräm
head	kutäk	kortra
eye	mer	wälkän
mouth	ta	nämpä
nose	ka	worp
neck	männ	wukal
belly	tip	impän
hand	ma'a	polläm
hungry	martji	paparra
swear	ak, äkän	ɲoi'yin
sleep	werp	ɲant
good	min	wet
spear (generic term)	kek'ä	torkäm
paddle (canoe)	pil'	pemarra
tree, wood	yuk	yurntj
go (I go)	i'äŋ	wen'yäŋ
speak (I speak)	wik tau'wäŋ	ɲon'k'mjinnmäŋ
hair	yäŋän	änkalläm <sup>32</sup>

#### THE JOKING RELATIONSHIP IN THE TJUŋUNDJI TRIBE<sup>33</sup>

The Tjuŋundji tribe of the lower Batavia River area on the Gulf of Carpentaria differs greatly both culturally and linguistically from the

<sup>31</sup> It is of interest to note the occurrence of certain at least of these words where euphemisms are required. Hence a pregnant woman is spoken of, and even addressed as, *impänäŋ*, obviously derived from the *ɲonk tonn* (*impan* "belly," -*än*, "with"). Thus while it is quite proper and correct to address a pregnant woman as *impanäŋ*, to speak of her as *tippiti*, "big belly," would be an insult, or at the least a phrase in very bad taste (*wik waiya*, "bad talk").

<sup>32</sup> This word again occurs in the name *Änkallämwet*, given during mourning to a man or woman who has lost a relation of a certain order, and is related to the fact that the hair must not be cut by an *Änkallämwet* except on certain set ceremonial occasions.

<sup>33</sup> A preliminary account of these people appears in a paper on A Hero Cult from the Gulf of Carpentaria (Journal, Royal Anthropological Institute, in press).

tribes of the south and east, and is very distinct from the Koko Ya'o, Kanju, or Wik Monkan tribes. Nevertheless the use of swearing, and the existence of a joking relationship, characterized by license in behavior and the organized use of obscenity, is well developed.

Swearing in the Tjuṇṇdji tribe is again readily classified under three main headings.

(1) Mild oaths or exclamations, in which the personal name of a person long deceased is called when a weapon or implement breaks or when a man strikes his foot against an object on the ground (see p. 467).

(2) Unorganized swearing, i.e., swearing used in times of anger to goad enemies to fight; falling under no social sanction.

(3) Organized swearing and license in behavior permissible between relatives of a certain restricted class, and taking the form of a joking relationship.

Within the second class of swearing, used in times of anger to goad to fight the strongest and most offensive expressions are used, since here there is no restriction except that imposed by fear of goading an enemy too far. A great number of expressions used only as taunts may never be employed even within the grades of the joking relationship in which the greatest license is allowed.

Thus there is one term which, although it would appear to a white man to be quite innocuous, must never be used playfully since this would invariably lead to a fight. No man who is tjirrita and no woman who is n'dju'upätti or mwäprännä may eat the flesh of any animal that has been killed or even bitten by a dog, nor any man who is ägammäthiggä'oti (father of an infant). But old people—a man who is wängämbwitti (just turning grey) or wegäpwa (old man), and a woman who is ndupriggä (old woman)—may however, eat food that dogs have hunted and killed. When a man who is still tjirrita eats flesh killed by a dog, even by his own dog, he is äthwamäṅgwätti, i.e., one who has eaten the spittle (äthwä) of a dog. This is regarded as a dreadful thing: nevertheless, none but the elder brother of the man, whose special privilege it is, dare apply this term to him, for athwamäṅgwätti is one of the worst and deadliest of fighting taunts. My informant remembered an actual instance in which the use of this word resulted in the death of the user, who was killed instantly, his neck transfixed by a spear before he could even rise to his feet.

There are also a number of other expressions in the Tjuṇṇdji language that may never be used jocularly, even under the extreme license of the joking relationship with the paiyā. While I may playfully say to a man ndru maritji! which means, in free translation, "You are a woman hunter,"

and he may only laugh, and I may even address him jocularly as *ärrega*, "a woman given to free intercourse," or by a still stronger term *winda* (a term equivalent to bitch), I may never say *äreräkwinnä* or *windatakwinnä*. *Kwinna* is a suffix meaning "having birth from," and these terms would have the significance of "son of a harlot," "son of a woman who was a bitch" respectively, and are therefore equivalent to addressing him as the son of a woman who was promiscuous. These are fighting taunts, and especially when used in public—in camp in the presence of others—they are intolerable, hence their use as taunts or goads.

There is a distinction between the terms *ärregä* and *winda*, the latter being much more serious and therefore rarely used as a joke. When I addressed one of the old men playfully as *ärregä*, he laughed; but when I called him *winda*, he cried *iaŋä paithuthi*! "I forehead bow," that is, "I am ashamed."

An oath in the *Tjuŋundji* language is *n'riŋi*, i.e., *n'dru naina ernai n'riŋi*? (thou me why swear), "Why did you swear at me?" But the behavior within the joking relationship is expressed in the phrase *laiŋi pfai'pratti*, "We two (dual) joke."

The marriage permitted in *Tjuŋundji* tribe was with the mother's mother's brother's daughter (*avondji* = *avondji*). The father's father (*erwe*), who is the individual in the *Ompela* and allied tribes with whom the greatest freedom is permitted, is not here a joking relation. *Erwe* is again the name applied to the clan totem as well as to the father's father.

The two chief joking relationships are:

1) *paiya* = *iäŋäpändi* (father's mother's brother = sister's son's son).

2) *ngyŋmra* = *nainän'nriŋännägä* (mother's brother's daughter's husband's = wife's father's sister's son). In each of these relationships considerable freedom is permitted, and the license extends also to the behavior, each of these two relatives being permitted to handle one another's penis. Considerable license is permitted with the *paiyā*, but sexual references are usually excluded. While to a woman he may use only such mild terms as *kwegänna*, "crooked foot" (*kwe*, "foot"), or *n'dwa pärrätji*, "eye big," to a man who stands in the same relationship he may say *n'dru ärregä*, "You (are) a harlot" (*ärrega* is normally applied to a woman; *maritj* to a man; *ärregä* is therefore in this sense a sharper thrust).

#### SUMMARY

1. Swearing and obscenity is of frequent occurrence among the natives of Cape York Peninsula.

2. It is not employed in a loose or haphazard manner, but with re-

straint; it is of several distinct types, each of which fulfils a sociological function.

3. Swearing and obscenity is employed deliberately as a taunt to goad an enemy to fight. This is of an *unorganized* type and falls under no social sanction. This type of swearing is characterized by the Wik Monkan as *ak kulkentanak*, "swearing anger to arouse."

4. In addition to this unorganized type of swearing there is an *organized* type of behavior falling under a definite social sanction, in which license in language and in behavior, of a set and stereotyped form, is obligatory.

5. In each of the tribes discussed in the present paper this takes the form of a joking relationship of similar type to those described from certain other parts of the world, in which it is customary or obligatory for individuals who stand in a certain relationship to jest or to exchange obscene remarks.

6. The behavior within this joking relationship is regulated by kinship. It induces a state of euphoria, of ritual well being, i.e., "it makes everybody happy," and is carried on in camp, in the presence of the group.

7. This ritual aspect of the joking relationship is shown by the fact that it reaches a maximum on ceremonial occasions, i.e., when social sentiments are stressed, as on the *ṇartji kintja*, the tabu ground, during the preparations for the ceremonies of the *Okainta*.

8. Furthermore, any departure from the customary norm of behavior, even if inadvertently committed, is a ritual offence and must be followed by ritual purification.

9. The behavior customary under the joking relationship is of two distinct types, characterized by the native of the *Ompela* and *Kokō Ya'o* tribes as *a'inyu kuna-go*, "play excrement-for," and *a'inyu wona-go* "play penis-for."

10. The greatest license permitted in each case is between individuals two generations removed. Moreover, it is permitted between individuals who stand in a *classificatory* and not in an *actual* relationship with one another.

11. In the joking relationship, which is regulated by the kinship system, the obligatory behavior consists of a reversal of the customary pattern, normally of severe restraint, appropriate to the relationship.

12. This is well shown in the joking relationship which is set up, when, through an irregular marriage, an incompatibility results. The rigid avoidance of the *yami* relationship in the *Kokō Ya'o* and *Ompela* tribes gives place to the license of the joking relationship.

13. In the Koko Ya'o, Ompela, and Wik Monkan tribes, the greatest freedom is permitted with the father's father, called pola, the name applied also to the clan totem. In the Tjuundji tribe the name for the father's father erwe is also applied to the clan totems but the father's father is not here a joking relation.

14. In addition to the rule of complete avoidance towards certain individuals, there are a number of others to whom the rule of behavior is one of partial avoidance; certain of these may be addressed "one side"—that is, they may be addressed indirectly through a third individual: generally a child or a dog.

15. In many of the tribes of Cape York Peninsula, a special language, distinct from that in ordinary use, is employed for relatives towards whom the rule of behavior is one of partial avoidance. This second or "one side" language is distinguished by the Koko Ya'o and Ompela peoples as *ɲonki* from the language of ordinary conversation, called *koko*, and by the Wik Monkan as *ɲonki won'k tonn*, from the ordinary language, *wik*.

UNIVERSITY OF MELBOURNE  
MELBOURNE, AUSTRALIA



## REPORTS

### ANTHROPOLOGICAL SOCIETY OF WASHINGTON

The Anthropological Society of Washington at its annual meeting held on January 15, 1935 elected the following officers for the ensuing year: *President*, Matthew W. Stirling; *Vice-president*, Frank H. H. Roberts, Jr., *Secretary*, Frank M. Setzler; *Treasurer*, Henry B. Collins, Jr.; *Vice-President of the Washington Academy of Sciences*, Matthew W. Stirling; *Members of the Board of Managers*, C. W. Bishop, G. S. Duncan, H. W. Krieger, T. D. Stewart, W. D. Strong.

A report of the membership and activities of the Society since the annual meeting held on January 16, 1934 follows:

#### Membership:

Life members . . . . .	3
Active members..	45
Associate members	8
Honorary members . . . . .	18
Corresponding members..	18
	<hr/>
Total . . . . .	92

#### Deceased:

Active members. . . . .	1
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#### New Members:

Active members..	1
Associate members.	1
	<hr/>

Total . . . . .	2
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The Society lost through death one of the oldest and most devoted active members, Mr Felix Neumann, February 7, 1934.

Members elected during the year were: Dr W. M. Cobb and Mr W. J. Winter.

The financial statement (Treasurer's report) is as follows:

Funds invested in Perpetual Building Ass'n. . . . .	\$1170.43	
21 shares Washington Sanitary Improvement Co., par value \$10 per share . . . . .	210.00	
2 shares Washington Sanitary Housing Co., par value \$100 per share. . . . .	200.00	
Cash in bank. . . . .	228.37	
	<hr/>	
Total.	\$1808.80	
Bills outstanding:		
To American Anthropological Association	\$50.00	
To printer.	3.75	
	<hr/>	
Total . . . . .	\$53.75	53.75
	<hr/>	
Net balance . . . . .		\$1755.05

Papers presented before the regular meetings of the Society were as follows:

January 16, 1934, 649th regular meeting, Indian Food Plants and their Historical Significance, by Dr W. T. Swingle, Bureau of Plant Industry, U. S. Department of Agriculture.

February 20, 1934, 650th regular meeting, The Historical Implications of Some Algonquian Studies, by Dr T. Michelson, Ethnologist, Bureau of American Ethnology.

March 20, 1934, 651st regular meeting, Future Problems in Anthropology, by Dr A. Hrdlička, Curator of Physical Anthropology, U. S. National Museum.

April 17, 1934, 652nd regular meeting, Some Laws of the Early Iroquois League, by President J. N. B. Hewitt, Ethnologist, Bureau of American Ethnology, who gave his retiring address.

October 16, 1934, 653rd regular meeting, Louisiana Relatives of the Ohio Mound Builders, by Mr F. M. Setzler, Assistant Curator, Division of Archeology, U. S. National Museum.

November 20, 1934, 654th regular meeting, Archaeological Explorations in Northeastern Honduras, by Dr W. D. Strong, Anthropologist, Bureau of American Ethnology.

December 18, 1934, 655th regular meeting, How the Northern Indian Hunts by Dr J. M. Cooper, Catholic University of America.

All regular meetings, except the 651st, were held in Room 43 of the U. S. National Museum.

FRANK M. SETZLER, *Secretary*

## AMERICAN ETHNOLOGICAL SOCIETY REPORT OF THE SECRETARY-TREASURER

The annual meeting of the American Ethnological Society was held January 28, 1935 at the American Museum of Natural History, New York City. The following reports of the Secretary-Treasurer were read and accepted:

### REPORT OF THE SECRETARY

To the Officers and Members of the American Ethnological Society:

It gives me great pleasure as Secretary of the American Ethnological Society to present the following report for the past year:

#### *Membership:*

The membership of the American Ethnological Society for the years 1933, 1934, and the present condition in 1935 may be summarized as follows:

1933—Life members 15, Members 38, Fellows 85, affiliated through Central Section and Washington Anthropological Society 6

Total	144
1934—Life members 13, Members 50, Fellows 118, affiliates 6	
Total	187
1935—Life members 13, Members 52, Fellows 112, affiliates 5	
Total	182

The figures for 1935 give the present constitution of the membership of the Society, corrected to January 24, 1935.

The increase of membership in 1934 was the result of a membership drive which added 55 new members and fellows; deaths and resignations reduced this to a net gain of 43.

Thus far in 1935 we have added 7 to our enrollment, but resignations, and the fact that some former members who could not be reached by mail were dropped has resulted in showing a net loss in 1935 up to the date of this report of 5.

It is proposed by the Secretary, with the approval of the Society, to continue our membership drive through 1935.

The Society lost through death in 1934 two valued members of over twenty years' standing in Dr Berthold Laufer of Field Museum of Natural History, and Prof Roland B. Dixon of Harvard University. (Since this report was prepared news has also reached us of the death of Prof Cleland of Williams College in the Mohawk disaster.)

#### *Meetings:*

Regular meetings of the American Ethnological Society have been held on the fourth Mondays of each month at the American Museum of Natural History in conjunction with the Section in Anthropology of the New York Academy of Sciences. The programs of these meetings have been as follows:

February 26, 1934. Genealogies and other history in the Codices of Southern Mexico. Dr Herbert J. Spinden.

March 26, 1934. Law in Primitive Culture. Prof Karl N. Llewellyn.

April 23, 1934. Native Life in West Africa. Prof Melville J. Herskovits.

October 22, 1934. Race Mixture in Brazil. Mr Ruediger Bilden.

November 26, 1934. Some Problems of Northeastern North America. Prof Frank G. Speck.

January 28, 1935. Saulteaux Conjuring. Prof A. I. Hallowell.

#### *Publications:*

Volume 15 of the Publications of the American Ethnological Society, Zuni Texts by Ruth Bunzel, was distributed early in the year.

Volume 16 of the Publications, Cree Texts by Leonard Bloomfield, was sent to be printed and issued in October and should reach members any day.

In view of the favorable financial balance shown by the Treasurer's report, submitted herewith, the Editor proposed that a volume of Caddoan Texts by Gene Weltfish be printed as Volume 17, the 1935 volume of the Publications, the cost of printing to be defrayed from the current funds of the Society, which the Treasurer has assured the Editor should by the close of 1935 be ample. This proposal was communicated to members of the Executive Council and has been approved by them.

The sales of our Publications were materially increased in 1934 by a concerted effort at circularizing individuals and libraries. Special prices for sets were made and special discounts were allowed, with the approval of the Executive Council, and the results have been gratifying. The sale in number of volumes, without the distribu-

tion of the 1934 volume as yet unissued, has amounted to 298 volumes. Sets sold, and the fact that many libraries completed their files of the Publications, have increased the sale of Volume 16 to libraries when issued from 26 to a probable 50 or more. In addition we have begun a list of individual subscribers in advance of publication, which now numbers 13. The probable pre-publication sale of new issues of our Publications has therefore been advanced from 26 to 63 or more, an increase of 37.

It is proposed by the Secretary, with the approval of the Society, to continue circularization in 1935 in the interest of increased sales of back volumes, sales of sets, and an increase in our list of subscribers.

Respectfully submitted,

ALEXANDER LESSER, *Secretary-Treasurer*

#### REPORT OF THE NOMINATING COMMITTEE

On motion of the nominating committee, the officers and directors for 1934 were reelected for 1935 by vote of the members of the society. These are:

President: Dr Gladys Reichard, Columbia University

First Vice-President: Dr Elsie Clews Parsons, Harrison, N. Y.

Second Vice-President: Dr Bruno Oettinger, Columbia University

Secretary-Treasurer: Dr Alexander Lesser, Columbia University

Editor: Dr Franz Boas, Columbia University

Directors: Dr Clark Wissler, American Museum of Natural History

Mr Clarence L. Hay, American Museum of Natural History

Dr Ruth Benedict, Columbia University

#### REPORT OF THE TREASURER

February 27, 1934 to January 23, 1935

##### CURRENT FUND

##### *Receipts:*

Balance, Current Fund, in Corn Exchange Bank, February 27, 1934 . . \$1470.50

Dues received:

1931, Fellow . . . \$ 6.00

1933, Members and Fellows . . . 28.00

1934, Members and Fellows . . . 1088.65

1935, Members and Fellows . . . 336.00

Affiliates, Central Section and Washington

Anthropological Society . . . . . 19.00

\$1477.65

Interest on N. Y. Mtg. Bond Co. bonds, to January 16,

1934 . . . . . 144.36

Interest on U. S. Government bond, 1934 . . . . . 21.25

Sales of Publications of AES, through Stechert, net . . . . . 689.00

2332.26

Credit, checks issued, uncollected to date	12 50
	<hr/>
	\$3815.26

*Expenditures:*

To American Anthropological Ass'n., 1933	\$ 10.00
To American Anthropological Ass'n., 1934	813 25
To Brill of Leyden for 20 copies each, AES Publications I and VII (pt. 1)	101 43
To J. J. Augustin, shipping, Publications XIV	72 80
Expenditures on meetings AES	35.29
Supplies	2.63
Printing stationery and circulars	129.05
Clerical and secretarial assistance	63.86
Postage	87.46
Mailing lists	3.90
Rent, safe deposit vault	6.60
Refund	5.00
Check taxes, collection charges	1 59
	<hr/>
	\$1332 86
Balance, Corn Exchange Bank, January 23, 1935	2477.75
Petty cash on hand	4 65
	<hr/>
	\$3815 26

## PERMANENT FUND

## Statement of Assets and Liabilities

<i>Assets</i>		<i>Liabilities</i>
3 N. Y. Mtg. Bond Co. bonds (par)	\$3,000.00	\$0 00
1 U. S. Govt. bond	500.00	
	<hr/>	
Net assets, permanent fund	\$3,500.00	

*Remarks on the Treasurer's Report:*

The balance of \$2477.75 in the current fund at this date is not the net worth of the current fund. Against this must be reckoned: That the membership was billed promptly after the first of the year for 1935 dues, and \$336.00 in dues has been collected and deposited against which no payments have yet been made to the American Anthropological Association. Furthermore, the cost of publication of Volume XVI, about to be distributed, has to be met from this balance. Finally, there are a few unpaid bills: \$26.00 for printing stationery being used in the 1935 membership and sales circularization; \$2.00 due Brill of Leyden for a volume of the back Publications; and \$23.75 owing the American Anthropological Association on our 1934 accounts. Credits due the American Ethnological Society in the form of unpaid dues, unlisted in the balance sheet, amount to over \$140.00 of which, in the Treasurer's judgment, not less than \$85.00 is ultimately collectible.

As noted in the Secretary's report, membership and sales circularization was carried on extensively in 1934. This accounts for the total of \$280.35 in the three items of printing, postage and clerical assistance. In the normal first year of a Secretaryship, the cost for these three items has been about \$50.00, so that the net cost of our drives in 1934 has been about \$230.00. The results are worth recording: In membership there was a gain of 55, and a net gain of 43; in sales, there was a net return (without the distribution of Volume XVI, the 1934 volume) of \$689.00 through Stechert, as against \$64.05 in 1933; and finally, the subscription sale of Volume XVI and future volumes as issued has been raised to a probable total of 63, as against a former subscription list of 26.

In the belief of the Secretary-Treasurer the expenditures have justified themselves, and it is proposed with the approval of the Society to continue circularization efforts the coming year.

Respectfully submitted,

ALEXANDER LESSER, *Secretary-Treasurer*

Audited and Approved by DR RUTH BENEDICT and DR CAROLYN ADLER, *Auditors*

## BOOKS REVIEWS

### NORTH AMERICA

*Spider Woman. A Story of Navaho Weavers and Chanters* GLADYS A. REICHARD.  
(xi, 287 pp., 29 illus. \$3.50. New York: Macmillan Co., 1934)

The literature of the American Indian can be divided broadly into two classes, the scientific and the sentimental. Dr Reichard has kept to the middle ground, giving her reader both instruction and understanding in the guise of a day-to-day narrative of Navaho life.

She has gone out alone to live with a Navaho family and learn to weave. Recognizing her sincerity, the family makes her a part of itself. She is "daughter" to old Red-Point, the chanter, and his wife Maria Antonia, "sister" to their three grown-up daughters and to every clanswoman of her generation. No theatric ritual is involved, no hint of the formal tribal adoption so dear to the sentimentalists. This adoption is bona fide and utterly simple.

She uses them to give us a charming picture of many aspects of the hard-working pastoral life of the tribe. The men busy themselves with livestock and farming, the women with household chores, the children with pet-teasing and play, and learning to be grown-ups. Joys and sorrows mingle in the story: a marriage ceremony, long chants to cure the sick, the family grief at the death of Maria Antonia. With much conversation and excellent background description the author depicts real people.

Weaving is the main theme of the book. Four summer seasons and four blankets span the author's hard apprenticeship to the great tribal craft. At the end she confesses that she could never make a living at it, although a capable weaver at last. Thanks to her entirely subjective treatment of this topic, we can understand and appreciate the conclusion. Here for the first time is a thorough analysis of the personal element in weaving, the many technical difficulties that attend the making of a Navaho rug.

"Spider Woman" should be judged primarily as literature, for the author has worked conscientiously to make it that. Barring a little evident piecing-out at the end, her success I should call complete. For those who want ethnology it is there in plenty, particularly in description of ceremonials and in those matters which ethnologists are wont to note without explaining: how mothers-in-law are avoided gracefully, why formal names are not commonly used, how children are reared without punishment. To say that it is disguised with human interest would be unjust; rather is it completed by a re-creation of its living, human setting, as ethnology deserves.

The belligerent tenor of the copyright notice is most ungracious in a field of activity in which a free interchange of published information is traditional and particularly in the record of a project financed in part by a society subsidy. This, I think, is the publisher's doing, not the author's.

CHARLES AMSDEN

SOUTHWEST MUSEUM

*Archaeology of North America.* PAUL S. MARTIN (Guide, Field Museum of Natural History, Department of Anthropology, Part 2. 122 pp., 10 figs., 8 pls., map. Chicago, 1933.)

This publication was prepared primarily to serve as a floor guide for museum visitors interested in the historical and cultural settings of the peoples represented by the exhibited materials. Following an introductory chapter on the origin and antiquity of the American Indians, the author defines cultures and describes the culture areas north of Mexico. With the exception of the final chapter, which lists the exhibits in the Hall of North American Archaeology, case by case, the remaining chapters are concerned with types of features and artifacts, treated as separate subjects, with historical and cultural comments. A glossary defining terms employed in the guide, and a bibliography of selected authors complete the work.

As a brief, concisely stated review of essentially important facts and theories serving to illuminate and evaluate the exhibited materials, the guide will serve its intended purpose to good advantage. One inclined to be critical will encounter no great difficulty in discovering in its pages inaccurate and controversial statements, a number of which are apparently the result of careless editing, but on the whole the information has been gleaned from the published works of qualified students, and the contribution reflects credit upon the author.

The reviewer regrets Martin's use of the outmoded, highly inaccurate culture-area concept, particularly unsuitable for archaeological classification because of its time-rigidity. A treatment of the subject stressing culture complexes, rather than areas and types of features and artifacts, as such, would have been preferable. However, there are those who will disagree with me in this, and the very newness of the suggested method may excuse the use of one long accepted.

W. C. MCKERN

MILWAUKEE PUBLIC MUSEUM

*Tobacco, Pipes and Smoking Customs of the American Indians.* GEORGE A. WEST. (Bulletin, Public Museum of the City of Milwaukee, Vol. 17. 2 vols., 994 pp., 17 figs., 257 pls., 19 maps. \$12.00. Milwaukee: Public Museum, 1934.)

The author of this ambitious two-volume contribution, while faced with the magnitude of the undertaking, has chosen an enviable subject for consideration. Tobacco and its utilization not only represent an outstanding trait of human culture but furnish a striking example of the spread of an element through diffusion. Nevertheless, the story of tobacco has not been accorded the attention which it merits; and aside from McGuire's early treatise and such brief studies as those of Laufer, Linton, Mason and some others, little has been published up to the present.

Volume I carries the text matter, while Volume II is given over entirely to maps and illustrations. The text discusses the historic discovery of tobacco and its spread over the world, the botany of the plant, with a map indicating aboriginal use of the several species, and a discussion of usage by the American Indian. Then follows the author's exhaustive classification of smoking tubes and pipes, comprising some thirty distinctive types, and a consideration of pipe materials and methods of



manufacture, based on personal experimentation. In addition to the usual index, the author has provided finding lists based on localities, collectors, and collections. The illustrations are numerous and acceptably good, as are the maps, particularly those indicating distribution of tubes, pipes and other smoking devices. Students of the material evidences of the use of tobacco in America will have noted therein apparent evolution, from south to north, of aboriginal smoking methods: in the extreme south, tobacco leaves merely rolled together, cigar-like; proceeding northward, the cigarette, with wrapper of maize, palm, or material other than tobacco; cane or reed cigarette tubes; stone smoking tubes, straight and curved; and, lastly, the conventional tobacco pipe, in its many types. Regardless as to whether there may be anything factual in this apparent progression, or whether the evidence is merely fortuitous, availability of maps showing distribution of the several methods of use are essential to further consideration of the possibility.

From the ideal standpoint "Tobacco, Pipes and Smoking Customs" leaves much to be desired—a fact which doubtless is as apparent to the author as to the reader or the reviewer. Nevertheless, the author's lifelong interest and pursuit of his subject at the expense of considerable time and money are widely known and generally applauded. Confronted with a task too great to be compassed within an average lifetime, he naturally and wisely decided to do what he could. It would seem rather late to refer to this effort as a pioneering one; nevertheless in a sense it is just that; and certainly there should be nothing to prevent future students from taking up the work where the author left off and doing what he doubtless would have done and perhaps yet may do.

Better organization and arrangement of the text, improved sequence of its presentation, more careful study and interpretation of the material evidences on which the contribution is based, are perhaps the more important considerations in forthcoming editions.

H. C. SHETRONE

OHIO STATE MUSEUM

*The Sanpoil and Nespelem: Salishan Peoples of Northeastern Washington.* VERNE L. RAY. (University of Washington Publications in Anthropology, Vol. 5. 237 pp. \$2.00. Seattle: University of Washington Press, 1932.)

*The Lummi Indians of Northwest Washington.* BERNHARD J. STERN. (127 pp., 6 pls. \$2.00. New York: Columbia University Press, 1934.)

The first of these is a carefully documented study, replete with direct quotations from informants, and giving adequate references to significant comparative material. Except for kinship terms and mythology (which are specifically reserved for separate studies) every aspect of individual and social life, the material culture and geographical data are covered with thoroughness. On almost every page is evidence that the author has taken pains to secure every possible bit of information on the topic in question. And on almost every page are evidences of full sympathy and understanding between investigator and informants. Ray's study should take its place alongside Teit's works on the Thompson and Shuswap as a source for the Plateau area.

Stern's study, on the other hand, is decidedly inadequate, judged by modern standards of ethnographic research. A tribe whose culture is still relatively alive (p. 9) cannot be satisfactorily described in 103 pages (exclusive of mythology, which occupies sixteen pages) even though those pages are dignified by being divided into fourteen chapters. The author's description of the cycle of life (six chapters, 25 pages) and of the potlatch festivals (18 pages) are far above the standard of the remaining sections. But in material things, particularly that part denoted "Getting Food in its Season" (Chapter VII) he displays what can only be described as naiveté. I offer the following examples:

The horse clams also are found on sand bars. As one walks along the bar at extreme low tide, one sees round formations on the sand. When these are touched there is a spurt of water and the clam pulls its head down. They work themselves down into the sand very quickly, so one must dig fast in order to catch them (p. 47).

Deer are caught with bows and arrows when a man hunts alone, otherwise by pits and by nets made from the fibres of the spinal cord of the deer (p. 48).

A good spearman usually strikes the spinal cord of the fish and kills it at once. If the prong enters the sides of the salmon, the fish may jerk them off the pole when swimming (*sic*) away, but the wooden buoy marks its course and it is easily overtaken and killed (p. 51).

Smelts are gathered all along the shores. they are shaken off floating sea weed to which they are attached when the weeds are thrown on the beach by storms (p. 51).

Some references to previously published material on the Lummi and some indications of knowledge of the culture of surrounding groups would have added some merit to the volume. As it stands, it is at best a sketchy account which for this and other reasons contrasts most unfavorably with Ray's description of the Sanpoil and Nespelem.

R. L. OLSON

UNIVERSITY OF CALIFORNIA

*The Carrier Language (Déné Family), a Grammar and Dictionary Combined.* A. G. MORICE. (2 vols., I. xxxv, 660 pp.; II: 691 pp. RM 80. Anthropos Linguistische Bibliothek, IX u. X Band. Modling bei Wien: Verlag der Internationalen Zeitschrift "Anthropos," 1932.)

This magnificent work, as its sub-title indicates, is not an ordinary grammar but also a dictionary, the lexical materials being skilfully disposed under appropriate grammatical rubrics. The "Vocabulary" at the end of the second volume, with its page references, helps the student to find his way in these materials. There is no confusion, only a mutually fertilizing treatment of the complex grammatical forms of the language and its lexical content. Not only is such an interweaving of grammar and dictionary allowable for Carrier and its cognate languages, it is in many respects necessary, at least if the grammatical survey is to be complete and definite. In all the Athapaskan languages many complicated grammatical rules apply only to single "words" or to small sets of words. One cannot, therefore, give as adequate a notion of the more intimate structure of Carrier or Kutchin or Navaho with a schematic statement of processes and categories as is possible in such languages as Yokuts or Arabic or Jabo (in Liberia), in which grammatical principles, once mas-

tered, can be applied with a high degree of confidence to the words or word elements. It is not, of course, a question of the relative complexity of the grammar as of the relation of the grammar to the vocabulary. Father Morice's "Carrier Language" deserves to rank as a real contribution to linguistic method. So far as I know, the problem of handling grammar and vocabulary as a formal unity has never before been attacked in so original a manner nor with so sure an instinct. Incidentally, this single attack on a dual problem seems not unsuited to the genius of English, different as its grammatical contours are from those of Carrier.

The body of the work consists of a preliminary treatment of Phonetics; Part First, "The normally non-verbal Parts of Speech" (subdivided into "The Noun," "The Adjectives," "The Pronouns," "The Postpositions, Conjunctions and Interjections," and "The Adverbs"); Part II, "The Verb morphologically considered" (subdivided into "The Verbal Stems," "The Verbal Prefixes," and "The Incorporating Verbs"); Part III, "The Verb grammatically considered" (subdivided into "General Notions," "The chief irregularities of the Verbs," "Grammatical Divisions," "Personal Divisions," "Modal Divisions," "Morphological Divisions," "Temporally incomplete Verbs," "Divisions based on Endings," and "Verbs with number-indicating Endings"); Part IV, "Syntax and Linguistic Peculiarities" (subdivided into "Syntactic Notes" and "Linguistic Peculiarities"); Part V, "Texts" (five texts with interlinear and free translations), and Part VI, "Vocabulary" (an English check list). This list of the main headings will serve to give some idea of the scope of the work. Details are hardly in place here.

It should be remembered that the strength of Father Morice's grammar lies in its wealth of descriptive detail, not so much in its ultimate configurative analysis. A comparative student of Athapaskan may want to reassemble much of the detail and redefine some of the fundamental outlines but he will always be grateful to Father Morice for the facts, of which he has an obvious mastery. One weakness of the work—which is, however, not an important drawback in practice—is that the orthography is not sufficiently systematized from the phonemic point of view. Thus, both *t* and *d* are used for what is obviously a single phoneme, a lenis stop, while the corresponding aspirated fortis stop is written *th*. One might write these phonemes, with advantage to economy and phonemic accuracy, either *d:t* (defining *d* as a voiceless lenis, varying to the ear between French *d* and *t*, and *t* as a strongly aspirated stop) or *t:t'*. Either method is justifiable and adequate, whereas *d:t:th* corresponds only vaguely to the true facts; *d:th* would be better but uneconomical. It is time that linguists realized that one of their first and most fundamental problems is to interpret the mass of purely phonetic data in terms of a phonemic symbolism that is as simple, as economical, and as powerful as possible. This ideal is rarely attained, yet its realization is implicit in the phonetic facts themselves.

There are many ethnological remarks and implications scattered in the book. The anthropologist's attention should be called to the list of ethnological references on pages xxix–xxxv of the first volume.

E. SAPIR

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- 11 *Blackfoot-English Vocabulary Based on Material from the Southern Peigans*. C. C. UHLENBECK AND R. H. VAN GULIK. (Verhandelingen der Koninklijke Akademie van Wetenschappen te Amsterdam, Afdeling Letterkunde, Nieuwe Reeks, Vol. 33, No. 2, 380 pp. Amsterdam: N. V. Noord-Hollandsche Uitgevers-Maatschappij, 1934.)

This volume is a companion to "An English-Blackfoot Vocabulary" by the same authors, issued in 1930. We may congratulate ourselves on the body of linguistic material on the Blackfoot, especially by the senior author. For comparative purposes these volumes will be indispensable. Blackfoot is a divergent Algonquian language, and thus far only a comparatively small number of Algonquian etymologies have been given. It is therefore a pleasure for me to say that in these vocabularies I have noted over 300 items for which Algonquian etymologies are very obvious. It also appears that the divergent character of Blackfoot is in large measure due to numerous and rather complex phonetic shifts which are of wide application. Analogy and grammatical specialization have also played their part. References to the published texts by page and line would have been a useful addition. However, we have a fine tool before us, and let us hope that a Blackfoot grammar will soon come from the pens of the authors.

TRUMAN MICHELSON

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- The Diabolical Root. A Study of Peyotism, the New Indian Religion among the Delawares*. VINCENZO PETRULLO. (xi, 185 pp., 1 fig., 10 pls. \$2.00. Philadelphia: University of Pennsylvania Press, 1934.)

In view of the rather alarming title of this volume it is reassuring to find that Mr. Petrullo has been willing to record the facts concerning Peyotism as he observed them, to relate the teachings and rituals of the cult as he was told them, and then to draw his conclusions without preachments.

An impartial reading of the evidence, however, discourages the idea that Peyotism is an insidious evil, dragging the poor Indian down to ruin; but reveals it rather as a medico-religious cult with dignified ceremonies and altruistic teachings, offering to the distraught Delaware a spiritual refuge amid the wreck of his own cultural heritage.

To offset the undeniable fact that the cactus, Peyote, consumed by the worshippers during the ceremonies, contains a narcotic drug, it is equally true that while "the remarkable properties of Peyote are at the basis of the religion . . . their interpretation is in fully religious and not narcotic terms." To this the reviewer will add from his own experience that the after-effects of the drug are, as a rule, negligible when proper care is taken to make them so, and that it is not usually habit-forming.

The book consists of five parts: first, an Introduction describing the nature and effects of Peyote, the history of Peyotism in general, and an account of the Delaware Indians whose adoption of the Peyote cult forms the subject of the volume.

Part II relates in detail the history and nature of Delaware Peyotism, including legends of its origin, the introduction of the two Peyote sects, one of which has Christian teachings, the other strictly native; and finally a series of narratives dealing with Peyote experiences. Thus far the book has been descriptive; now in Part III we find a study of the dissemination and development of the cult, the limits of its variation in ritual and doctrine, its assimilation with the ancient religious ideas of the people, and finally the above-mentioned Christian influences. Part IV contains the summary and conclusions in brief, and this is followed by a bibliography.

As for illustrations, a crude drawing of the interior of a Peyote tipi forms a decoration for the title-page; then there is a plate showing the Peyote cactus plant entire, the dried "buttons" eaten during the rites, and living plants in containers. Another plate offers portraits of three Delaware Indian leaders of the cult, while the remaining plates, photographic and drawings, all show variations of the "moons" or altars about which the ceremonies center. The museum man looks in vain, however, for a plate showing the paraphernalia—staff, drum, fans, etc.—and the reviewer thinks that a picture of a Peyotist in costume as he remembers them in the attitude of singing would have been a desirable addition.

Be that as it may, in the reviewer's opinion Mr Petrullo has achieved an appealing human document, and at the same time has made a valuable scientific record. From it we may learn, among other things, one process by which rituals and creeds may be transferred from one tribe to another and become naturalized in their new surroundings.

M. R. HARRINGTON

SOUTHWEST MUSEUM

*Indian Life of Long Ago in the City of New York.* REGINALD PELHAM BOLTON. (162 pp., 23 illus. New York: Joseph Graham, 1934.)

Dedicated to the memory of Alanson Skinner, this book presents the story of the Indians of Manhattan Island and the immediate vicinity in a manner that Mr Skinner surely would have approved. Well illustrated by reproductions of pencil drawings by the author, the text describes from such meagre facts as exist or which may be deduced from comparative studies, the social organization and material culture of the aboriginal inhabitants of the island that now marks the location of a great city. Of particular value are the chapters dealing with Indian names and other Indian place names in New York City. Personal names treated in a chapter of two pages, lists three names two of which have a number of variations on old deeds. It would have been interesting to have listed all the personal names that "appear upon deeds for the purchase of land." While some of the material in the book is speculative and general, there are a number of valuable original observations and good descriptions of implements found on the several sites upon the island. Indeed the map showing the sites in the several boroughs is a contribution to archaeological knowledge. Some of the sites are named for their original occupants and others mentioned as locations. The book will be remembered and used for its unusually

well executed drawings and for the facts it gives concerning names and locations of sites. As a popular handbook it fills a real need.

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## MEXICO AND SOUTH AMERICA

*Archaeological Researches at Teotihuacan, Mexico.* S. LINNÉ. (Publications, Ethnographical Museum of Sweden [Riksmuseets Etnografiska Avdelning], No. 1. 236 pp. 339 figs., color plate, 6 maps. 25s. London. Oxford University Press, Humphrey Milford, 1934.)

Dr Sigvald Linné, in his "Archaeological Researches at Teotihuacan, Mexico," has done Middle American research great service. He has brought the specific problems of the archaeology of the Valley of Mexico into relationship with the more general aspects of Central American prehistory. Through his detailed examination of the material culture of Teotihuacan, he has made a grateful supplement, from the point of view of the field archaeologist, to the somewhat symbolical analysis of Seler<sup>1</sup> and the general description of Gamio and his associates.<sup>2</sup> Dr Linné has also described the culture of Mazapan, a new phase in the cultural development of the Valley of Mexico and, finally, has demonstrated the utility of microscopic analysis as an aid to ethnographic research on pottery.

The foundation of Dr Linné research was the shrewd dissection of two sites in the town of San Francisco Mazapan adjacent to the archaeological zone of Teotihuacan.

It is clear to see from Dr Linné's research that the division of the archaeology of the Valley of Mexico into Archaic, Toltec, and Aztec, although useful and easy to remember, hardly expresses the stages in the development of material culture over long periods of time. A subdivision of the stages in such an evolution is necessary before one can begin to plot relationships with other areas and thereby subject such an excavated history to the limits of a measured chronology. Thus a detailed study like Dr Linné's work at Xolalpan establishes various levels of influence, as for example his determination of Maya influence in certain types of carved pottery vases; or the linking of the Mazapan culture with Oaxaca by means of a vessel in tiger shape held in the hand of a representation of Xipe.

It is unfortunate for Dr Linné, and for students of Mexican archaeology in general, that neither Xolalpan nor the archaeological zone of Teotihuacan itself produces a straightforward stratigraphical history of the Teotihuacan culture. To the reviewer, it would seem that the periods laid down for the Xolalpan building present phases of the later part of Teotihuacan history and do not epitomize the complete unfolding of that culture. Nonetheless, to illustrate and describe from any

<sup>1</sup> E. Seler, *Die Teotihuacan-Kultur des Hochlands von Mexico* (Gesammelte Abhandlungen Amerikanischen Sprach- und Altertumskunde, Vol. 5, pp. 405-585, Berlin, 1915).

<sup>2</sup> M. Gamio, *La Poblacion del Valle de Teotihuacan* (3 vols. Mexico, 1922).

time level at Teotihuacan such an imposing body of material, is inestimably useful.

The description of the Mazapan culture is also extremely interesting and important. Similar material had been previously noted in collections, and was usually assigned to the egregious "Toltec" culture. Stylistically the Mazapan pottery is different from that of Teotihuacan, and as Dr Linné at Xolalpan and the reviewer on the archaeological zone of Teotihuacan found this material overlying the Teotihuacan remains, the Mazapan culture must be a distinct entity in itself. The reviewer also found Mazapan pottery beneath layers of Aztec refuse. Thus the gap, sometimes known as the "Chichimec" period, which intervenes between the end of the "Toltec Empire" and the rise of Aztec civilization has some representation in the excavated aspects of Mexican history. Furthermore the work of the archaeologists of the Mexican Government at Tenayuca has added another culture phase, different from Mazapan but also pre-Tenochtitlan Aztec, to clarify this somewhat nebulous period in Valley of Mexico culture. The lines of written and traditional history and the stratigraphical ceramic levels are at last beginning to converge in Central Mexico.

Besides this treatment of archaeological and historical problems, Dr Linné gives microscopic analyses of several potsherds. The results indicate that one ware, yellowish-red, was made of a non-volcanic clay, while another, black, was made of volcanic. This condition led Dr Linné to believe that the yellowish-red pottery was made outside the Valley in non-volcanic country and brought in by trade. Yet according to the observations and sherd-counts of the reviewer, yellowish-red is one of the commoner service wares at Teotihuacan sites, so that it appears unlikely that the vessels were imported in bulk. It would be far more probable that the clay was extracted from some non-volcanic formation in or near the Valley and the vessels manufactured in the various Teotihuacan sites, so that the ware would thus be in style and occurrence a purely Teotihuacan trait. If a fair sample of Teotihuacan pottery had been studied, this misleading result would not have been reached. However, if the conclusion derived from this particular analysis be faulty in view of unpublished data inaccessible to Dr Linné, the use of the microscopic method is highly significant, and makes one more step toward removing archaeology from the realm of opinion to that of fact.

The greatest achievement of Dr Linné from the viewpoint of the general student is the presentation of a technical field excavation in such terms that anyone can follow his text and see the relationship between the details of position of specimens and the larger problems of history and anthropology. Since the works of Beuchat,<sup>3</sup> Joyce,<sup>4</sup> and Spinden,<sup>5</sup> there have been no means for the person not a specialist to keep abreast with technical studies of Mexican and foreign field workers on the archaeology of the Valley of Mexico, the most intensively studied area in Central

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<sup>3</sup> H. Beuchat, *Manuel d'Archéologie Américaine* (Paris, 1912).

<sup>4</sup> T. A. Joyce, *Mexican Archaeology* (London, 1914, 1920).

<sup>5</sup> H. J. Spinden, *Ancient Civilizations of Mexico and Central America* (Handbook Series, American Museum of Natural History, No. 3, 1917, 1922, 1928).

America. Therefore Dr Linné should receive the gratitude of all Americanists, not only for his great contribution to the field archaeology of the Valley of Mexico, but also for his clear and erudite introduction to the history of Mexico before the Spanish Conquest.

G. C. VAILLANT

AMERICAN MUSEUM OF NATURAL HISTORY

*Red Destiny*. MELVILLE J. HERSKOVITS AND FRANCES HERSKOVITS. (343 pp. 28 illus. \$3.00. New York and London: McGraw-Hill Book Co., 1934.)

Some investigators have come to a reasonable understanding of Negro traits and culture in the United States. Others have studied various aspects of Negro life in Africa, but few or none have been able through personal contacts to bring together significant traits of Negro culture in the United States, Africa, the Caribbean, and in Dutch Guiana for contrast and comparison *in toto*. Dr and Mrs Herskovits in this study of the Saramacca tribe of Bush and coastal Negroes of Suriname have moved a step forward in the direction of this all-sided point of view.

Their study is likewise as valuable from the viewpoint of Negro backgrounds as it is from the mustering of present day culture traits of dispersed Negro groups. Inadequate documentary evidence of the precise African ancestry of American Negro slaves forces the anthropologist to comparison of present Negro cultures in Africa and in the New World for secondary genealogical evidence. However native wars, conquests, and European contacts in Africa have made the Africa of today far different from the Africa of slave-trading days. It is precisely here that the present study is of considerable value because in the Guiana bush a considerable element of Africa of the seventeenth century was blocked off from outside contacts and remains today, basically uninfluenced by either white or Indian culture. African gods still abound, there are Nyanzompom, Legba, and others—and remnants of African sacred dances live on, resembling in part certain features of religious dances among our own Negroes.

The book is not a conventional ethnographic treatise, but rather an informal account of Saramaccan beliefs and practices pieced together in a very readable fashion by the chronology of Saramacca travel. Although native and village names have at times been disguised, strict veracity of episode is secured by including nothing either in descriptive detail or in thought which was not directly witnessed or obtained from informants.

The foreground of the spiritual life of the bush is dominated by death and *kunu*. *Kunu*, probably derived from the Ewe word *kunu*, 'death,' is a supernatural force controlled by the ancestors and gods, which punishes with extinction the families of those who violate the laws of the ancestors. It is *kunu* which enforces the tribal law of succession and inheritance through the temple fire and the greatest *kunu* comes from incest. The hooded drum of the Caribbean is good magic in the Saramacca bush, used for healing, guarding, protecting, and so on. Similar to the hoodoo charms of the Negroes of the United States, they enter into the practical everyday existence of the people, although not possessing the overwhelming importance of



the great spirits. At least some of them require spraying with rum, as in our own South and in Africa, although the use of red cloth coverings seems not so important in the Guiana bush. In the training of the obia man there "was something which approached the formal education of our own civilization."

In general the tone of Saramaccan culture seems more that of the African Negro than of the Negro of our own country. Aside from the obia charms and the religious dances previously mentioned, other possible resemblances between Negro culture in Guiana and in the United States are the common use of the word *pinda* for peanuts, the use of secret "day names" for children, the use of song as a means of communicating with the gods, and the common practice of interpolations in speech and in ceremonies.

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### OCEANIA

*Beiträge zur Ethnographie des Papua-Golfes, Britisch-Neuguinea.* PAUL WIRZ. (Abhandlungen und Berichte der Museen für Tierkunde und Völkerkunde zu Dresden, Vol. 19, No. 2, 103 pp., 41 figs., 28 pls., 2 maps. RM. 32. Leipzig: B. G. Teubner, 1934.)

In this publication Dr Wirz, well known for his work in Dutch New Guinea, especially that on the Marind-anim, extends his investigations into the neighboring portion of the British territory, and gives us a general and comparative study of the coastal peoples from the lower fly to the Purari delta inclusive. This is based largely on his journey of 1929-1931, with numerous references to what little literature we have on this region.

There may be recognized in this area at least two physical types, with all degrees of intermixture, while the culture is so complex that any attempt to unravel it is very difficult. Dr Wirz, while referring to other writers for details wherever he can, describes and coordinates as far as possible the various cultural elements, material as well as social, found in the different, fairly well marked subdivisions of the general region, often comparing them also with those of neighboring peoples, especially the Gogodara and Marind-anim, both of which he has more fully described elsewhere.

This is a very important and suggestive study. Our knowledge of the region, however, is very imperfect, and the relationships may not be so simple. While Dr Wirz, for example, includes the Purari delta people, he has not included those further east, whose influence on the inhabitants of the delta can not be ignored.

FIELD MUSEUM OF NATURAL HISTORY

A. B. LEWIS

*Law and Order in Polynesia: A Study of Primitive Legal Institutions.* H. IAN HOBGIN. Introduction by Bronislaw Malinowski. (296 pp., 8 pls., 3 maps. \$3.50. New York: Harcourt Brace and Co., 1934.)

This book really consists of three distinct contributions to anthropology: a theoretical article by Dr Malinowski, a discussion of law and order in Ontong Java,

and a rather brief summary of previously published information on social organization and government in Tonga, Samoa, and Hawaii, with a few conclusions drawn from this material. The introduction and the section on Ontong Java are by far the more important, making the title of the book somewhat misleading.

Most of Dr Malinowski's article is given up to a reformulation of conclusions presented in his earlier book on "Crime and Custom." It provides a valuable statement of the present aims and attitudes of his school. As compared with his earlier writings, it seems to show an increasing recognition of the importance of the biological and psychological needs of individuals in the shaping of social institutions. This portion of the book is strongly recommended to all who wish to keep abreast of recent developments in functional theory.

The section on law and order in Ontong Java is less satisfactory. In spite of the theoretical emphasis of the functional school on the necessity of studying cultures as wholes, most of their published studies have dealt with selected phases of particular cultures. The present work is a case in point. The author seems to have devoted nearly all his attention to those aspects of social life and religious belief which contributed most directly to the solidarity of the group. He gives excellent descriptions of the family, of cooperative groupings, and of tribal ceremonies, but the whole field of personal property is ignored so completely that its existence would be doubted if it were not for incidental references. He repeatedly mentions rich and poor families, but there is no discussion of the relative status of such families or of the frictions which might arise from such economic distinctions. Lastly, there is a single reference to taboos imposed by the chief, with the native name for them (p. 226), but no discussion of what was, elsewhere in Polynesia, one of the most important techniques for social control.

Most of the factual material presented in the last section of the book is already readily available and the reviewer feels that the author's interpretations are sometimes open to question. In particular, his attempts to minimize the importance of chiefs in Tonga and Hawaii, and his doubts as to whether such chiefs were as tyrannical as they have been represented seem unjustified. In both these localities there had been important political changes immediately prior to the first satisfactory European records, changes which had resulted in a centralized and despotic control. Even the position of minor chiefs was quite different at this time, when they were vassals supported by the central authority, from their position in earlier days when they were the heads of local groups and derived their authority from the consent of the governed. All contemporary records agree as to the great power of chiefs and their frequently unjust and tyrannical conduct.

RALPH LINTON

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*Ethnology of Tongareva*. TE RANGI HIROA (PETER H. BUCK) (Bulletin, Bernice P. Bishop Museum 92. iii, 225 pp., 8 pls., figs., maps. Honolulu. Bishop Museum, 1932.)

This volume maintains the high level of Dr Buck's earlier contributions to Polynesian ethnography. The fact that the material was collected in only seventeen days shows how much can be done in a limited time by one already familiar with the language and general culture patterns of the group studied. The description of material culture could hardly be bettered. The account of the old sacred places on the island is also as complete as it could be made without excavations, which the scruples of the natives rendered impossible. In the study of social organization the approach to the formal aspects of the system is that which the natives themselves use. The Tongarevans seem to have carried the general Polynesian pattern of attention to genealogies to unusual lengths. This may be due in part to the fact that all the present natives trace their descent from three original families whose descendants have intermarried extensively so that every native can trace some relationship to every other. A combination of descent and primogeniture, both of the individual and of ancestors in his direct line, determines both the individual's position in society and his rights to land. The importance of any common ancestor in establishing a bond between individuals is emphasized in the relationship terms.

Blood kinship, as revealed by pedigrees is viewed from two important angles, that of direct lineal descent (reckoned primarily in the male line) and that of collateral relationship. When a person uses the terms grandparents, parents, etc., he visualizes five genealogical strata of which he himself forms the middle stratum. . . . A man applies to his collateral relations the same classificatory five strata into which they fall when measured from a common ancestor. Theoretically, it is immaterial how distant the common ancestor is. The counting of the generations from the common ancestor by both parties will reveal the same number, two or one shorter, or two or one longer. The collateral relationship term is indicated by the difference in the count (pp. 26-27).

A somewhat fuller account of actual attitudes and mutual obligations between collateral relatives would have been welcome. The existence of mutual obligations is proved by many incidental references, but it is difficult to tell just what these obligations were or, more especially, in how far they were conscious and formalized.

The section devoted to religion is brief and leaves many points unsettled. Since the island was Christianized in 1856 most of the old beliefs and practices have been forgotten. Dr Buck has had to reconstruct them largely from Lamont's account of his sojourn on the island. However, Lamont deals almost entirely with the more formal and public aspects of religion. The present account makes no mention of beliefs regarding the souls of the dead, their activities, and especially their relations with the living. Moreover, there is no reference to sorcery or to native explanations of the causes of illness. The emphasis which the Tongarevans placed on descent makes the former existence of some sort of ancestral cult highly probable, while the ascription of illness to angered ancestral spirits is a rather common Polynesian pattern. It seems probable that in this case what was originally an important segment of native belief has, of necessity, been omitted.

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## PREHISTORY AND PHYSICAL ANTHROPOLOGY

*Adam's Ancestors: An Up-to-date Outline of What is Known About the Origin of Man*. L. S. B. LEAKEY (xix, 244 pp., 30 figs., 12 pls. New York: Longmans, Green and Co., 1934.)

Africa of late is rapidly yielding up its archaeological secrets. Besides the activities of resident investigators, published mostly in the Transactions of the Royal Society of South Africa, several English, German, French, Italian, and American expeditions have recently surveyed other parts of the vast field, with the result that immense authentic collections are now available and in large part published. Dr. Leakey has himself within the past four years issued two detailed reports, respectively on the "Stone Age Cultures" and the "Stone Age Races" of Kenya Colony, and now seeks, in the light of the newly accumulated facts (including some from Europe and Asia), to restate the entire Old World problem with respect to prehistoric man. The bold title of the book indicates the general tenor of its contents and some of the new facts and interpretations may be expected to excite lively discussion.

Beginning with a brief two-chapter outline of the geological, paleogeographical, and paleontological aspects of the subject, in the course of which he frames for us a new definition of the Pleistocene, his presentation ends with three chapters devoted to a historico-descriptive account of the most important human skeletal finds now on record for Paleolithic times. In between are placed four chapters dealing with the contemporary cultural remains. The principal aim of the osteological portion seems to be the substantiation of the opinion long held by Sir Arthur Keith that modern man, *Homo sapiens*, is of much greater antiquity than has hitherto been generally supposed. Thus the author, in support of this view, brings forward his two most recent (1933) Kenya Colony discoveries, the one a jaw fragment excavated at Kanam (near the eastern shore of Lake Victoria Nyanza) in association with a crude proto-Chellean industry, and the other a number of skull fragments found at Kanjera from two to three miles distant with a typical Chellean industry. Still other recent East African skeletal finds are incidentally mentioned, as for example those of the author at Elmenteita in Kenya (1927) and that by Dr Hans Reck (1913) at Oldoway in Tanganyika Territory to the south, both of which are referred to the Aurignacian culture stage. By means of certain specific osteological characters, previously described and illustrated, these finds, in spite of the obvious Early and Middle Pleistocene age of the two first mentioned as judged by accompanying faunal remains, are declared to be representative not of the Neanderthal but of the Cromagnon species and to be accordingly the oldest known remains of our modern type of man.

On the cultural side Dr. Leakey begins with an interesting exposition on flint-working technique, based at least in large part on his own experimentations. He next gives a brief descriptive account of the successive Paleolithic flint industries, in which are brought together all the latest discoveries and observations by Professor Henri Breuil of Paris. The chief outcome is that the Middle Paleolithic stage,

generally regarded as embracing the Levalloisian and Mousterian industries, has been much expanded both as to time duration and as to differentiation in technique, with the important result that among these very similar flake industries a new phase (with even a sub-phase) has been recognized and named Clactonian, after Clacton-on-Sea, southeast England. The Levalloisian and Clactonian flake industries with their various subdivisions are regarded, on the one hand, as contemporary and as parental to the true Mousterian industry and, on the other hand, as running back to Chellean times, thus paralleling the core industries represented by the coup-de-poing or handaxe of well-known Chellean and Acheulian characteristics. The suggestion is made that these long contemporary but distinguishable flake and core industries are attributable to different racial type groups, the first being the product of the Neandertal man, now extinct, and the second the handiwork of the Cromagnon man, our own supposedly uncontaminated ancestor.

The little book is well written although at times exuberant. For the reviewer its chief weakness lies in the incomplete treatment, both as to text and illustrations, of the newly developed Clactonian and Levalloisian industrial phases. Specialists may question the author's new definition of the Pleistocene and will doubtless take him to task for his opinions about man's relation to nature, the relation of bone to tissue, and especially for his rather positive assertions on such subjects as, for example, patination. But taken as a whole the book is timely and well worth consideration.

N. C. NELSON

AMERICAN MUSEUM OF NATURAL HISTORY

In spite of its general excellence, this is a rather tantalizing book. Omissions are inevitable in a brief treatment of a vast subject. Yet it would be well that the bibliography provide references where the occasional close and inquisitive reader could seek out for himself the answers to questions stimulated by the text. No such aid is given by Dr Leakey to the student who learns (p. 116) that the Levalloisian came into France from the northeast, and that "there is already some evidence to suggest that it had been developing in Germany" during the second interglacial period.

It is also regrettable that the same limitations led the author to omit any discussion of the prehistoric cultures of South Africa, and the Paleolithic of Siberia and the Ordos valley. The remoteness of these regions, and the difficulties of correlating their local cultures with those of the more thoroughly explored areas, should have no terrors for an author who ventures, even with reservations indicated by italics, the statement: "The Choukoutien culture *appears to me* to be closely related to the Icenian of East Anglia" (p. 104).

The presentation of the physical types which have been found in Pleistocene deposits is accompanied by illustrations which are unusually effective. Here, as throughout the book, Dr Leakey has presented fresh views of his material, on which he is to be congratulated. In defining the Neandertal type of man he selects for particular stress the conformation of the tympanic bone, the absence of a canine fossa, and the heavy brow-ridges. He omits two characteristics which are usually

included: the enlargement of the pulp cavities and the condition of platycephaly. Both of these features would exclude the Rhodesian skull, which he considers closely related to the European Neandertal. On the other hand, the Solo finds, whose resemblance to the Neandertal type and to the Rhodesian as well has been pointed out by their discoverers, are referred to our own species.

CHARLOTTE D. GOWER

UNIVERSITY OF WISCONSIN

*The Horse and the Sword* HAROLD PEAKE AND HERBERT JOHN FLEURE. (152 pp., 62 illus. \$2.00. New Haven: Yale University Press, 1933.)

This is the eighth volume in the series "The Corridors of Time" and covers the later half of the second millenium B.C. In the earlier parts of the series a major phase in the development of prehistoric cultures in the Old World was surveyed with conspicuous success in each small volume. But from the opening of the second millenium the increasing wealth of material and the multiplication of specialized cultures greatly complicates the task. Feeling, no doubt, the urge to continue as they began, the authors have been compelled to devote separate chapters to widely contrasted cultural regions and the unity of the book as a whole is inevitably sacrificed.

Since the present book cannot be read satisfactorily without very frequent reference back to the preceding volume, "Merchant Venturers in Bronze," the several chapters of which are continued in this book, it seems unfortunate that the two last volumes were not regrouped on a geographical and cultural basis. The history of the Near East in the second millennium B.C. could then have been traced as a continuous development in one volume and the parallel development of Bronze Age cultures in Europe in another.

The chapters on the Near East during the period covered here involve the difficult task of interpreting in broad cultural terms an increasing amount of literary and epigraphic material which is nevertheless very limited in scope. Despite these difficulties, however, greater prominence might well have been given, in a series of this kind, to the broad economic and social changes at the expense of summaries of the multifarious campaigns and intrigues to which the monuments testify.

The other great source of material for the more western areas, the Greek heroic literature, is analyzed in some detail. A general knowledge of Greek mythology and the Homeric legends is assumed, and the interpretation of the archaeological record in relation to them closely follows Myres' recent work "Who Were the Greeks?"

For central and western Europe the sequence is carried on several centuries later than in the Near East in order to maintain the continuity of the story down to the cultural decline on the eve of the Iron Age. The complexity of the archaeological material for the late Bronze Age in Europe, makes it, "increasingly difficult" to apply "the methods used in this series as later and later phases of cultural evolution come to be treated." The bold treatment of widespread cultures contrasted in economy and equipment must perforce give way to sketches of the distributions

of specialized implements, swords, winged and socketed axes, and fibulae which spread from people to people over wide areas, but concerning whose mode of origin and dispersal there are many unsolved problems and gaps in our knowledge. Since so many sites are mentioned, a key map similar to those provided for the earlier chapters should have been supplied to assist the reader of these chapters, and as the aim, as stated in the preface, is "less to provide a popular account of prehistoric times . . . than to help the serious student, who is not a specialist" and that "even the specialist might . . . be enabled to see his own particular field in more correct perspective" it is most unfortunate that so few of the illustrations are provided with scales or statement of provenance, and that no references are given to the many detailed studies alluded to in the text.

The small compass in which to survey so wide an area has necessarily involved much compression, but the allusive manner in which generalizations and explanations are sometimes introduced is baffling rather than illuminating. One wishes that less space had been devoted to the enumeration of sites and peoples, so that the basis of many suggestive inferences might have been made clear. There is, for example, the suggestion that the development of cities was retarded in Italy despite Minoan and Mycenaean contacts on account of the unsuitability of the region for mixed farming, and again that in the late Bronze Age "increased use of grain seems to have involved, as usual, an increased demand for salt."

C. DARYLL FORDE

UNIVERSITY COLLEGE OF WALES

*From Fetish to God in Ancient Egypt.* E. A. WALLIS BUDGE. (xii, 545 pp., 240 illus. \$7.50. Oxford and London: Oxford University Press, 1934.)

This book was designed to fill the long felt need for a general and popular treatment of Egyptian religion and to supplant demands for "Gods of the Egyptians," an earlier work now out of print, by this great pioneer of Egyptology.

The late author has retained much valuable material from his older works and has also considered results available from research and archaeological discoveries of recent years. The book is divided into an introduction and two parts. Among the more important points in the introduction is the author's interpretation of the inscription of Shabaka (about 700 B.C.). This indicates that a spiritual and philosophical monotheism centered in Ptah, god of Memphis, and that such a concept was far removed from the grossly materialistic doctrines connected with other Egyptian gods. Part I is concerned with a descriptive treatment of divinities, animals, and objects having fetishistic affinities; theological systems and dogmas and their political centers; various kinds and classes of gods, imported foreign divinities; the religions of Osiris and Ra; the underworld as described by "The Book of the Dead" and other funerary works. The arguments advanced for the connection of the Babylonian god Asari with Osiris are very convincing. Part II consists of a carefully selected series of hymns to various gods and a number of the more important religious and creation myths and legends.

The title is somewhat misleading, since the bulk of the contents is concerned with Egyptian religion in general, rather than with the relationship of fetish to god. There is a prolific use of hieroglyphic throughout the text. The drawings are unusual and valuable. One could wish, however, that more information as to the sources from which they were copied and their chronology had been given. Although the work was designed principally for the general reader, it contains an abundance of material of value to the anthropologist and to the Egyptologist. It is an important and welcome contribution to the study of ancient religion.

HENRY A. CAREY

NEW YORK CITY

*Fossil Man in China: The Choukoutien Cave Deposits with a Synopsis of our Present Knowledge of the Late Cenozoic in China.* DAVIDSON BLACK, TEILHARD DE CHARDIN, C. C. YOUNG, AND W. C. PEI. Edited by Davidson Black. (Memoirs of the Geological Survey of China, Series A, No. 11. x, 166 pp., 82 figs., 3 tables, 6 maps. Peiping: Geological Survey of China and the National Academy of Peiping, 1933.)

This publication in summary form of the results of the excavations from 1927-1932 at Choukoutien cannot take the place of the detailed monographs which have appeared and are still appearing in "Palaeontologia Sinica," as Bulletins of the Geological Society of China, etc., but it does bring together in compact form all the major aspects of the relevant data concerning *Sinanthropus* himself and the time in which he lived. The tripartite arrangement of the present Memoir is a happy one due to the special competence of the authors of each section and to careful editing by the late Professor Davidson Black.

Part One considers all the geological and palaeontological evidence of the surrounding region, the cave itself and the associated fissures, past climate and environment, the age of the deposits, their relation to European ones, and finishes with a concise summary. The deposits are older than the Loess, younger than the Samenian (late Pliocene), but share faunal characteristics with both. They are the only true Pleistocene pre-Loessic formations in eastern Asia, but to what part of the Lower and Middle Pleistocene they belong is still an uncertainty. Climatically the conditions in the area were milder than at present but the suspicion of a southern (South Asiatic) connection does not warrant, in the opinion of the authors, any evident relationship of Peking man with *Pithecanthropus*.

Part Two concerns the anatomy of *Sinanthropus*. All of the extant and prepared material at the time of writing is briefly described and compared. The numerous line drawings are helpful and the compact summary which closes this chapter is admirable in the manner in which it recapitulates the descriptive conclusions and the genetic position of *Sinanthropus* relative to other hominids, both ancient and modern.

The cultural remains associated with Peking Man—the stone industry and the much disputed bone artifacts—form the Third section. In addition to the careful consideration of the Choukoutien industry, there is an invaluable brief summary of



the prehistoric sequence in North China of which Figure 82 is a graphic representation of the present information and opinions on this subject. Last but not least, from the standpoint of utility, is a bibliography of 127 items concerning all aspects of the Choukoutien deposits.

It is a modest looking volume in its grey paper cover, but the wealth and lucid brevity of the information contained within it make it perhaps the most important single contribution of those written or inspired by the genius of the late Honorary Director of the Cenozoic Research Laboratory.

THEODORE D. McCOWN

DOWNE, KENT, ENGLAND

*On the Discovery, Morphology and Environment of Sinanthropus Pekinensis.* DAVIDSON BLACK. (The Croonian Lecture. Philosophical Transactions of the Royal Society of London, Series B, Vol. 223, pp. 57-120, pls. 6-15. 15s. London: Harrison and Sons, Ltd., 1934.)

The Croonian Lecture on *Sinanthropus*, in its published form, is similar textually to the second part of "Fossil Man in China." Some slight additions in the form of a brief preface describing the discovery and the field work at Choukoutien, and a concluding section dealing with the age of the deposits, a brief account of the use of fire, the lithic and bone artifacts, and a general summary of the facts and conclusions derived from these round out and amplify what is largely the strict consideration of the anatomy of Peking man. The text figures are the same as those in the above-mentioned Memoir but are reproduced with greater clarity and on a larger scale. The most welcome feature of the published lecture is a series of excellent photographs, particularly of the Locus D and E skulls and of the handful of remains from the body skeleton of *Sinanthropus*.

THEODORE D. McCOWN

DOWNE, KENT, ENGLAND

*A Comparative Study of the Endocranial Cast of Sinanthropus.* JOSEPH L. SHELLSHEAR AND G. ELLIOT SMITH. (Philosophical Transactions of the Royal Society of London, Series B, Vol. 223, pp. 469-87, pls. 53-55. 5s. London: Harrison and Sons, Ltd., 1934.)

The subject of this monograph is the endocranial cast of the adolescent Locus E skull from Choukoutien. It is referred to throughout the present paper as the *Sinanthropus* cast without further specification. The inference from the state of its remarkable completeness that it belongs to the Locus E juvenile skull is confirmed by reference to the photograph of that specimen *in norma basilaris* and to Figure 17 of the paper under review—the defective areas of the base of the skull in both specimens are the same.

The remarkably preserved sulcal pattern is given most detailed treatment in a manner familiar to the many who are acquainted with the extensive researches by Sir Grafton Elliot Smith and his school at University College on the brain of ancient and modern man.

Of greater importance, the authors feel, are the form and proportions of the cast. The comprehensive analysis of these features and the comparisons of the specimen with the endocranial casts of the great apes, *Pithecanthropus*, and a Bushman lead them to the following conclusions. (1) The specimen under consideration is the ideal generalized type of brain from which all the brains of both fossil and modern man can have been derived. (2) The occipital region is identical with that of the apes. (3) Its human status is unquestioned in view of the precocious expansion of the posterior end of the second temporal convolution and of the orbital margin of the frontal territory as well as that of the lower parietal area (supramarginal convolution), the pushing forward of the pole of the temporal region, and the expansion of the mid-temporal area. The development of the first two probably relate to the acquisition of speech and the others to the attainment of a greater ability to perform skilled movements, particularly those of locomotion. (4) The brain of *Sinanthropus* was sufficiently developed to warrant the conclusion that he was capable of the skill and possessed the intellectual capacity to make implements and use fire. (5) The resemblances between *Sinanthropus* and *Pithecanthropus* are close but the former's brain

is definitely more primitive and in an earlier stage of evolution than that of *Pithecanthropus*; that the conditions found in *Pithecanthropus* are all in harmony with the suggestion that *Pithecanthropus* must have been derived from a generalized type. Further, their essential similarity is so marked and they are so closely related that this type is probably *Sinanthropus*. We are also of the opinion that the period of time necessary for the greater development in *Pithecanthropus* could not have been very long (p. 484).

(6) The bilateral symmetry of the endocranial cast of *Sinanthropus* is so marked as to be exceptional.

It is scarcely necessary to emphasize the fact that if the reasoned hypotheses presented above receive general acceptance a considerable number of changes will be required in the at present accepted doctrines with regard to the ancestry of man.

THEODORE D. McCOWN

DOWNE, KENT, ENGLAND

*The Anatomy of the Rhesus Monkey (Macaca mulatta)*. CARL G. HARTMAN AND WILLIAM L. STRAUS, JR., editors. (ix, 383 pp., 128 figs., frontispiece. \$6.00. Baltimore: Williams and Wilkins Co., 1933.)

The cat and the dog, the cow and the horse, and even the humble frog have been made the subjects of elaborate anatomical manuals. No non-human primate has been honored by similar treatment. The literature on primate anatomy is considerable, but fragmentary in nature. In general we encounter a description of a single structure or organ in one or more members of the primate series. Nowhere do we find the complete anatomy of any one animal; the closest approach to this in pre-existing works being Wollard's monograph on *Tursius* and that of Beattie on the marmoset. This dearth of comprehensive accounts seems almost inexplicable in the case of a group of animals of such profound interest to students of evolution.

The present volume marks a great forward step in our knowledge of primate morphology. We have here for the first time a detailed account of the entire anatomy of a non-human primate; and the choice of an old-world monkey as a subject seems especially fortunate, for this family is the most poorly known anatomically of any primate group.

Nineteen competent workers have taken part in this coöperative study, which embraces in sixteen chapters an account of the various organ-systems, together with preliminary chapters on the taxonomy of macaques and growth stages, and an appendix on the housing and care of monkey colonies. As the editors note in their introduction, the work is somewhat unbalanced in treatment. Nearly two-fifths of the volume is occupied by an excellent and detailed account of the skeleton and muscles while the entire central nervous system, for example, receives but fourteen pages, and the circulatory system but little more.

As the editors have noted, the increasing use of the macaque as a laboratory animal was a major factor in stimulating the production of this book. A second field of utility lies in the increasing use of the animal in comparative anatomy courses where the close approach to human structure renders the monkey an attractive object for dissection to the pre-medical student. Still another use for the study of macaque anatomy, with this manual as a guide, has been suggested to the writer. Many students entering research in physiology or bacteriology and not candidates for the medical degree have little need for the minutiae of human anatomy and can ill afford the time required for the usual medical school courses in the subject. For such men the dissection of the rhesus monkey would give in a comparatively short time satisfactory acquaintance with the general features of primate and human anatomy.

Our knowledge of the macaque alone among primates is now placed on a really solid basis. Perhaps it is not too much to hope that this book will serve to stimulate similar studies on representative members of the other primate groups. Then and only then will we be able to discuss the evolution of the primates with proper perspective.

A. S. ROMER

HARVARD UNIVERSITY

*On the Size of Man's Brain, as Indicated by Skull Capacity.* GERHARDT VON BONIN. (Journal of Comparative Neurology, Vol. 59, No. 1, pp. 1-28, 1934.)

Dr von Bonin in this important contribution has attempted to examine how far regression formulæ based on one race approach the results of direct observation on other races. In order to do this he has selected Hooke's formula for English skulls,  $C = 0.000366P' + 198.9 \pm 45.8/\sqrt{N}$ , and has applied it to a large series of racial varieties. He finds, as a result, that most absolute values differ by less than 40 cc. In a few groups, e.g., the Austromelanesians and Aino, the estimates are too high, and in some, from the Himalayas and Tibet, too low. Apart from these few groups, which are subsequently treated separately, the mean difference between the cal-

culated and observed means for the remaining races is  $12.2 \pm 2.3$  cc. with a standard deviation of 25 cc. Hooke's formula thus gives a capacity of between 10 to 15 cc. more than by direct measurement.

The author then considers capacities both in living and fossil races of mankind, incidentally determining what is to be regarded as a small capacity and what as a large, and classifies these races, according to the scheme thus determined, with respect to size of brain. The evidence made available in this way indicates that:

While the human brain is larger than that of our subhuman ancestors, no further increase has taken place since the time of Neanderthal man, and there is a definite indication of a decrease at least in Europe within the last 10,000 or 20,000 years.

The mean skull capacity of Europeans is not higher than the interracial mean, and races of low civilization show very large as well as very small capacities (Eskimos, Kham-Tibetans on the one, Austromelanesians on the other hand). The interracial distribution of cranial capacity is satisfactorily described by the 'normal' law of errors.

All these facts suggest that the size of the brain has become stable, its evolutionary path has turned a corner, and internal organization rather than bulk appears now to be of positive selective value.

M. F. ASHLEY-MONTAGU

NEW YORK UNIVERSITY

#### MISCELLANEOUS

*Dynamics of Population. Social and Biological Significance of Changing Birth Rates in the United States.* FRANK LORIMER AND FREDERICK OSBORN. (xiii, 461 pp., 54 figs. \$4.00 New York: Macmillan Co., 1934.)

This book is a useful summary of recent literature for careful readers. The authors show that population in the United States is rapidly tending to become stationary, but that great differences in the reproductive ratios of certain groups exist. The agricultural South outbreeds the industrial North, cities drop behind rural sections; laborers have more children than do professional workers; while imported stocks tend to fall to the level of native white fertility. The social consequences of such differences are to accumulate surplus population of low economic efficiency, to transmit its blunted capacity to future generations, and so to check national progress. In short, multiplication of poor stock threatens cultural development.

It is admitted that environmental factors can be modified so as to promote more desirable vital tendencies. The writers recommend urban and rural planning, education for parenthood, early marriage, birth control and economic security for children, selective immigration, less prejudice toward colored races, and more wholesome attitudes toward family responsibilities. Doubtless, most of these reforms would benefit society in general. Unfortunately, slight indication of how they may be secured is given.

The work contains many tables and charts of data from good recent sources. There are twenty-three technical appendices, forty pages of bibliography, and two workable indices. The amount of domestic material covered is impressive: the for-

eign literature cited is scant. This restriction is explained by the localized nature of the study.

Whether a satisfactory treatment of the "Dynamics of Population" can be based upon the recent experience of one nation may be doubted. The imperfect record of a mixed stock in a changing habitat during one hundred and forty years is significant, but inconclusive. What biological and cultural transformation has been established by this historical experiment is not yet clear. Empirical results cannot be regarded as adequate expressions of general tendencies. "The Social Significance of Population Growth in the United States" would be a more fitting title.

The authors agree that human behavior is affected by physical environment, biological inheritance, and culture patterns impressed by group life. The interaction of these factors is taken for granted; but the relative importance of each is seldom indicated. Yet if we are to modify heredity by social pressure, we must know how much force to apply. This critical point is one about which writers on eugenics are wont to dogmatize, to speculate, or to argue in a circle. Without clear definition of the variables, we cannot accurately describe their complex functions. Our analysis of the elements involved in heredity and culture is too imperfect to give confidence in applying any formula that combines them. Eugenics and euthenics are still experimental arts.

The writers of this treatise advise improvement of living conditions as a means of increasing the vitality of all resident stocks. Such counsel assuredly advances beyond the inhuman doctrine of natural selection, lately advocated by some theorists.

Attitudes toward racial betterment are formed largely by current practices. To change the latter, we must alter the economy in which they are embedded. In such work, the presentation of data often overlooked, and discussion of their meaning in regard to common welfare, have a part. So investigation of population movements and their social consequences, like the essay under consideration, may help in this orientation. The work of popular writers on genetics, until recently, has been mostly negative. They helped to make the economic position of Malthus on population appear ridiculous. They placed in the hands of intellectuals means for restricting their families. They urged the sterilization of criminals, without understanding the social conditions of crime, and the segregation of feeble minded, without knowing the effects of widely spread negative traits. They put dangerous half-truths into the minds of racial and national bigots. They gave grounds for believing that economic classes are sharply divided by inherited capacity. They often confused native endowment and cultural development. Thus, they set up a pseudo-scientific authority, which intrigued young people and impressed elderly reformers.

Fortunately, we are beginning to recover from the effects of such narrow dogmatism. Lorimer and Osborn call attention to some of these encouraging tendencies. Perhaps their work may help to direct the efforts of students and officials toward more accurate analysis and more constructive action in the field of population growth. Let us hope so.

HOWARD WOOLSTON

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*Selbstmord und Todesfurcht bei den Naturvölkern.* J. WISSE. (548 pp., Zutphen, Netherlands: W. J. Thieme et Cie., 1933.)

After these many years we receive in this capacious tome in the form of a doctor's dissertation the consummation of Dr Steinmetz' suggestive paper on "Suicide Among Primitive Peoples" published in 1894.<sup>1</sup> The author has combed the ethnographic literature of the world and has dealt with instances of this social phenomenon for 375 peoples including even the primitive people in Europe in early historical times. His bibliography is voluminous and includes many early sources. Moreover, he has handled his source material reasonably critically, as evidenced, for example, by his analysis of the meaninglessness of some of Hrdlička's observations in the Southwest and his censure of Westermarck's generalizations on the basis of few and scattered facts. A detailed table of contents is also very helpful.

By far the largest portion of the book (459 pages) is concerned with the enumeration and analysis of instances of suicide in the various primitive groups. The study of the fear of death appears almost too incidental to warrant a berth in the title, though he does show that in most cases where there is little fear of death and (or) a belief in a better life after death, suicide seems to be more frequent.

The author's conclusions are summarized in the last forty pages. He finds that the phenomenon has a world wide distribution among primitive people. Though perhaps not universal, it is certainly quite frequent, thereby disproving conclusively the statement that suicide is rare among them. In a five-page table he lists all the people for whom he has data in one of four columns ranging from those where the rate appears to be very high to those where suicide seems to be very rare. Though subjective factors undoubtedly play a part in the evaluation of the accounts, and the placing of one group here and there may be purely arbitrary, the picture is, nevertheless, on the whole a very helpful one.

The author has taken little account of North American data of the last two decades, but this is not as serious a defect as it seems. The picture for North America would not be materially changed. Moreover, it is amazing how little attention has been paid by present day American students to this reversal of the biologic will to live which may be at once a symptom of disintegrated individual personality and social structure.

While each grouping in the frequency table includes primitive people in almost every cultural level, the lowest cultures do seem to indulge less often than the others. The Vedda, Bushman, Hottentots, and Kuba, as well as Australian tribes and our Californian tribes, fall in the group of low frequency and there appears to be no evidence of suicide among the Yahgan or Andamanese. When suicide does occur it seems very often to take a passive form, i.e., instead of a quick death by shooting, hanging, or drowning, they choose to die through hunger, subjecting themselves to the bite of a poisonous snake, seeking combat with an enemy, etc.

With great pains, Dr Wisse has analyzed the motives that lie behind the cases he has collected, presenting them in tabular form. While such motives as "Religiöse

<sup>1</sup> American Anthropologist, Vol. 7. 53-60, 1894.

Beweggrunde," "Freiheitsliebe und Heimweh," "Liebesmotive," etc., are very general and superficial, and cast no light on the basic underlying causes, it would be difficult to probe much deeper without reading too much into the sources. There is a crying need for some thoroughgoing psychological investigations in the field on this subject.

In Chapter IV of the general section, he deals with the problem of how far suicide among primitive people is explained in terms of external factors and how far in terms of the psychological characteristics of the individual. Closely related to this is the problem of whether or not suicide is an act committed voluntarily or forced upon the individual by the prevailing religious and social customs. Chapter VII shows suicide to be more frequent among women than men.

This volume represents great effort and is a useful contribution not only to anthropology but to psychiatry as well.

ELNA SMITH

WASHINGTON, D. C.

*L'ogénèse culturelle. Traité d'ethnologie cyclo-culturelle et d'ergologie systématique.*

GEORGE MONTANDON. (778 pp., 445 figs., 32 pls., 31 maps. 100 fr. Paris: Payot 1934.)

Cycles culturels is the author's translation of *Kulturkreise* (p. 30). His use of the term *cyclo-culturelle*, however, seems better translated by our terms *culture pattern* or *culture types*. The preliterate world, somewhat after the manner of Spengler in *Obverse*, is divided into a number of types of culture, neo-matriarchal, paleo-matriarchal, totemic, and so forth, and into others of distinct provenience, such as the Sudanese, the Mexican-Andean, the Arctic, Sinitic, and so forth, down to and including Modern. More than two hundred pages are devoted to a characterization of these respective cultures. The remainder of the book treats of phases of culture or of culture traits, such as hunting, fire-making, and metallurgy, and so on through a long list including a large number of items.

There are a large number of figures, plates, and charts showing the world distribution of certain traits of culture. The work is thus essentially a cultural ethnology. There is a brief discussion of the theories regarding culture diffusion. The various culture types are regarded as so many stages between primitive culture, represented by Negroids, Tasmanians, and Australians, and the higher culture of modern times, civilization proper.

The author has appended an excellent index of subjects and authors.

WILSON D. WALLIS

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## DISCUSSION AND CORRESPONDENCE

### "ABENAKI" CLANS—NEVER!

In a recent article in this journal, "Some Moot Problems in Social Organization" (Vol. 36, 1934, pp. 321-30) Dr Lowie allows the virtues of an able discussion of Dr Olson's treatise on sib and moiety in North America to be slightly marred through his perpetuation of what has been shown to be an error as regards a clan organization among the Abenaki (Wabanaki of northern New England). Dr Lowie refers in several places to the "Abenaki" as possessing certain "clan names" and lists them as such (pp. 327-28). His reliance is evidently placed upon Morgan who included the Abenaki among groups characterized by paternal clans. It is time to expurgate again certain misconceptions from our sources on social phenomena of the north-eastern tribes as I attempted to do some years ago. The Wabanaki, or "Abenaki," tribal groups, which include the Abenaki specifically, really are not to be included in the series of clan-possessing peoples of America, but rather with the socially undetermined hunters of North America north of the St. Lawrence and the Great Lakes. It evidently falls to my lot to quote a portion of an article printed in 1917 (F. G. Speck, *The Social Structure of the Northern Algonkian*, Publications of the American Sociological Society, Vol. 12 [1917], pp. 85-86, 94-96) in which I discussed the social typology of certain Wabanaki peoples (Penobscot, Micmac, Malecite) basing criticism of Morgan's definition of "Abenaki" clans upon findings made among aged informants among those tribes. Undoubtedly Dr Lowie, whose professional insight is unquestionably keen, has forgotten rather than overlooked the section in question.<sup>1</sup>

Thus far research among these predatory northeastern Algonkian . . . through Labrador, northern New England, and the Maritime Provinces to the Atlantic shows that the tribal subdivisions are based, without exception, on the family grouping with patrilineal tendencies. The family group as the social unit comprises the individuals of one family connection, primarily through blood but also through marriage relationship, who hunt together as a herd within the confines of a certain tract of country. This district constitutes a paternally inherited territory retained more or less exclusively by the family of the right of usage. These simple conditions are universal in the area, as has been established by the writer through personal investigation there. Nowhere in this sweep of territory have true clans or gentes been *reported*, except in two instances—among the Ojibwa as just stated and among the *so-called Abenaki*, the latter instance resting on the sole authority of Morgan. . . .

The status of the Abenaki, established by Morgan, however, requires some attention and revision.

In a very short presentation Morgan gives Abenaki material which by its linguistic form appears to be Penobscot. It would have been very helpful if he had told us, as he has in other places, who his informant was. Since, however, we cannot directly question his source, an internal criticism of his data is all that can be attempted. He notes fourteen gentes for the Abenaki, all of which coincide in name with family band names of the Penobscot, except five,\*

<sup>1</sup> I have added italics to certain passages.

\* The name of "Spotted Animal" (gens. No. 5 of Morgan's list) is given as Ahlunk-soo.

on the corrected basis, Morgan's Snake, Caribou, Crane, Porcupine, and Muskrat gentes. On the other hand, we have twelve in the authentic list which are not included in Morgan's (Perch, Otter, Water Nymph, Fisher, Raccoon, Whale, Insect, Toad, Eel, Sculpin, and Lobster and Crab). The list of family names furnished the writer by his informant, Newell Lyon, during a lengthy period of study of the Penobscot, can still to a large extent be verified among the families on Indian Island today. Moreover, Lyon's memory is clear and full on events of forty years ago, at the time (about 1878) when Morgan consulted his Abenaki interpreter. At this period the family organization was still strong and the territorial hunting system had not entirely decayed. Comparing, then, what we may consider as the more critical list of family names, those given by Lyon, with Morgan's, we are, I believe, justly obliged to correct and amplify the latter's. Morgan says that descent is now in the male line, which is also true of the family group. He says that intermarriage in the gens was anciently prohibited. If his informant meant intermarriage within the family, this, of course, is also true to a certain extent. Morgan finally says "the office of sachem was hereditary in the gens." If we interpret in his favor by choosing to make this mean that the office of leader of the group was hereditary in the family, then this is also true but not exact, because there was only a vague idea of headship in the family. This completes his contribution to our knowledge of the social life of the Wabanaki.

As for the Abenaki proper (St. Francis Abenaki) Hallowell's more recent intensive study of their social forms, shows a similar situation—no indication of "clans" whatever.

In the Wabanaki area south of the St. Lawrence the patronymic family group is likewise the basis of society. Specific data, however, as to the numbers, naming, and geographical location of the families have only been obtained from the Penobscot and Micmac up to the present time, though indications point to the prospect that similar characteristics will be found among the Malecite and Passamaquoddy.

The Penobscot were divided into twenty-two families, comprising about four hundred individuals. They were exogamic only in respect to kinship. As noted in the case of the other tribes, there was a general tendency for a man to affiliate himself with his father-in-law's family. . . . The family bands possessed paternally inherited hunting territories which were referred to by the individual as "my river". These were marked by boundary signs, either blazes or birch-bark representations of the animals from which the proprietors derived their names. Resentment against trespass was not noticeably strong. Tacit self-control answered for the prevention of trespass, or the policy of exchanging privileges nullified it. One important point here, however, is that, with the Penobscot, the families held themselves in a certain

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This is Penobscot *alāṅksu*, "wolverine" (*Gulo luscus*). In gens No. 2, given by Morgan as "Wild Cat" (Black), Pis-suh, we recognize *psu*, the term of "Bay Lynx" (*Felis rufus*). There is no black wild-cat, so it is hard to account for Morgan's parenthesis unless it be that he was misled by the informant's confusion in an attempt to describe the animal in English. The Penobscot call the Fisher (*Mustela pennanti*, Erxleben) the Black Cat. This does occur as a family name in the tribe. Again, Morgan lists as No. 10, Pigeon Hawk, which he gives as K'-che-ga-gong'-go. In Penobscot pigeon hawk is *awē'lās* and *kci'*'gagago is raven ("big-crow"). Raven is also a family name, but Pigeon Hawk is not.

Morgan's Abenaki information is, on the whole, so misleading and fragmentary that were it not for his known reputation as an observer it could well be ignored. Without being tested linguistically, evidence of identity furnished by half-educated natives is very unreliable.

association with animals. These animals are those upon which they prey for their subsistence. This peculiar condition, so far as I know, is unique as a group institution in America, although in individual association it is not so uncommon in the northern area, as I have attempted to show in the second paper referred to above. The Penobscot family names in most instances were derived from the animals which gave identity to the hunting districts through their being most numerous in them. The members of the family were known generally by their patronyms. As regards personal nicknames we find that a suggestion of band identity is involved in them. A fairly large percentage of these are derived from "baby talk" terms, others are derived from peculiarities of speech and behavior and from humorous anecdotes concerning the owners. Some are "use" names, nicknames derived from the kind of game hunted. That the Penobscot nicknames sometimes betrayed a certain connection with the ideas characteristic of the animal associated with the family is shown by a few examples. There is, moreover, still another imaginary association between the family and the animal eponym which appears in the idea that the families inherit some physical peculiarities from the animal. The members of the Whale family (Stanislaus) are pointed out as large, portly, and dark persons, those of the Rabbit family (Newell) as small, timid, and weak, those of the Bear family (Mitchell) as orderly and dignified, and so on. Their traits are thought to be traceable not only to descent but to the fact that they "used" them so extensively. A further psychological association exists between the family groups and their animal eponyms in the belief that certain (about thirteen) of them traced their descent indirectly to animal prototypes. This relationship is accounted for in a myth relating how the culture hero released the world waters from the belly of a monster frog which had held them back in his belly. After slaying him the culture hero frees the waters so that they flow again forming the river systems of the Penobscot country, whereupon some of the people who had been dying of thirst became transformed into marine creatures as a result of overindulgence in quenching their thirst. The others who restrained themselves escaped transformation, to become the ancestors of the human families, assuming, however, the names and to a certain extent the identity of the particular animal into which their nearest relatives were transformed. Totemistic characteristics seem not to be wanting altogether in the case of the Penobscot. What interests us more at this time is the structure of their society as regards possible marriage selection among the Penobscot and throughout the whole northeastern region, as well as upon the deductions from marriage statistics in several bands (Montagnais and Penobscot), a summary of the procedure may be stated, I believe, with some degree of correctness. There are no formulated regulations of marriage between members of different families or bands.

There is, in short, no reason to perpetuate any longer the illusion that any of the Wabanaki peoples possessed a clan organization. And the sooner the correction of Morgan's allegations is admitted the nearer we shall come to a valid understanding of what the termini of distribution mean in American ethnology.

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### KINSHIP TERMINOLOGIES IN CALIFORNIA

In a recent paper<sup>1</sup> Professor Kroeber returns to the question of the correlation

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<sup>1</sup> Yurok and Neighboring Kin Term Systems (University of California Publications in American Archaeology and Ethnology, Vol. 35, 1934, pp. 15-22).



of kinship terminology and social organization. I have no evidence that he is therein seeking to controvert the position I have held for a long time, but I have a suspicion that he is, and if that be so there are certain misunderstandings that should be removed. For the past twenty-four years I have maintained that we can expect to find, in the majority of human societies, a fairly close correlation between the terminological classification of kindred or relatives and the social classification. The former is revealed in kinship terminology, the latter in social usages of all kinds, not only in institutions such as clans or special forms of marriage, but specifically in the attitudes and behavior of relatives to one another.

For me this assumption (for it was hardly more) was originally a deduction from a more fundamental working hypothesis, viz., that any social system (meaning thereby the total social structure and the totality of social usages of a defined group) must normally possess a certain degree of functional consistency. Functional inconsistency occurs whenever two elements of the social system in their working or "functioning" conflict with one another. The working hypothesis goes on to assume that whenever marked functional inconsistencies occur they tend to produce change and that the tendency continues until the inconsistencies are resolved. (It may be well to point out that functional consistency is something different from logical consistency. My own view is that this latter is one special form of the former.) Starting with a working hypothesis<sup>2</sup> of this kind the scientific method would be to derive from it a number of deductions and then test them by a sufficient examination of the facts. One deduction from the primary hypothesis is that stated above with reference to kinship terminology. This has now been tested, by myself and others, in a number of instances in Australia, Melanesia, Polynesia, Africa and North America, and a fairly satisfactory degree of verification has been attained.

First it should be remembered that it is not an absolute, one-hundred-per-cent, but a relative degree of consistency that is posited as a necessary condition of existence for social systems. We may assume that no human society ever attains to perfect functional consistency. Therefore, if some inconsistencies are found in certain societies this does not invalidate the hypothesis. So, in the matter of kinship, if instances are found (and some have been found) in which the social classification of relatives and their terminological classification do not exactly correspond this does not disprove the proposition that in general they tend to correspond fairly closely.

Professor Kroeber points out in his paper that the Yurok, the Wiyot, the Tolowa, and the Karok and Hupa have four different types of kinship terminology giving four different modes of terminologically classifying kindred. Yet at the same time these five tribes "seem to possess almost identical institutions of marriage, society,

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<sup>2</sup> A working hypothesis is a method of formulating a problem or a group of connected problems for investigation. The problem formulated by the general hypothesis above is—What is the nature of the unity or internal consistency which every social system seems to possess in a greater or less degree, within what limits does the degree of unity or consistency vary from one society to another?

wealth and law." Therefore, they present an instance of absence of correlation between social organization and kinship terminology.

So far as my own position is concerned this argument means nothing. We cannot infer from the differences of kinship terminology that these tribes have different social classifications of relatives, but still less can we infer the contrary from the fact that their village and domestic organizations and their customs of marriage show a considerable degree of similarity. We do not possess for any one of these five tribes any detailed published account of the social classification of relatives, i.e., an account of how the individual regards, and behaves towards, his relatives of different kinds. It is this that I believe to have usually a fairly high degree of correlation with the terminological classification.

If we have two systems of classification of the same set of objects any inconsistency in the two is likely to make itself felt. Thus, in Montenegro, for instance, the application of a single term of relationship to the father's brother and the mother's brother would be likely to be felt as inconsistent with the fact that one stands in very different social relations to the two men. Inversely different terms for these two relatives in England would be likely to be felt as inconsistent with the fact that one behaves in the same way with both of them.

In their terminologies the Yurok classify father's brother and mother's brother together, whereas the Tolowa distinguish them. There are three possibilities. (1) That the Tolowa have differences in attitude and behavior towards the two kinds of uncles and Yurok do not. In this case both terminologies are correlated or consistent with the social classification. (2) That in both tribes there is no significant difference in the attitude and behavior towards the two kinds of uncles. In this case the Yurok terminology is consistent with the social system and the Tolowa is not. (3) That in both tribes there is a significant difference in attitude and behavior towards the paternal uncle on the one side and the maternal uncle on the other. In this case the Tolowa terminology is functionally consistent with the social system and the Yurok is not. There is at present no published evidence that points towards any one of these three as being the reality. And the same thing is true for all the other differences between Yurok and Tolowa terminology.

What is necessary to controvert the hypothesis which I hold is to show (1) that in the tribes of northern California there are real inconsistencies between the terminological classification of relatives and the social classification and (2) that there is no tendency in these societies to change the system in such a way as to move towards consistency.

If Professor Kroeber, in reply, should ask me to prove my hypothesis, I would answer (1) that I am doing my best, and (2) that it is much easier to disprove a false hypothesis than to prove a true one.

At the end of his paper Professor Kroeber also raises again that perennial question of the relation of history and sociology. His penultimate sentence is one with which I find myself in complete disagreement. It is that "the time for an attack on the problem" of the relation of kinship terminologies "to coexistent institutions is, on the whole, after some insight into their history has been attained, not before."

I find it necessary to distinguish between two different kinds of "history." One

is the detailed and documented history of the historian. The other is what I hope I may be permitted to call the hypothetical history of the ethnologist.<sup>3</sup>

A sociologist who neglected history of the former kind would be gravely at fault. Thus if one is considering the present-day kinship systems of the English-speaking peoples it would be unscholarly and unscientific to ignore the information we have (unfortunately not as full as could be wished) as to the history of kinship and kinship terms amongst Teutonic peoples and also in Roman law and its derivatives.

But a sociologist who based any of his conclusions on a hypothetical history would be just as gravely at fault, since it is his duty as a scientist to rely only on well ascertained facts.

In the paper referred to, Professor Kroeber does three things. By a comparative analysis he shows that the five tribes, Yurok, Wiyot, Tolowa, Karok, and Hupa possess "two basic types of kinship designation"—the Yurok-Wiyot and the Tolowa-Hupa-Karok. "The first of these is fundamentally similar in plan to the Salish-Wakash systems." The second belongs to a type widespread in North America. This seems to me an important and significant fact to bring out and one of which account should be taken in any study of the kinship systems of those people. Secondly Professor Kroeber concludes that these affiliations must have some historical basis, and here I would entirely agree with him, at any rate in respect of the similarity of the Yurok-Wiyot terminologies to the Salish-Wakash. Thirdly Professor Kroeber offers an outline of what he thinks may have been the series of historical events by which the present condition may have come about. This last is what I am calling hypothetical history and here I find it quite impossible to estimate what degree of probability should be attributed to the hypotheses apart from a purely personal judgment, and I know of no way in which I could find an objective measure of their probability. Professor Kroeber knows much more about the culture of this area than I do, probably more than anybody does, and is therefore likely to make better guesses than any one else. But I cannot, at any rate, accept his hypotheses as facts.

But even if they were facts and known indubitably as such, they would afford little or no help to the sociologist. The latter works with hypotheses that are either synchronic or diachronic. An example of the former is the proposition enunciated above, that a certain degree of functional consistency is a necessary condition of the existence of any social system. It is evident, I hope, that this does not require for its verification any historical knowledge (in the sense of a knowledge of the successive changes undergone by a social system). If we can examine a sufficient number of social systems each as it exists at a given moment of time, we can test this generalization. We can, it is true, obtain additional verification by showing that a given social system, through a series of changes, does maintain or recover its consistency, but this is not essential.

An example of a diachronic hypothesis is the statement given above that any marked functional inconsistency in a social system tends to induce change. The

<sup>3</sup> If the term "hypothetical" be thought objectionable it might be called "circumstantial" history in which inferences as to past events are based on circumstantial evidence as distinct from the evidence of eye-witnesses.

verification (or testing) of this obviously requires the study of actual processes of social change, i.e., of historical process. But what it needs is authentic, not hypothetical history, i.e., a detailed knowledge of actually recorded changes. The sociologist is the first person to regret that we possess so little of such knowledge. But he cannot accept as a substitute the hypothetical history offered him by the ethnologist.<sup>4</sup>

To return to northern California, if it were actually determined that in one or more of the five tribes there is some inconsistency between the kinship terminology and the rest of the social system taken as a whole, what the enquiring sociologist would wish to know is (1) by exactly what process did this come about and (2) is there or is there not any evidence of a tendency to resolve the inconsistency? So far as the first question is concerned it would not help even if we knew for certain that a tribe had retained its own terminology and taken over an inconsistent classification of relations for social purposes from another tribe. We should still need to have knowledge of the circumstances and details of this change, and this we cannot derive from hypothetical history.

When the ethnologist directs attention to cultural affiliations, as Professor Kroeber does in his paper, this is of very obvious value to the comparative sociologist. When he projects them into the past by a historical hypothesis he does something of which the sociologist can take no account until some precise objective criteria of the probability of his reconstructions are available. I fear there are none such at present.

As a sociologist investigating the nature of human society I cannot quarrel with Professor Kroeber as an ethnologist seeking to reconstruct history. But when he suggests that the sociologist should postpone his investigation of the kinship systems of some particular region until the ethnologist has provided "some insight into their history" it is necessary to protest, and strongly.

The investigations of the sociologist in the testing of his hypotheses are carried out by means of field research. He has to conduct his own research since the descriptions of the simpler societies by ethnographers whose work is controlled by an interest in ethnology naturally and quite inevitably fail to provide the data which the sociologist needs. The ethnologist is usually satisfied, having in view his own special interests, to collect a list of kinship terms and to note the occurrence of such distinctive and easily labelled usages as the levirate and sororate, the avoidance of the mother-in-law, and perhaps nowadays a joking relationship or some special form of marriage. One cannot expect an ethnologist with his own special interest, with no training in comparative sociology, often having no idea what the latter is all about, or, even worse, having totally erroneous ideas about it, to carry out the researches that are needed by the sociologist for the testing of hypotheses already formulated or for the elaboration of new ones. The problem of the nature and degree of inner consistency to be found in kinship systems is one of the innumerable problems in the solution of which the sociologist finds little or no help in the standard

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<sup>4</sup> It is also unfortunately only too often true that he cannot utilize the history of the historian.

ethnographical monograph. In the last few years there have been a number of investigations of this specific problem in tribes of Australia, Melanesia and Africa, and, in North America, in the Haida, Hopi, Fox, Arapaho, Cherokee, and Apache, and perhaps others with which I am not acquainted.

It is in this way, by many laborious field researches directed to testing as thoroughly as possible his provisional hypotheses, that the comparative sociologist has to work. It is in this way that he makes what Professor Kroeber calls his "attack on the problem of the relation of kinship terminologies to coexistent institutions." I cannot believe that Professor Kroeber really means that we should postpone such studies while the ethnologist is seeking that "insight into history" of which he speaks. But in that case I find it difficult to know what he does mean.

It would save a great deal of unnecessary misunderstanding if ethnology on the one hand (related as it is to archaeology and history) and social anthropology or comparative sociology on the other, were recognized for what they are, two different disciplines having different aims, different methods, and different interests in field investigations.<sup>5</sup>

The question of priority of one kind of study over the other need perhaps not be raised. But if it is raised, as it is by Professor Kroeber, I would feel a desire to turn the tables on him. Surely the proper time to make hypotheses about the history of a people and of their social system is not before but after we have obtained the fullest possible knowledge about and insight into the system as it is.

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A. R. RADCLIFFE-BROWN

### POTTERY-MAKING IN THE SOUTHWEST

I am glad to note in Dr Frank H. H. Roberts' excellent article, "A Survey of Southwestern Archaeology," in the January-March, 1935 issue of the *AMERICAN ANTHROPOLOGIST*, that he takes occasion to correct the growing misconception of the nature of pottery made by the paddle-and-anvil method in the Southwest. He states (p. 20): "... there was not as great a difference in Southwestern ceramics as the general statement of coiled versus paddle and anvil would indicate. Basically they are similar, the distinction being in the finishing process."

The opening sentence in my paper on "Pottery-Making in the Southwest" stresses the fact that both methods employ coiling: "... there are two methods of making *coiled* pottery . . . in the Southwest." The second paragraph begins: "The principal criterion of method is the use or non-use of a wooden paddle and a stone or pottery anvil in shaping the vessel."

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<sup>5</sup> By ethnology is here meant the study of peoples (ethnic groups) in their geographical and historical relations. Social anthropology is the investigation of the nature of human society by the comparison of societies of diverse types.

<sup>1</sup> University of California Publications in American Archaeology and Ethnology, Vol. 23, pp. 353-73, 1928.

## NOTES AND NEWS

### NOMINATION OF OFFICERS FOR 1936

The President of the American Anthropological Association has appointed as a Nominating Committee, John R. Swanton (chairman), Frank H. H. Roberts, Jr., and Frank G. Speck.

It was voted at the Pittsburgh meeting that the following procedure for nominations should be adopted: "Within three months of his election the President shall appoint a Nominating Committee of three members and transmit the names of the personnel of this Committee to the Editor who shall publish the names in the number of the *AMERICAN ANTHROPOLOGIST* which appears next after the time of election, with an invitation for suggestions; after considering such suggestions the Nominating Committee shall report its slate to the Council which shall pass on the recommendations, with such changes as are deemed advisable, to the annual meeting."

### LATIN AMERICAN INSTITUTE

A Latin American Institute for Race and Culture Studies has been established under the auspices of the University Museum, University of Pennsylvania, to further studies of archaeology, ethnology, and linguistics, primarily in South America. Interests will include the social conditions and literature of modern primitive peoples and the cultures which they develop by their admixture with Europeans. It is contemplated to sponsor publications and lectures, carry on documentary research, conduct expeditions, secure motion and still pictures and sound records of music and languages, and to train and exchange students for cultural studies.

THE *ENCYCLOPAEDIA OF THE SOCIAL SCIENCES* was completed in February of this year by the publication of its fifteenth volume. It will be recalled that American anthropologists were represented in the production of this work by the appointment of two delegates of the American Anthropological Association (Robert H. Lowie, Clark Wissler) to the board of Advisory Editors, by the inclusion of A. L. Kroeber on the same board, and of Franz Boas on the Board of Directors. Subscription to the *Encyclopaedia* is urged as compensation for the subventions to publication secured from various sources.

### RUSSIAN EDITIONS OF MORGAN'S WORKS

The Research Association of the Institute of the Peoples of the North, in Leningrad, has recently published a new Russian translation of Lewis H. Morgan's "Ancient Society" by Professor M. O. Kosven, to supersede a previous translation printed in 1900, which has become a bibliographical rarity. To the volume is prefixed Frederick Engel's preface to the fourth edition of "Der Ursprung der Familie" written in 1891 and appended is Morgan's preface of Lorimer Fison and A. W. Howitt's "Kamilaroi and Kurnai." Professor J. Alkor, as editor of the series of

"Contributions to Ethnography" of which this is the first volume, has written an introduction. A translation of Morgan's "Houses and House Life of the American Aborigenes" by Professor Kosven has also been published by the Institute, which will likewise soon issue the "League of the Iroquois."

BERNHARD J. STERN

"ANTHROPOLOGY IN NORTH AMERICA," comprising reprints from the *Journal of American Folk-Lore* and the *AMERICAN ANTHROPOLOGIST*, 1914, is still in print, contrary to popular belief, and obtainable from the publisher, G. E. Stechert and Co., New York.

"UTAH LAKE SKULL CAP" (*AMERICAN ANTHROPOLOGIST*, Vol. 36, pp. 431-33) the legend of Plate 7 should read Neanderthal instead of *Pithecanthropus erectus*.

GEORGE H. HANSEN

THE BULLETIN OF THE ARCHAEOLOGICAL SOCIETY OF CONNECTICUT, Number 1, January, 1935, may be obtained from Remington Schuyler, Editor, Peabody Museum, Yale University, New Haven, Conn.

#### RECENT DEATHS

Marshall H. Saville, Loubat Professor of American Archaeology, Columbia University, and former member of the staff of the Museum of the American Indian, Heye Foundation, died May 7, 1935, aged sixty-seven. Widely known for his archaeological researches in Middle America, he was elected president of the American Anthropological Association, 1927-28.

Benjamin March, Curator of the Division of the Orient, Museum of Anthropology, University of Michigan, died December 13, 1934.

Eugenio Yacovleff, Lima, Peru, died in November, 1934. He was an experienced field worker in Peruvian archaeology and collaborated with Dr Luis E. Valcárcel in editing the *Revista del Museo Nacional del Peru*

Frederick S. Dellenbaugh, New York City, who died January 29, 1935, was an occasional contributor to this journal. He is best known for his exploration in the canyon of the Colorado River with Powell.





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## HISTORY AND SCIENCE IN ANTHROPOLOGY

By A. L. KROEBER

A RECENT presidential address by Mrs Hoernlé<sup>1</sup> deals largely and interestingly with the old question of laws and history in anthropology, but seems to rest on an incomplete conception of certain currents of recent anthropological thought. Particularly is this true of the attitudes imputed to Boas; and this misunderstanding, if real, is certainly of importance because of the outstanding position of Boas in contemporary anthropology. During the last forty years he has not only trained many of the American and some European ethnologists or social anthropologists active today, but certainly influenced all of them, at least in the United States. The question of his methodology is therefore much more than a personal one in its significance. On the other hand, individual elements inevitably do play a part; in fact it is probably ignorance of some of these that has led Mrs Hoernlé to fail to realize the Boasian position in its entirety. If therefore the following remarks seem at times to savor of the personal, it is because I believe it to be necessary for full understanding. And if I take on myself the presumption to act as spokesman or interpreter for another, it is for three reasons. The first is that as the leading public character of anthropology Boas is in a position where even his individual attitudes are of public concern. Second, I have been trained and influenced by him. On the other hand, and third, my methodological views do not wholly coincide with his, and I have been criticized by him for them, and have replied; so that I believe I can speak at least with a certain detachment.

### 1

To begin with, it is of indubitable significance that Boas' educational training was in the physical laboratory sciences—in physics, in fact. This led him into psychophysics and physical geography: his doctoral dissertation was on the color of sea water.<sup>2</sup> This in turn led to a one-man, two-year,

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<sup>1</sup> New Aims and Methods in Social Anthropology (South African Journal of Science, Vol. 30, 74-92, 1933).

<sup>2</sup> Beiträge zur Erkenntniss der Farbe des Wassers (Kiel, 1887)

geographical expedition to Baffinland which brought with it intimate contacts with natives. The result was the "Central Eskimo" in 1888; and a career of anthropology since. From physics Boas brought into anthropology a sense of definiteness of problem, of exact rigor of method, and of highly critical objectivity. These qualities have remained with him unimpaired, and his imparting them to anthropology remains his fundamental and unshakable contribution to our discipline. Compared with them, his or others' views as to the degree of validity of sociologic laws or historical reconstructions are of only secondary moment.

This source from an exact and highly developed laboratory science is particularly significant by contrast. So far as I know it is unique for social anthropology, at least among its leaders; certainly it is exceptional. Inevitably it brings with it also certain limitations or colorings of objectives and method; and it is the non-recognition of these limitations that has led to misunderstandings like that of Mrs Hoernlé and many others.

Next, it is almost certainly significant that Boas stands alone also in having worked simultaneously in three fields as diverse as ethnology, linguistics, and physical anthropology, not merely with occasional sideline contributions, but with massive and basic ones. This fact of course presupposes a wide scope of method as well as interest, which must not be lost sight of in an attempt to understand his position in a single field such as that of social anthropology. The sole generally accepted department of anthropology in which he has shown little interest is that of archaeology. He has not even to any serious extent utilized the authenticated archaeological findings of others in his interpretations. In view of the fact that archaeology and ethnology both deal with cultural material, and somatology does not, this lack of preoccupation with archaeology while physical anthropology is actively pursued, may seem a strange inconsistency. It is entirely self-consistent, however, in that his basic approach is throughout "scientific," only rarely and hesitantly historical.

This may seem a strange dictum in the face of the fact that Boas has always emphasized the historicity of cultural phenomena, and that his "school" has sometimes been designated as that of "historical realism." But the epithet of "dynamic" has also been applied; and obviously neither is wholly accurate. In fact, there is no "Boas school," and never has been, in the sense of a definable group following a definable, selective program. For that matter, there have been no "schools" of any sort in American anthropology, as compared with British, French, and German. This national difference is in large measure due precisely to the Boas influence, which has consistently been exerted against the singling out of any one

method—psychologic, sociologic, diffusionist, functional, or *Kulturkreis*—as constituting a king's highway superior to others. This is again a result of the exact or laboratory science point of view. These sciences recognize fields or departments, like organic chemistry or spectroscopic physics, and differences of technique, but they do not recognize schools differing in method; there is only one method in physical science. By contrast there is something immature, or partisan and incomplete, in the very fact of the anthropological schools advocating each its program. In reality they differ, and legitimately enough, in objective, and that means that they differ at bottom in what they are most interested in. But from this they have too often proceeded to make propaganda not only for their interests but for their results, until in extreme cases special methods have been advocated almost like panaceas.

In competition with these more one-sided movements, Boas has been at a loss to explain his position in terms intelligible to the members of schools. When he cautions against the one-sidedness of historic-reconstruction interpretations, he is construed as wanting to be a functionalist of his own private "dynamic" variety. When he is skeptical of sociological or psychological laws, and insists on the historical complexity of cultural phenomena, he is promptly labeled as "historical." Mrs Hoernlé has at least realized that matters are not so simple as that. But she fails thoroughly to understand Boas' real position when she pictures him as at first defending "historical method" and later "conceding" or "admitting" or retreating to a dynamic functional program and methodology. To the best of my understanding, he has always used both sets of labels with hesitation, in a kind of last-resort effort to make himself intelligible to people who insisted on seeing things more one-sidedly. In fact both terms, "historical realistic" and "dynamic," are not his own slogans, but were coined by his followers. Far truer than Mrs Hoernlé's picture would be this one: Boas has never really followed the historical method except in a rather narrow, special sense which I hope to make clear; but he was a functionalist, in that his prime interest lay in structural interrelations, change, and process, before Radcliffe-Brown or Malinowski had written a line.

Process, rigorously determined process as such, is the one constant objective of Boas' work; and it is the one common factor, though present to a varying degree, in the work of those definitely influenced by him. What is this but an objective, and therefore a methodology, taken over from the physical sciences? Of course its limitations and difficulties in the field of culture, as compared with the inorganic world laid on the laboratory table, are numerous and obvious; and Boas was intelligent enough never to delude

himself on this point. When he came on the scene, he found anthropology taken up with schematic interpretations—Morgan's will serve as a typical example; and he unhesitatingly proceeded to show that these schemes seemed valid only as long as the fact was ignored that they were built up of subjectively selected pieces of evidence torn out of their historical context, that is, their actual context in the world of nature. In his insistence that this context may not be violated, Boas may have seemed, possibly even to himself, to be following historical method. But it was merely historical method applied as a critical safeguard; the problems with which he concerned himself were not historical except in minor cases, but concerned with process as such. Obviously, historical method as something positive becomes operative only when one is trying to do history. As regards specific schemes of the type of Morgan's, all trained and even half-way sound historians have always distrusted them profoundly; as much so as physicists in their field. In fact, all schematic explanations seem essentially a symptom of a discipline's immaturity.

## 2

The treatment of art may serve as an example. In his many studies of the subject, culminating in the 1927 book, Boas has considered the whole gamut of process factors: conventionalization, influence of technique, symbolism and secondary interpretation, virtuosity, cursive slovening, and the rest. The examples are from all over the world: superficially they look as diverse as those in a book following the old "comparative method" of schematizing. But they are never wholly out of their context; and they are never in a scheme. The ultimate conclusion is that the factors of involved process are many and variable; they differ in each succeeding case; and objectively critical analysis is needed to determine them in the complex variability of phenomena. As a historian might say, the uniqueness of all historic phenomena is both taken for granted and vindicated. No laws or near-laws are discovered. But neither are there any historical findings. Even the special chapter on North Pacific Coast art goes no farther in this direction than to record an "impression" that this art was formerly more geometric and less symbolic than now. The methodological requirements of history—such as continuity (with context as corollary) and uniqueness—are fully observed; but no history is done.

Allied evidently in origin and certainly in significance is the fact that style as such is never dealt with in the book. It is recognized as part of the context in each situation; but only that. There is no examination into what an art style is, of how or why styles develop; no characterization even of

the essential quality of any style as such. When any style is dealt with at all it is as briefly as possible and merely as a point of departure for inquiry or proof in some problem of conventionalization, virtuosity, symbolism, or other "dynamic factor" or process. Surely a book on art which leaves the fundamental element of style out of consideration as much as it can must seem strange to orthodox writers on art who deal with the definition or history precisely of styles. These remarks are made not in depreciation or censure, but in analysis of a method whose importance is great enough to warrant its being clearly understood: as to its limitations of aim as well as for its positive accomplishments.

The one serious exception in Boas' work to the rule that he does not do history, is, significantly enough, "The Central Eskimo"—his first major ethnological production. It is also the only one in which the geographic setting is given other than perfunctory or minimal consideration. I may add that a distinguished British anthropologist has confessed, privately, that he found Boas' descriptive ethnographical works, valuable as they undoubtedly were, extremely difficult to use and even to understand—except for this same Central Eskimo. Evidently Boas' characteristic pattern of approach had not yet become settled in this work of self-apprenticeship. It must be admitted that some of us on this side have at times shared a little in our trans-Atlantic colleague's perplexities. The cause, however, is plain on a little reflection. It is not lack of lucidity: I doubt whether there is an argument or sentence by Boas in print whose meaning is not perfectly clear and exact, provided it is approached with reasonable intelligence. The cause is rather a lack of interest in factual description for its own sake, in other words, in phenomena. This is of course allied to lack of interest in historical depiction. In each case the exposition as such suffers.\* To Boas the descriptive facts of a culture are always only the materials for the setting up of a problem, or series of problems. These problems deal with processes. Naturally the presentation does not yield the same integrated *picture* as a presentation made primarily on its own account with process left implicit or secondary. But from its own point of view, it is just as orderly, coherent, and clear. If the marshaling were all from the angle of one process singled out as the all-important one—as a more or less universal explanation—the scheme of presentation would probably seem lucid enough to everyone. But it would then be a scheme, and Boas' endeavor is normally to prove the multiplicity of factors.

\* The lack of organization on the purely descriptive side is perhaps also due in part to an intense conviction of the urgency of rescuing at all cost as many perishing data as possible without wasting time over their arrangement for the convenience of the user.

Here again we have the science approach. A physicist or chemist does not give a descriptive picture of what he encounters in nature. He starts with a problem; then presents such data as bear on it, and no others. Of course this method cannot be transferred directly to cultural anthropology because this is not a laboratory discipline, and in general it is not feasible to deal in each case only with those data immediately pertinent to the problem; sooner or later the descriptive context of the whole culture or set of cultures in which the problem lies must be made available. With the quantity or quality of descriptive data secured and recorded by Boas no one would quarrel, especially in view of his duplicating in linguistics his achievements in ethnology. It is only the form of presentation on which there have been strictures. In fact, considering the primary impulse always to formulate problems dealing with process, the mass of new and accurate descriptive data secured by Boas is really stupendous. I doubt whether it has been surpassed by any worker.

In brief, one may define the Boas position as basically that of the physical scientist, but fully aware of the requirements of cultural or human material: the need for all possible context, the strong element of uniqueness in all the phenomena, an extreme caution of generalizations savoring of the universal. All these are criteria of sound historical method; and because he observes them, Boas is right in insisting over and over again that he uses historical method. Only, he does not *do* history. And that does make some difference. Every thou-shalt-not which a professional historian might exact is fulfilled, but next to no positive historical results are produced; instead a problem about the dynamic factors involved is answered or attacked.

This strange attitude is evident not only in reluctance to prosecute history, but in strictures upon those who do so within anthropology. Granted that historical reconstruction from ethnographic data is a different thing from the writing of history from documents extending over a range of datable time—a point to which I shall revert below—it will I think be admitted that assailing the historical reconstructions of Wissler, Elliot Smith, Schmidt, Spinden, and myself all together,<sup>4</sup> is treating the extreme and the moderate sinners as equally guilty. This can only argue that historical reconstruction is *per se* unsound or vicious, irrespective of the degree to which it is carried or the method by which it is arrived at. Since dated documents are not available in ethnography, it would mean that we are to follow historical method rigorously but perpetually refrain from

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<sup>4</sup> Primitive Art (Oslo, 1927), p. 6

historical interpretations. To be sure, archaeology is extolled as sound method for those who wish to know about the past of unlettered peoples. But as its data are admittedly always incomplete, that does not help very far. And, more significant still, Boas has practically never made use of archaeological findings in his own work!

It seems clear that there is involved here a resistance to historical interpretations of any sort, at least within the limits of anthropology—what the historians of profession do with their written documents of the past is perhaps over the fence and none of our concern. Such a resistance is most easily understood as the deep-seated distrust of a mind schooled in the approach of the inorganic exact sciences, toward a fundamentally and qualitatively different type of interpretation; although also a mind intelligent enough to realize that in dealing with historical material—as cultural material is, in the wider sense—the methodological safeguards of history must be observed.

In this connection an incident of the 1928 International Congress of Americanists may be of interest. On the last day of the session an informal group gathered to discuss historical method in anthropology. Present were Nordenskiöld, Bogoras, Koppers, Gusinde, Preuss, Boas, Sapir, Kidder, Wissler, and several others. At first the discussion revolved around *Kulturkreis* principles; but before long it shifted, until for the last two hours it became a debate between Boas on one side and all the rest, including the *Kulturkreis* representatives, on the other: Boas consistently maintaining that his work was genuinely historical! It is small wonder that Mrs Hoernlé in distant South Africa should have failed to get his position clearly. But she can rest assured first that Boas has not recanted his faith that his activity is historical, and second that the majority of his colleagues do not see it as essentially such.

3

It is evident that we are at a point where it is necessary to try to define somewhat more sharply historical activity or the historic approach, as distinct from merely historical technique or safeguarding procedure. I suggest as the distinctive feature of the historical approach, in any field, not the dealing with time sequences, though that almost inevitably crops out where historical impulses are genuine and strong; but an endeavor at descriptive integration. By descriptive I mean that the phenomena are preserved intact as phenomena, so far as that is possible; in distinction from the approach of the non-historical sciences, which set out to decompose phenomena in order to determine processes as such. History of course does not ig-

nore process, but it does refuse to set it as its first objective. Process in history is a nexus among phenomena treated as phenomena, not a thing to be sought out and extracted from phenomena. Historical activity is essentially a procedure of integrating phenomena as such; scientific activity, whatever its ultimate resyntheses, is essentially a procedure of analyses, of dissolving phenomena in order to convert them into process formulations.

These two approaches are applicable to all fields of knowledge, but with varying degree of fruitfulness. It is in the nature of things—I do not pretend to explain why—that in the inorganic realm the processual approach of science has yielded most results, but as we pass successively into the realms of the organic, psychic, and social-cultural—"historical," this approach encounters more and more difficulties and its harvest diminishes. It is customary to say that the phenomena are more "complex" on the organic and super-organic levels. I incline to doubt this, and to believe rather that the difficulties lie in their being epiphenomena—from the point of view of the analytic, processual science approach. Hence the constant tendency to resolve organic phenomena into physico-chemical explanations, psychological phenomena into biological ones (the reflex arc), social-cultural phenomena into psychic ones. From the angle of science this procedure is perfectly correct; because so far as it can be applied, it yields coherent and verifiable results.

The historical approach, on the other hand, was first applied, and proved most readily productive, in the field of human societies; and it encounters increasing difficulties as the inorganic is approached. In the organic field it is still fairly successful; in geology and astronomy it leans so heavily on processual science that the nature of these disciplines, which by their objectives are clearly historical, is generally understood as being completely "scientific." As regards biology, I recently pointed out, in an essay on that subject,<sup>5</sup> that a whole series of phenomenally formulable "processes" familiar in anthropology—convergence, degeneration, areal grouping, etc.—were equally important in those biological activities covered by the old term "natural history," and that the problems of natural history run closely parallel, at many points, to the problems of human or cultural history. I do not believe in the slightest degree that these resemblances are "mere" analogies and empty and misleading. That may be true from the point of view of processual, experimental science. From the point of view of historical science, however, or history, or the historical

<sup>5</sup> Historical Reconstruction of Culture (Growths and Organic Evolution) (*American Anthropologist*, Vol. 33: 149–56, 1931).



approach to the world, they are obviously of methodological significance, because corresponding objectives involve corresponding methods.

I am not trying to assert that these two approaches can never meet, still less than they are in any sense in conflict. Ultimately, and so far as possible at all times, they should supplement each other. The degree to which astronomy has profited by leaning on and borrowing from experimental science is a case in point. But, precisely if they are to cooperate, it seems that they should recognize and tolerate each other's individuality. It is hard to see good coming out of a mixture of approaches whose aims are different.

As to the element of time sequence: if I am correct that the essential quality of the historical approach is an integration of phenomena, and therefore ultimately an integration in terms of the totality of phenomena, it is obvious that the time relations of phenomena enter into the task. I am not belittling the time factor; I am only taking the stand that it is not the most essential criterion of the historic approach. Space relations can and sometimes must take its place.

If this is correct, the point often made, not only by Boas and his followers but by sociologists and functionalists, that history is legitimate and proper, but historical reconstruction unsound and sterile, loses much if not all validity. I would maintain on the contrary that history and historical reconstruction have the identical aims and approach and make use of the same mental faculties. (In technical language, they possess the same basic objective and method; but it seems best to avoid the latter word because it is likely to be ambiguous in the present connection.) It is true that history has the time relations largely given it in its data whereas historical reconstruction largely seeks to ascertain them. But this makes the latter only a special and somewhat more difficult case of the former, taken in its widest sense.

A little reflection will show that all historical procedure is in the nature of a reconstruction; and that no historical determination is sure in the sense that determinations in physical science are sure; that is, objectively verifiable. Historical determinations are in their essence subjective findings; and at best they only approximate truth or certainty. They differ from one another in seeming more or less probably true, the criterion being the degree of completeness with which a historical interpretation fits into the totality of phenomena; or if one like, into the totality of historical interpretations of phenomena.

History is supposed to tell "what really happened." But obviously this is impossible: the "real" retelling would take as long as the happenings,

and be quite useless for any conceivable general human purpose. The famous principle is evidently to be understood obversely: history is not to tell what did not happen; that is, it is not to be fictive art. More useful is the definition of a historian as one who "knows how to fill the lacunae." But even this is too narrow. The professional historian is no doubt most conscious of the occasions when he encounters frank gaps in his data; but he is all the time, habitually and largely unconsciously, reading between the lines of his data on the one hand and omitting less significant data on the other. If he did not, he would never reach an interpretation. Whether this procedure is avowed or not, or if avowed whether or not scientists know it, does not much matter: it has been and is the procedure of all historians. If some of us ethnologists attempt to do time history for the poor dateless primitives, we have an additional unknown to deal with, and our results are undoubtedly more approximative only. But if we frankly admit that fact, there seems no valid reason why we should be condemned as inherently unsound for doing under greater difficulties the same type of thing which historians are respected for doing. That historians pay little attention to us, their poor relations, is expectable enough: who are we to enter the houses of the substantial when we do not possess even one document written before our day?

Many scientists do not know what history is, or merely assume that it is not science. But it is old and reputable, and is accepted as long as it sticks to documents. In counterpart, scientists make scarcely any effort to apply their methods to documentary materials. If the aim of anthropology is to ascertain the processes of change or dynamics in human societies and cultures, why this timorous sticking to the primitives whom we can observe only an instant, while rich data on change for centuries back are available on our own and other lettered civilizations? The usual answer is "complexity." But is this a serious obstacle as against the advantage of operating with timed data in studies of change?

Well, the result is that historical reconstruction on the basis of datable documents is not seen as reconstruction and is held up as laudable or permissible even though not scientific; but once the reconstruction is patent, because the dated pieces of paper are not there, it is considered wasted effort or unsound.

4

Of course not all reconstructions are good, either. In general, their value seems proportional to their being made with the qualities that characterize sound straight-historical work.

The Elliot Smith and Perry reconstruction suffers from the fatal defect of positing the cardinal event of culture as consisting of the originating of one complex at one time in one place. Any documentary historian who proposed half so simple an interpretation would get no hearing whatever from his colleagues. The scheme is really little more than a formula, and has been able to subsist only because it was posited in the obscurity beyond the boundary and attention of history. Some definite results of value have been attained by the "diffusionists;" a new weighting of the stagnancy, from one point of view, of many primitive societies relatively unexposed to higher culture contacts; also of the rôle of deterioration or possible extent of cultural losses; and certain resemblances and probable connections between particular clusters of elements far separated in space. These are worth-while positive findings. But compared with the scheme into whose frame they are set, they are specialties, and they do not in the least retrieve the scheme itself, which remains contrary to all historical precedent. Significant broad historical findings are not much more likely to emanate from laboratories than significant chemical ones from scholars' libraries.

The case is different, and rather puzzling, for the earlier form of the corresponding German reconstruction, the *Kulturkreis* theory, because Graebner, the leader of the group, is said to have begun as a professional historian. His "*Methode der Ethnologie*" is in fact based largely on Bernheim's "*Lehrbuch der Historischen Methode*," reduced and made over to some extent to allow room for his own scheme. It may be conjectured that Graebner, finding no suitable outlet in his earlier career, tried to force one by attempting in the unpoliced no-man's-land of ethnology what would have been promptly suppressed or ignored in history. That he operated with six or eight wholly disparate blocks instead of only one is no palliative to any historian, as long as the principles of continuity and uniqueness are fundamentally violated.

The reformulation of the *Kulturkreis* scheme into the *Kulturgeschichtliche* "*Methode*" of Schmidt and his collaborators is to be taken more seriously, because Schmidt undoubtedly possesses genuine historical insight, in regard to language as well as culture. The skill with which he has gradually remodeled the stark Graebner scheme out of all semblance to its original form, is evidence of this capacity. However, it does remain a scheme, and therefore all Father Schmidt's keenness, immense knowledge, and love of argument cannot make it a genuine, empirically derived, historical interpretation.

Spinden did begin empirically, restricted his field largely to part of

America, and seems to me to have genuine historical feeling. He has evidently yielded at times to an infatuation for the grandiose; but his chief defect appears to be an over-early and rigid crystallization of a formulation which started off on a sound enough basis, but ended by tending to blur the variety and unique features of many of his data.

Rivers, with his "History of Melanesian Society," is the classic case of a man of undoubtedly very high ability trying to apply a laboratory type of formula—he was trained in physiology and psychology—to a historical problem. His "Todas," though valuable for its new observations, shows the same lack of realization of there being such a thing as a pertinent historical approach. This strange little culture, obviously a specialized modification at innumerable points of the higher Indian culture, is treated with scant reference to this context; although its relation to this is the most significant problem which it presents. Boas, though also a laboratory graduate, has shown far more adaptability in similar cases; witness the keenness of his scent in trailing well disguised motives of Old World origin in American Indian tales.

Radin, who possesses the feel and insight of a historian, fails in his "Story of the American Indian" not because he lays these qualities aside when reconstructing, but because he reconstructs hastily without sufficient pains and detail. In securing and evaluating ethnological documents, he has shown extraordinary skill; which, if exercised in the field of orthodox history, where good documents are recognized and valued, would have brought him far more appreciation, and might have spared us certain sharp outbursts in an otherwise stimulating recent volume on ethnological theory.

Myself, who has been bracketed with several of the foregoing, I shall not attempt to judge or defend. I will express the purely personal opinion that those of my reconstructions which were published in professional organs for a professional public as an end-product or by-product of intensive preoccupation with a body of material,<sup>6</sup> continue on the whole to satisfy me as sound workmanship of their kind. It may gratify those who react differently to learn that these reconstructions have brought me some censure, no commendation whatever that I know of, and for the most part have been

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<sup>6</sup> The History of Native Culture in California (University of California Publications in American Archaeology and Ethnology, Vol. 20: 125-42, 1923); The Patwin and Their Neighbors (University of California Publications in American Archaeology and Ethnology, Vol. 29, No. 4, 1932), pp. 391-420; Yurok and Neighboring Kin Term Systems (University of California Publications in American Archaeology and Ethnology, Vol. 35: 15-22, 1934); Archaeological Explorations in Peru. Part I: Ancient Pottery from Trujillo (Field Museum of Natural History, Anthropology, Memoirs, Vol. 2: 108-14, 1930).

as completely ignored by my colleagues as I expected them to be by the larger world.

On the other hand, one of the genuinely significant reconstructions in ethnology was made by Boas himself. I am not now referring to scattered suggestions, nor to a brief paper on northern elements in Navaho mythology,<sup>7</sup> but to a formal, undisguised reconstruction: "The History of the American Race."<sup>8</sup> It is true that this is a presidential address, that it is brief and sketchy, and that since many years Boas seems to have avoided reference to the article. But it was an illumination and inspiration to many of his students and former students. When Wissler a few years later published "The American Indian," it contained many other things, but its main unifying synthesis after all was a more detailed development of Boas' reconstruction; and others, including myself, have followed with partial developments. Perhaps it was the very fact of the influence exerted by his own suggestive interpretation that helped drive Boas farther into his profound distrust of all reconstruction. But that so many other Americanists were ready to accept his outline as sound and valuable, and that so far as it went it has never been challenged, should at least indicate that there are better and worse reconstructions.

In many qualities Spier's general anthropological product perhaps stands nearest to that of Boas: high grade observation, definite restraint, conscious rigor of method—all the "scientific" qualities. Recently he has turned against reconstruction as misleading and unnecessary,<sup>9</sup> and out-Boased Boas in including in his condemnation his own sun-dance history which everyone else had always accepted as reasonable and worth-while. Driver and I, reusing his data with another technique, statistically, have since come to virtually the same conclusions as Spier originally formulated regarding tribal participations in the growth of the sun-dance and therefore one aspect of its indicated history. The case is perhaps of no great moment in the present connection except as an instance of how far and strongly the current against a historical interest in ethnology has run.

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Recent developments in American linguistics illustrate the same point. More than forty years ago Powell had a list and map of linguistic families

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<sup>7</sup> Northern Elements in the Mythology of the Navaho (*American Anthropologist*, Vol. 10, 371-76, 1897)

<sup>8</sup> *Annals of the New York Academy of Sciences*, Vol. 21, 177-83.

<sup>9</sup> Problems Arising from the Cultural Position of the Havasupai (*American Anthropologist*, Vol. 31, 1929), p. 222.

north of Mexico compiled. Most of the participants in the undertaking were wholly untrained in philology; the leader was a biologist; but the work was consistent, impartial, business-like, filled a practical need especially as regards ethnic relationships, and at once became standard, even though almost no evidence was presented. Many of our younger students probably know the famous major only as the author of "the Powell map."

About the same time Boas was beginning his collecting studies and analyses of American languages, a labor carried out with such qualitative and quantitative success that the product, even without his work in ethnology and physical anthropology, would have been a monument. Until a very few years ago it was literally true that every competent worker in American linguistics except one or two had been trained as well as inspired by Boas. With that, his own output—the fundamental monographs on the Chinook, Salish, Kwakiutl, Tsimshian, Kootenay, Keres languages, besides contributions on many others—was as great as that of any two of his juniors; in each case a basic body of texts with an analytic description of the structure of the language in terms not of an abstract pattern but of its own characteristics. The value of this body of work is probably unparalleled and certainly incalculable: the method, so far as it goes, thoroughly sound.

As more material accumulated, it became apparent to a number of workers—Swanton, Dixon, myself, Sapir, and others—that some of the languages classified as separate by Powell were indubitably related. If so, this meant ethnic relationship, hence conclusions of obvious ethnologic-historic significance. Some of our group were perhaps primarily interested in these non-linguistic significances, and did not push the search for linguistic evidence much beyond the point of establishing a more or less strong probability of connections. Sapir took part in this movement; but, being primarily a linguist, and having been trained in orthodox "philology" as well as by Boas, he went farther and proceeded to apply the reconstructive method of this philology in the American field.

Indo-European philology, which constitutes the overwhelming bulk of what is conventionally called or miscalled philology, is a discipline with a highly developed methodology and rigorous technique. It uses the comparative method for historical objectives under a strict set of principles. It reconstructs the hypothetical original Indo-European speech not as an ultimate end in itself but as part of a method of tracing the changes which have taken place in the several Indo-European languages. On the history of many of these we possess only intermittent and brief documentation. If philology had confined itself to studying actually documented changes, its

history of this group of languages would be fragmentary: in fact, mostly lacunae. It has gone on the principle that by operating with a hypothetical Indo-European, built up not by random guesses but according to a consistent methodology as exhaustive as possible—the seeming exceptions as important as the seeming rules—it could make this history far more complete and significant. The nature of language happens to be such—its range is narrow and apart in comparison with culture but its forms are precise and readily definable—that convincing results were easily obtained by these reconstructions. At any rate they have been accepted as convincing, and philology has had, deservedly, the reputé of enjoying probably the strictest methodology and most exact technique of any discipline among the social studies and humanities—in the *Geisteswissenschaften*. This discipline is called comparative, but its ends are historical, and its fundamental mechanism of operation is precisely reconstructive.<sup>10</sup>

Now when Sapir began to apply this well-established method to a somewhat widened Athabascan and Algonkin, and when those of us who were less ambitious drew more elementary conclusions as to speech relationships which if true must have been of definite ethnic and cultural influence, Boas reacted negatively and has continued to do so. The evidence was declared insufficient, our procedure dubious, the problems themselves unfortunate because they distracted attention from more important problems of process. So far as I know, Boas has never analyzed and refuted the positive evidence offered for specific relationships, but has tried to throw the whole case out of court on the ground that no satisfactory evidence was being offered in the premises.

His chief argument of rebuttal has been that the similarities, even in structure, might be due to contact-influencing of originally unrelated languages. It must be admitted that there is a real problem here, to which Boas began calling attention forty years ago. On the other hand, it is obvious that the problem cannot be attacked without recognition of the factor of relationship—for instance on a purely geographic-statistical basis—else similarities undoubtedly due to common origin, as between French and Spanish, or Navaho and Apache, would be undifferentiable from similarities really due to contact transfer, as between French and Basque. The argument in short can be run indefinitely in a circle unless certain facts as

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<sup>10</sup> It is true that orthodox Indo-European philology has tended to become an isolated, highly-specialized, self-sufficient pursuit somewhat sterile in comparison with what it might become with broader objectives, or has thought at times that it could attain these broader objectives by injecting bits of metaphysics. But the fact remains that it enjoys universal respect for a sound technique while being historically reconstructive.

to relationship are first agreed on as established. What shall this basis of agreement be? The fifty-eight families lined up more than a generation ago by an ornithologist for an administrative head who had been a geologist-geographer? Or the much smaller number of families to which these fifty-eight have been reduced by a group of anthropologists trained for work in language by Boas, and headed by a linguist of the eminence of Sapir?

Granted that some of us, including Sapir, may have been at times over-enthusiastic and a bit speculative—most Europeans consider us, as a body, ultra-conservative—a reasonable basis might have been found for a temporary working agreement, and Boas's own substitute problem could have been genuinely attacked by now, instead of being merely advocated as a reason why linguistic effort in the American field should remain restricted to collecting and analyses.<sup>11</sup>

It cannot be believed for a moment that the Powell map has any fetishistic value for a man like Boas. For one thing he is too fundamentally impatient of all classifications. Nor may one believe that with the stealing on of the years he had begun to feel the need of a sure, unchanging world. The new problems he is developing, the old ones he is extending, his receptivity to certain new movements like the psychological approach in ethnology, all controvert such a suspicion. He is not looking for a secure retreat but for new enterprise. The only convincing explanation for his opposition to problems of speech relationship is that such problems are in

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<sup>11</sup> Boas' contact-modification problem is of genuine intrinsic interest. As usual, it bears on process. The overwhelming mass of precedent in the history of languages is to the effect that large absorptions of content can take place, also some modification of phonetic form, but that imports or assimilations of structure probably constitute normally only a minute fraction of the structural growths that develop internally. The opinion of strict philologists is not particularly conclusive on this point because they usually begin and end by concerning themselves only with changes internal to a family; but there are linguists as well as philologists. The real problem of course is when, how, and to what extent the process of imitative borrowing from outside takes place. This has not yet been investigated systematically, and is worth being investigated, even though most linguists may feel that their experience warrants them in estimating that the external factor will turn out to be a minor one. It is a tribute to Boas' insight that he formulated the problem, and did so before he used it as a weapon against the historians of speech.

Of a different sort is the opposition of Uhlenbeck and Michelson to some of Sapir's findings. This springs not from any anti-historical bias, but from an over-complete submergence in orthodox philology, in which both men were reared. They will not admit any relationship until it is proved with the same intensiveness as in Indo-European languages, which have had hundreds of students for one in the American languages. This means that the formal code of a highly organized discipline must be adhered to to the last letter even in pioneer situations, in short, the code is more important than results.



their nature historical, and that he genuinely distrusts the historical approach and historical interpretations, no matter how made or by whom, at any rate in his own discipline of anthropology and in regard to his particular American field.

This linguistic example is somewhat special for anthropology as a whole but illustrative on account of its clearcutness.

## 6

In physical anthropology Boas' important contributions on growth and type changes have been through statistical rather than anatomical procedures. He has in fact made original contributions to statistical theory. At first sight it may seem strange that he has never applied statistical method to cultural data. Efforts made in this direction have been ignored by him; and his few general utterances on the subject are to the effect that statistics cannot be used in ethnology, as Tylor's error of method shows. Since Tylor was attempting to solve a universal problem, one of inferences; and since statistics can be and have been applied to specific historical situations within a given time and space frame, Tylor's insufficiency, like that of Hobbhouse-Wheeler-Ginsberg, obviously does not close the issue as completely as Boas seems to assert. I believe again that his opposition is due to a fear that statistical method will be used in ethnology for historical findings, and especially of a reconstructive kind; as indeed it inevitably will be.<sup>12</sup>

## 7

A seemingly strange product to come out of the Boas movement, and an attest of its strength and breadth, is the characterization of cultures in prevalently psychological terms by Fortune, Mead, and Benedict in recent years. Perhaps "in association with" would be more accurate than "out of" the Boas movement, for one of the three has been stimulated also by Malinowski. Malinowski's final interpretations, however, are psychological to a considerable extent, whereas the works of these three investigators remain essentially cultural analyses with a strong psychological coloring. That is to say, the findings are in part expressed in psychological terms, but they are findings about cultural phenomena, not resolutions of them

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<sup>12</sup> Perhaps the difficulty of measuring and defining cultural material as precisely as anatomical material also plays a part. But in that case the definition of elements, whose use takes the place of direct measurements in statistical ethnology, deserves a destructive examination. In one of his early monographs, Boas counted elements—folkloristic motifs or episodes—to establish routes of historical transmission; but since then he has used such elements chiefly to deal with processes.

into their psychic springs. There is also definite consideration of the place of the individual in his society, of his rôle in his culture. There is no sharp line of demarcation from the Malinowski attitude, but at least historically this approach stems mainly from Boas. At any rate it has his definite approval and encouragement. Two of the group deal with data of their own collecting, the third interprets chiefly materials already recorded. All three concern themselves with the functioning of cultures as wholes. Their analyses therefore do not primarily serve to extricate processes as such, but are preliminary to a coherent synthesis of the totality of the culture conceived much as a living organism, not pictured statically. Closely allied is the work of Bunzel.

In vividness of characterization, quickness (in both senses) of insight, ability to coördinate masses of detail into a unified and on the whole convincing picture, the work of this group is of a very high order. I say this explicitly, because in reviews I have once or twice felt compelled to dwell also on certain deficiencies of workmanship which did not seem enforced by the nature of the undertakings but to spring from an overpersonalization of approach. This perhaps is almost inevitable in first attempts at a type of presentation as intimate as this one; and in the present connection, where we are concerned rather with the nature of a kind of approach than with a precise appraisal of particular works, I do not wish to emphasize previous strictures. I mention them only because while I have not withdrawn them, I wish to be understood, as I meant to be in the reviews, as regarding the work of all members of the group as valuable.

What is of special relevance in the present connection however is that all this type of approach aims not so much to isolate process as to show it at work in a picture of the culture conceived in terms of its own totality. The method may therefore be called dynamic or functional or psychological; but ultimately it is a form of the historical approach. It does, as a means of heightening its own particular quality, deliberately leave out the time element and all its functions, and therefore passes as non-historical. But, as I have said before, time is only an incident in the historical attitude, although an important one. The essential types of apperceptions and evaluations that count in the Fortune-Mead-Benedict approach seem very closely allied to those requisite in a good historian, or for that matter for a reasonable culture-historical reconstruction. The elements needed to build up the picture are selected, and those not needed are omitted, or slurred with intentional subjectivity. On the other hand the painstaking analysis and non-selective objectivity of the "scientific" approach are lacking. Criticism of the group has indeed been based largely on the subjective

quality of their work; which however is no longer a defect as soon as its essentially historical nature is accepted.

On the contrary, criticism perhaps should lodge rather for failure to be broadly and completely historical. It is easier to obtain a sharp, unified picture by cutting out antecedents and surroundings and focussing on the impressionistic, cinematographic image which is being unrolled. Such restriction of aim is not *per se* a fault of method; but it tends to result in a series of dazzling, disconnected effects. These pictures of course ought sooner or later integrate into a picture larger in geography as well as duration; and on reflection many problems of the how of development and interrelation arise; but these larger views and further problems have not, at least not yet, been followed out by the authors in question.

*Mutatis mutandibus*, the work produced by this group seems close in its essential character and spirit to, say, Burckhardt's "Renaissance." Boas realized this when in his preface to Benedict's book he speaks of her approach as being concerned with the "genius of a culture." Here appears to lie the real quality of these productions. They are analytic; but so is Burckhardt—intensely so, and like him, they analyze in order to build up an integrated picture. Like him, too, they succeed in so doing; and this is the one aspect of their work of which to date we can positively affirm the value. Benedict's psychiatrizing formulations are original, suggestive, and stimulating; they may open up new and fruitful approaches; but on the other hand they may remain mere analogies. Personally I am sympathetic and hopeful; but also realize the danger of over-enthusiasm; the real proof lies in results: and Benedict will have to work over more material, and think her results through farther, and others will have to test her approach before we can be sure what it really means. On the contrary, she has given us an integrated picture of Zuñi and Kwakiutl culture seen from a psychological angle which we know to be valuable. The same holds for Mead.<sup>13</sup> She may think, and she may be right in thinking, that the ultimate value of her work lies in the generalizing chapters, those which deal with process or with applications to our own lives. But I would not trade them for the picture of Manus, the high quality of whose workmanship is immediately convincing, whereas the value of the reasoned remainder remains subject to test. The authors themselves may put the emphasis the other way; but if so, this is presumably due to their springing out of an environment which rates science high and history low. The whole conditioning of nineteenth and twentieth century civilization is in this direction. The way to be suc-

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<sup>13</sup> Fortune and Bunzel have hewn somewhat more closely to the line

cessful is to be scientific. But I am trying to see the less transient values - without closing the door to newer ones.

This group, then, may or may not have made an important contribution to scientific anthropology; it has made one to historical anthropology.

## 8

It may seem that this discussion has revolved largely about a personality. It has of necessity, because this personality is not only the largest in anthropology, but has stood most distinctively and successfully for the application of scientific method in the subject. The Boas movement comprises probably the most numerous group of active, able, and sound workers in anthropology today. If those less directly but still traceably or partially under its influence are included, there is no doubt as to its being largest.

If now we try to sum up this influence, the following findings seem salient. First of all, the movement stands for the application of what is generally recognized as the method of science to a body of material previously treated either historically only or merely by naive methods, broadly speaking. Next, the movement recognized that this body of material was sufficiently distinctive that it could not be treated by the direct transfer of methods evolved in the experimental sciences: hence the failure to seek "laws," or sociologic surrogates. Third, it did avail itself of existing sound historical method, has consistently practiced it, and to that extent may properly claim the title by which it is most often known. But, fourth, perhaps because it emanated from science, it never fully understood the underlying objectives of history, therefore in general failed to formulate its problems historically, and actually took from history essentially only its negative safeguards. The consequence is that the results are to an overwhelming degree unhistorical, and that the attitude of the movement has been anti-historical in tendency. This is perfectly consistent with its scientific origin; and the outcome may be all to the good, ultimately; but the situation should be recognized for what it is.

I will only repeat, to prevent possible misunderstanding, that by "historical" I refer not primarily to a preoccupation with time sequences, but to a basic and integrative intellectual attitude of which such preoccupation is normally an outflow.

## 9

It remains to consider another side of anthropology, that which does not claim to observe historical method and frankly disavows all attempts at historical results. These movements have usually been labelled sociologic

or functional. In their nature, they must obviously be concerned if not with laws then with constants in the field of culture. At the outset it must be said that this is not an objective which, *per se*, anyone would quarrel with or has quarrelled with. The only question is, whether fruitful results are obtainable and how.

The most active and influential exponents of one wing of this movement at present are the *Année Sociologique* group and Radcliffe-Brown; of another, Malinowski.

Durkheim and Mauss are avowed sociologists who have specialized on primitive culture. Their method is the "comparative" one, their findings are general conceptualizations. They observe, in general, the safeguards required by history: they do not deal with small bits of culture torn out of their context. Nevertheless, their results are not integrations in terms of a larger culture whole, and therefore historical, but integrations in terms of conceptual constants, and thus unhistorical. What are these constants? With Durkheim it resolves ultimately, if I understand him aright, into a social group's sensing its culture as at once its *raison d'être*, its cohesive force, and its life blood, and trying to maintain or shape its culture in accord with this integrative principle. The emphasis seems to be more on this principle, or its dim apperception or symbolic expression as that which holds social forms together, than on the social forms as such. This seems to savor of mysticism; but the mysticism is perhaps mostly due to difficulty in formulating such ultimate concepts. The concept appears to be a perfectly valid one as a hypothetical explanation, but of course difficult to connect satisfactorily with specific evidence.

Durkheim has at least built some kind of a bridge across the gap which has always separated sociology and anthropology.<sup>14</sup> He does deal primarily with the social group, the social machinery; but this, according to him, succeeds in existing and functioning only because of another element, its culture, which thereby becomes, if my understanding is correct, a sort of *primum mobile* for society. This is not an idea to be discarded lightly as merely mystical. It certainly is not a historical concept. It verges on the philosophical; perhaps falls most nearly within *Geschichtsphilosophie*; and can become scientific in proportion as it is empirically verifiable. Obviously, however, such verification is difficult on account of the breadth of the

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<sup>14</sup> The persistence with which these two theoretically allied disciplines, born nearly at the same time in western Europe, have in general kept separate from each other, is in itself an interesting problem in culture history. It suggests that they spring from different sets of impulses and aim at different ends.

concept and its remoteness from the surface of phenomena; and to date Durkheim remains mainly a prophet who has glimpsed a great vision.

Mauss comes nearer to earth again, and the "comparative" method is more in evidence. However, not only, in contradistinction from an earlier generation, are historical requirements as to context observed, but the constant found is not so much a specific one as the fact that elements function in relation to one another. That is, the older naïve type of interpretation that A normally produces B, and B, C, is replaced by the conclusion that A, B, and C normally function in relation to one another in a larger, integratively functioning whole. Few would be disposed to disagree with this, and the point is well worth being kept in mind, especially by the hasty in specific interpretation. But it is hard to see the attitude as of much utility in a concrete attack on concrete problems. Here the philosophic paternity—or perhaps more exactly, ancestry—is evidently still operative. An expression, too, of this strain, is visible in the reluctance of the group to embark actively in field studies, which the definitely scientific as well as historical minded students of primitives have since more than a generation pretty unanimously felt as a real need.

Mauss's categorizing also fits badly with the procedure of both the main currents of anthropology. We no longer feel the grouping of phenomena under such concepts as Gifts or Sacrifice to be profitable, because these concepts are derived from common, unscientific experience, and not specifically from the cultural data under investigation. No physicist or biologist would approach his data from the angle of the categories "long" and "flat" and "round," useful and real enough as these concepts are in daily life. The historical approach, it is true, does not shrink from currently using concepts of this order: it is one of the characteristics of history that it does not need, or at any rate has not generally employed, technical or symbolic terms. But historical treatment can follow this seemingly slovenly procedure because it organizes its material in terms of the time or space or phenomenal content relations, never primarily in terms of concepts derived from unhistorical experience. Similarly the descriptive ethnologist may group his new data under headings of this sort—warfare, religion utensils, etc.—but this is merely a convenience of external, conventional order, not of underlying or significant organization.

## 10

Radcliffe-Brown perhaps stands nearest the French group. He has not hesitated to admit that his aim is sociology. He does not repudiate history as illegitimate; but he realizes that it is a different thing from sociology

and insists on their being kept separate. He does explicitly intend to work without unnecessary historical considerations; and he does believe that there are laws in the socio-cultural field and that they can be found. These laws are not merely similar patterns within which the phenomena of culture have recurrently happened, but they refer to factors which bring it about that culture phenomena do happen, must happen, in certain ways. For instance, the parts of a culture function with reference to each other so as to produce as integrated a whole as possible, and when they fail to do so readjustments in this direction are set in order. From the French sociologists Brown perhaps differs most conspicuously in his insistence on first-hand investigation, on the type of acquaintance with materials which permits them to be freshly dissected; in short, field work. He is therefore an empiricist, and can claim to stem from science rather than from reasoning or philosophy; as indeed he does, biographically: he was trained in psychology by Rivers.

The segregation of social anthropology from history is not necessarily to be condemned. While the whole tenor of my argument is that the definitely historical approach is justified and valuable in all disciplines dealing with cultural material, it is certainly legitimate to lay it aside in the hope that a rigorously non-historical attack may yield new results. The test after all should be by results. Now here the general verdict to date is that if Brown's generalizations are broad they are also tenuous, whereas in proportion as they are concretely applicable, they tend to lose their universality and are no longer laws or constants. This verdict it is difficult not to concur with. It appears to be part of the old dilemma of the sociologist: by the time he finds a formula that no one can cite exceptions to, it has become so essentially logical, so remote from phenomena, that no one knows precisely what to do with it. Its only value is as an end in itself. Brown's thesis that every society or culture tends to function integratively, is of this order. As a point of view to be kept in mind it is no doubt sound enough, and may prevent distorted apperceptions; but neither as a tool for further inquiry nor as a final synthesis will it satisfy either the scientifically or the historically minded. Its significance seems to be in itself, to those who find satisfaction in that type of formulation. Every physiologist would accept the fact, probably takes it for granted, that there are strong integrative tendencies in the functioning of all organisms. But would any physiologist consider such a principle to be either the end result of his science or a specific tool for prosecuting it further? He would view it as a background presupposition, to be invoked when one-sidedly dissociative interpretations threatened the balance of his discipline. It is in something

of this light that we must see this law or basic hypothesis of Brown's: it represents a reaction or corrective against the extreme analytic tendencies of the Boas movement.

Mrs Hoernlé's cited examples of cultural laws in Bantu legal and marital systems of course are not laws at all, but only descriptive summaries of uniquely occurring phenomena. They are really fragments of good history which she does not recognize as such because they are presented without reference to the time element. They are also raw materials for potential scientific interpretation.

Apart from his program or propaganda for laws, Brown's specific attitudes are really very close to those of the majority of American anthropologists—I mention them because they include no diffusionist or Kulturreis adherents. Particularly would his position be close to that of Boas, if only he would refrain from specifically ruling out historical control method. After all, even a heretic like myself is not dreaming of making a weekly exercise of historical reconstruction obligatory on all anthropologists, but merely pleading that those of us who wish to give cultural phenomena reasonable positive historical treatment be permitted to do so without having a yellow cap set on our heads for it.

## 11

Malinowski is also a functionalist, but with a more psychological trend in his final interpretations than Brown. He does not professedly look for laws. Both his field data exposition and his interpretations are stimulating, important, and sane. But his data are drawn almost wholly from one limited area, and within that overwhelmingly from one small culture. For the rest, his conclusions depend essentially on the exercise of a keen mind. His generalizations therefore may lack some of the validity which they appear to possess. After all, there is no more reason to infer cultural or psychological universals from Trobriand culture than from our own. That is the first and by now quite elementary lesson of anthropology. To be sure, Malinowski is very careful not to assert universally binding validity for his findings; but his points tend to be developed with an elaboration of manner which is likely to convey to any non-anthropologist who is not highly cautious, the impression that they are universal or near it. There is general agreement, to which I heartily subscribe, that Malinowski's conclusions, so far as they really go, are suggestive and generally sound. But it is clear that they are so because he possesses an unusually keen imagination and intellect, not because of his method, which as something transferable seems exceedingly limited. We know enough by now of the little



culture area of which the Trobriands form part, in fact in certain respects enough about all Melanesia, to make it evident that many characteristic Trobriand institutions and attitudes are reworkings, specializations, or warpings of institutions and attitudes widespread in the area. Obviously such facts are of bearing even in a picture of the culture *per se*. They are still more important if generalizations beyond the culture are to be attempted. Whether it is a question of the Kula potlatch or of the relation of father and child, the data on institutionalized giving or trading and on the relations of near kin in Melanesia as a whole, or at least in the Massim area, are obviously pertinent in proportion as generalizations of breadth are undertaken. I am of course not asserting that the first prerequisite to any other work is a reconstruction of the past history of culture in Melanesia. Problems enough can successfully be approached with a complete omission of time factors, if one so prefers: on a merely one-moment basis which is comparative within a limited and patently interrelated area. Even this modest concession would be historical. In fact so definite a functionalist as Radcliffe-Brown has made it the basis of his approach in his "Social Organization of the Australian Tribes," which many of us, presumably for that reason, consider perhaps his most valuable single piece of work. But Malinowski so far has preferred to travel his dazzling orbit unhampered by even rudimentary historical considerations. It is the more pity because his insight is excellent and his mind fruitful.

## 12

I am afraid I have transgressed the twenty minutes—even of silent reading—in which souls are supposed to be saved; and therefore regretfully pass over a number of other important workers: Wissler, for instance, some of whose methods I have recently discussed in detail;<sup>15</sup> my colleague Lowie, whose soundness is so careful that his basic approaches would require intensive dissection to analyze out; Nordenskiöld, who has made historical reconstructions which for once no one has found fault with, and added to them empirical investigations of the conditions surrounding invention; Kidder and the other archaeologists, whose approach is of necessity primarily historical. It would however be inappropriate if in an essay devoted to emphasizing the importance of the historical attitude, I were to deal only with contemporaries. To save space, I shall confine myself to two pairs of figures customarily bracketed together in Germany and England: Bastian and Ratzel, Tylor and Frazer.

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<sup>15</sup> S. A. Rice, ed., *Methods in Social Science* (Chicago, 1931), pp. 248-65.

Bastian need be mentioned only on account of his name. His real service was the fervor with which he preached the need of collecting data and objects while there was still time. As a thinker, he was if not a mystic at any rate highly obscure. He had a certain quasi-philosophical point of view, but no method; and he did not perceptibly influence anyone.

Ratzel the geographer is a strange personage to figure as one of the founders of our discipline, and it is only the amorphous condition of nineteenth century anthropology which allowed him to attain even that conventional repute. His influence on anthropology was not primarily environmentalistic—his minor sins in that regard are badly over-emphasized in his English version, and he started no environmental movement within anthropology—but definitely historical. He saw and emphasized historical problems where the documents customary among historians were lacking; and he recognized the phenomenon of peripherality. If his influence even among German anthropologists was not greater, it was perhaps primarily because his discussions of primitive peoples were incidental to geographic considerations and insufficiently clear-cut, referring somewhat ambiguously to peoples and their cultures.

Sir James Frazer is still with us from another generation, the generation of the unity of the human mind as an active spontaneous principle, and of the importance of survivals. In these days when we are so conscious of method—over-conscious, the reader may have concluded—the suave, urbane unconcern of our forbears sometimes seems like the golden age of untrammelled innocence. Frazer pursued the exotic story of forgotten nooks as an end in itself. If ever a sense of scientific problem or time perspective troubled him, it was but transiently. Reared in a classical tradition steeped in history, he became the supreme antiquarian. Yet his impress on the educated public was for a time as wide and deep as it has been light on more recent anthropologists. He must have undermined much formal religious dogma by implications almost inevitably drawn from his works. Professionally he seems to stand above all for an interest in cultural pathology. His preoccupation is with those customs and beliefs that deal with the ever-present problems of incest and its regulation, with sacrifice and cannibalism, with the will-to-power attempts of magic, the security devices of taboo—all the neurotic manifestations of helpless cultures. To a considerable extent he has been read from the same interest that makes readers of erotica, pathologica, mystica. We younger men, and women, are of sterner if less cultured stuff, and leave these palatable morsels in order to bite into tough problems or psychiatric formulations. Nevertheless Frazer, though lacking in any formal method, did feel in his phenomena—they are

ever-recurrent, so far as cultural phenomena can be—some kind of an import, which Freud was quick to see even if we walked by it. The last few years have seen the beginning of an inclination—in Malinowski, Fortune, Mead, and others—to approach these phenomena once more with somewhat the interest of Frazer, though of course through the medium of a more modern psychological and cultural methodology.

Tylor, so often coupled with Frazer, seems related to him in the fact of sharing certain presuppositions and evaluations typical of their time, rather than by any inner kinship. They both belong to the period when anthropology was beginning to crystallize out as a subject. In Tylor the sense of problem is as strong as it is deficient in Frazer. Spiritually, if not formally, he was a man of science: he saw the need of proofs. His famous attempt to make a demonstration by treating the frequency of seemingly independent "adhesions" failed, as was recognized by some even at the time, because the cultural independence of his ethnic units remained unexamined. Also, his constants, like "avoidance," were only roughly constant. Nevertheless the attempt revealed a genuine sense of problem and method. That it was not repeated for long, shows Tylor to have been ahead of his age. But he did not only seek laws; he realized the importance of historical connections; and again he sought a method of establishing these where the continuity in space and time had become interrupted.

That Tylor accepted the essential unity of the human mind should not be held against him, for we do so too, though less explicitly. That he drew positive and specific inferences from this postulate which we no longer draw, was the fault of his being a pioneer. In the two generations since his prime we, in common with psychologists, have come to realize intensely the plasticity of this mind material, the enormous conditioning to which it is subject. Inevitably, therefore, we are much less ready to define the mind, or to use its unknown quantity for explaining phenomena which we are able to define better than we can define it. But this after all means only that we operate with a more critical methodology. The fact that this methodology insists on dealing first with the measurable or characterizable phenomenal factors A, B, and C, and relegating the difficult and protean X of the mind to the rear, does not abolish the X. The X, or its relation to the Y of culture, does remain our ultimate problem. This fact, in our enthusiasm, we tend to forget; and, probably more than we know, we are bringing up our students and successors in an ultra-behavioristic attitude of operating with a scientifically sound methodology and a minimum of orientation as to the end-purposes of the method. These lines are of course not a plea for the reintroduction of a metaphysical entity; nor are they

strictures on the point of view underlying the modern methodology—only a caution against this being taken as the end-achievement. If there is a human mind, it has a structure and constitution, and these must enter into its phenomenal products. It is a sign of advancement of our studies that we realize the difficulty of defining this structure and constitution; but it remains a factor in our basic task none the less. We have learned by experience that we can reach more specific results by setting ourselves partial problems which are so rigged that they omit the mind, even where the approach is psychological. But it is well to remember that we are making a deliberate omission for practical purposes for the time being; and above all that we have not yet proved that  $X$  equals 0.

Tylor's fundamental position is therefore far from being liquidated, though many of his specific findings may be. He possessed genuine scientific curiosity of a high order, sanity and far-sightedness, and balance as between alternative approaches. He must be construed as easily the greatest of Boas' predecessors.

### 13

The point of view which underlies the foregoing discussion is that there is a historical attitude and approach as well as a scientific attitude and approach, and that, in a field like anthropology, each has its genuine problems and equally important and fruitful results. If I have leaned one way, it is because the current of the day runs the other. At least so it seems to me: there may be bias. My education included some contacts with experimental science which I found highly stimulating, but consisted primarily of generalized activity in the linguistic-literary-historical field, remaining rather undifferentiated until I settled upon anthropology as definitive profession. It seems only fair to make this statement after commenting on the influences that have borne on others.

History of course is in the present connection to be understood as an attitude of mind of which history *de métier* is only one and an imperfect expression. It is necessary to repeat that while the time factor can never be permanently left out of consideration in history, preoccupation with sequences is not the cardinal quality of history. It may have been so in the annalistic origins; but even Herodotus was already beyond that. And it is genuinely significant that he was not only the "first" historian but the first ethnographer. In modern times Burckhardt was a real and a great historian though time sequences scarcely enter into his "Renaissance." And there is nothing in his attitude, in the problem or task he set himself, or in the methods he used, which is not good anthropology. Obviously I

am not trying to restrict anthropology to work of the Burckhardt type. But I am trying to prevent endeavors of this type, or of any soundly historical type, including reconstructions—Burckhardt's "Renaissance" is nothing if it is not an integrative reconstruction—from being ruled out of anthropology. That is why I have tried to show that some of our work which passes as meritorious because it seems scientific has its major values lie in being, though unrecognizedly, historical in character.

The two approaches need not conflict. We are fortunate in having both of them available. We need them to supplement each other. The scientific element has freed anthropology from some of the limitations of conventional history. We are ready to face process as such, which historians will scarcely do. But pulling any number of process demonstrations out of the mass of phenomena does not really prove very much that is positive, because the processes which anthropology has succeeded in isolating have so far failed to integrate into a larger system of processes to any considerable degree, as they do integrate in the experimental sciences. Unless we stand ready to content ourselves with demonstrating that cultural or historical material is very difficult to resolve wholly into processes, we must fall back into doing something with the phenomena themselves. What we generally do besides merely recording or enumerating them, is to define their patterns. But a pattern is not a process; it is a descriptive representation of a constellation having its basis, or believed to have it, in the reality of phenomena. It is fundamentally a historical and not a scientific formulation, even if its description be exact or quantitative. If we discern generalizable process at work in it, nevertheless the pattern always remains a unique historical phenomenon. It is simply larger and more relational than a single historical fact, element, or event. When patterns interact, we can again see familiar processes operating in fluctuating strength, but what is most definable to our understanding is the product, the new patterns resulting. Sound history, and at least to a considerable extent sound anthropology, concern themselves with finding patterns and putting them into their actual relations essentially on the phenomenal level. At any rate such has been the case until now. In proportion as a historian specifies "causes," he is to be distrusted, and generally is distrusted by other historians.

Basically a functional approach is rather close to the historical approach. It does not, if it is wisely critical, specify causes. It does not, for the most part, distinctively isolate processes. It really concerns itself largely with depicting patterns and their interrelations. It does try to view these as living: "dynamically" or "functionally" instead of "statically," or, speaking in analogy, physiologically as well as anatomically. This is just

what historians do. Only, having their data given them in the flow of time, they take for granted that they deal with them functionally, whereas we with our momentarily known primitives have had to discover functionalism and are still somewhat elated about it. When a functional program goes farther and attempts to discover laws or calculable processes, it has, at any rate until now, mainly done one of three things: it has discovered patterns and mislabelled them laws; or it has formulated laws which are so predominantly logical or conceptual as to be of little service in investigating phenomena; or it has isolated processes whose strength however is so variable and incalculable that they remain inadequate instruments for helping us to understand the fullness of phenomena. I wish I could see the situation more optimistically. So does every historian. It will be a great and intensely stimulating day in the course of human understanding when we determine definable and measurable processes operating under precise laws in history and culture. But a realistic attitude compels us to admit that that millennium is not yet here.

As to historical reconstructions, they can be defined as a special form, under special circumstances, of the endeavor to see and understand phenomenal relations of culture patterns. If they are honestly that, they are as justified methodologically as anything else that a legitimate historian attempts. They are even necessary at times, because the whole aim of history is to understand in terms of successively larger integrations, not to cling timorously or mechanically to the thread of narration and re-narration of the known. That reconstructions are more tentative in result than interpretations based on continuous data is an obviousness to be taken for granted, not an argument for putting them under the ban. On the contrary, in the hands of those who do not sense what a culture pattern is, reconstructions become verbal bridges over the unknown, or fictive pretenses—fictive without the value of art.

Anthropology, as an accident of its materials, stands with one foot in the field of the undoubted sciences; with the other, squarely in history. The fact that its central theme is the unlettered and forgotten peoples, kept it from absorption in narration and from overemphasizing the particular event, the particular individual, and directed its attention more readily to culture as such. The most obtrusive data on a primitive tribe are its culture. Once culture-conscious, anthropology did not have far to go to become pattern-conscious. For much the same reason, it became process-conscious. The efforts of the pioneers like Tylor and Ratzel, however fumbling, were at least partly in this direction. It was Boas who first made us all able to see and deal better with process as such. This is his great

contribution; this and the unswerving rigor of his critical standards. But process has not and cannot displace pattern, which retains its intrinsic significance in all historic material. The two simply are findings of different orders. And they are not in conflict. With knowledge of the processes at work, patterns as patterns are undoubtedly better understood. Without realization of the inherent patterns, the application of process concepts to material like culture leads to highly incomplete results. Not that the two approaches should be mixed; that would be fatal. They need intellectual differentiation, precisely because we shall presumably penetrate further in the end by two approaches than by one.

One can write a Q.E.D., or a virtual Q.E.D., under a scientific demonstration of process, or hope to do so. No sane historian writes a Q.E.D. under anything; neither a piece of history, nor archaeologic prehistory, nor a reconstruction, nor a pattern formulation. Those who like proofs above everything else are certainly entitled to make them. It is all to the good to have proofs made. It is also the privilege, in fact the wisdom, of those so minded to stop where their material no longer yields critically valid proofs. But this limit is not necessarily the limit of all intellectual endeavor because it is the limit of one approach; nor is what is beyond it necessarily the field merely of problemless antiquarians, biographers, and story-tellers. Differences in approach are probably at bottom largely dependent on differences of interest in individuals. It is perfectly legitimate to confine one's interest to the scientific approach, or to the historic, or to use alternately one or the other according to occasion. But sympathetic tolerance is intrinsically desirable; and certainly advantageous to deeper understanding: to "scientia."

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## THE SUN DANCE OF THE HEKANDIKA SHOSHONE<sup>1</sup>

By E. ADAMSON HOEBEL

AT the present time two sun dances are held simultaneously on the Fort Hall Reservation. One of these dances occurs on the flats near the agency twelve miles north of Pocatello and is participated in by Lemhi, other Shoshone, and Bannock. The ceremony of the H<sub>3</sub>kan'dika Shoshone is held in their own isolated portion of the reservation on a bench of land in the Bannock Creek Valley, twenty miles west of Pocatello. The ceremony reported here is the latter one, though informants aver that the ritual is identical in both dances. Several short visits were made to the dance near the agency to check this, and so far as I could observe, it is fact. The essential features of the sun dance recorded in this article will therefore hold for the sun dance of the Lemhi Shoshone and the Bannock as well.

Though the dance was scheduled to begin on July 21, early arrivals began to raise their lodges three days ahead of time. Individual camps were set up by family groups; those expecting a large congeries raising dwellings of upright and interwoven willow branches to form a large room thirty feet across and rising fifteen feet. The walls slanted slightly toward the center and the ceiling was left unrooted. Canvas wall tents with their entrances flush against the wall of the brush lodge served as private sleeping quarters for small family units. Isolated old women built themselves scanty wind-screens, and in a few instances conical brush lodges of willow and pine were raised by small families<sup>2</sup> (plate 12, upper figure). A camp circle of twenty lodges formed the community. The circle was one-hundred yards in diameter and had a very broad mouth opening to the east. The site of the dance lodge was at the approximate center. Though there were no tipis

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<sup>1</sup> The author, assisted by F. Gore Hoebel, in the summer of 1934 made an intensive study of certain Shoshone groups on the Fort Hall Indian Reservation near Pocatello, Idaho. The investigation was under the auspices of the Council for Research in the Social Sciences of Columbia University. The Shoshone living on the reservation are the surviving members of numerous bands. Today one band alone has maintained its integrity as a group. This is the H<sub>3</sub>kan'dika, or "Seed-eaters," band. They remain in their original habitat, which centers about what is known as Bannock Creek, a tributary of the Snake River, flowing northward from the low range of mountains lying along the Idaho-Utah line.

The phonetics used in this paper are the symbols of the International Phonetic Association tables.

<sup>2</sup> The semi-circular screen according to J. G. Bourke (*On The Border With Crook* [New York], 1892, p. 340), was used by Shoshone on the warpath. The conical brush lodge is obviously a modification of the Plains tipi.



at Bannock Creek, there were a dozen to be seen among the hundred lodges that formed the camp circle at Fort Hall.<sup>3</sup>

Throughout the day there was desultory drumming and singing of sun dance songs and at night there was serious practicing of the songs before an open wind-screen, which was placed before the westernmost lodges. No preparatory lodge was used for either dancers or singers.

The gathering of materials and building of the dance lodge was confined to the day on which the dance began. A Ford truck was pressed into service to bring the timbers down from the mountains thirty miles away. There was little ceremony about it. The officiating medicine man sought out the cottonwood tree for the center pole and prayed over it in solitude. It was then chopped down by a crew of men without further ado. Quaking aspen poles for the stringers and uprights were procured, and the whole load brought down to the site.

The raising of the center pole took place late in the afternoon. The ritual was confined to men alone and the most active part was taken by the youths. The pole was laid out due west from the hole which was to receive it. A large bundle of willow withes equal in size to the body of a man was fastened in the crotch. A colored cloth was affixed to each of the arms of the crotch. One was for the medicine man and the other for the medicine woman, but no special significance was given to them in the form of symbolism.

The participants ranged along the pole on both sides. Then the first prayer was sung by a young man "who knew the songs." The log was lifted chest high with much facetious grunting, moved forward a bit, and set back upon the ground.

The second prayer was sung by the same singer and the feint at raising repeated. The sequence was followed a third time. After the fourth song the raising was quickly completed, with one group pushing on the pole, a second tugging at the guy-ropes, and a third manipulating scissors of crossed "tipi poles" (plate 12, middle figure).

Twelve uprights were placed in the already prepared holes, and four stringers were laid from the crotch of the center pole to the uprights at

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<sup>3</sup> The foundation of the tipi frame is of the four pole type. Compare R. H. Lowie, *Notes on Shoshonean Ethnography* (Anthropological Papers, American Museum of Natural History, Vol. 20, Part 3, 221, 1924), for the distributional significance of this fact. By reason of the continuous distribution of the four-pole foundation pattern from the Crow and Blackfoot through the Shoshone to the Ute it is probable that the Ute tipi is derived from northern sources rather than from the Cheyenne and Arapaho, of whom the three-pole foundation is characteristic.

the cardinal points of the compass. This was in conformity to the dream of the medicine man which had instructed him to depart from the traditional pattern of running a stringer to each of the uprights. Willows were lashed to the frame to form a fairly thick wall, an opening being left to the east. The completed lodge was forty feet across. The center pole stood sixteen feet to the crotch; the arms extending six feet higher. There was no roofing over the rafters. A mounted buffalo head was hung below the crotch on the west side of the center pole. The drum was placed on the floor to the south of the entrance. It was made of freshly prepared cowhide stretched over a steel frame thirty-six inches in diameter and eight inches wide. Seats of quaking aspen logs were laid about it for the drummers. The total endeavor was completed in three and one-half hours (plate 12, lower figure).

The persons who were to participate in the dance had retired to their various lodges to prepare themselves. Shortly after eight o'clock the male dancers began to gather at a spot thirty yards behind the dance lodge. The female dancers collected by themselves just in front of the westernmost camps. At sunset the dancers advanced in single file towards the lodge. They were blowing softly upon the eagle bone whistles held between their teeth.<sup>4</sup> Approaching the lodge, the file split into two lines. One, lead by the medicine man, circled the lodge from the south while the other came about from the north. After circling the structure one and one-half times, the two lines crossed each other at the entrance and entered along an oblique path. The dancers then ranged themselves in a line before the center pole, each person facing the west. The spectators, who had been standing in two groups by sexes outside the entrance, now surged to the door where they waited while the medicine man, extending his arms towards the center pole, held out two downy eagle feathers and uttered a short prayer. At the close all persons present joined in a deep inbreathing and patting of the joints and muscles of their bodies, following this with heavy exhalations. By these acts the power invoked by the prayer is used to absorb the aches and pains of the body. The ritual is termed *na'βaβuk*<sup>5</sup>, "blow it away."

Everyone fell at ease and the drummers entered to settle themselves, while the dancers found their places along the western wall (figure 1). The medicine man took a position directly west of the center pole, the men ranging themselves to the north of him and the women to the south. A couple

<sup>4</sup> The bone is the humerus from the wing. It is decorated with a downy eagle feather which is wired to stand upright at the end, and it is suspended from the neck of the dancer by a colored ribbon.

of infirm aged sat against the northeast wall, and women and young girls gathered about the drummers, seating themselves upon the ground.

The song leader began drumming and sang one whole song *sotto voce* before the men about him joined in. Finally, when the drift of the song had been caught to the leader's satisfaction, the drum was hit four heavy strokes and the drummers burst into full-throated song, accompanied by the singing of the women. Forthwith, the medicine man ran towards the pole and with short hopping steps danced backwards to his original position. Other dancers followed suit at random. Each dance hop expelled a breath

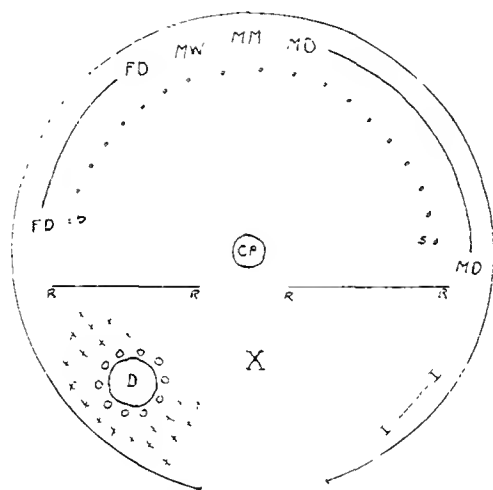


FIG. 1. Arrangement of the Sun dance lodge. CP, Center Pole; X, Fire; MM, Medicine man; MW, Medicine woman; FD, Female dancers; MD, Male dancers; S-S, Poles forming screen for dancers; R-R, Railing; D, Drum; O-O, Drummers; X-X, Female singers; I-I, Inactive aged participants. The entrance faces east.

of air through the whistle, causing the emission of short staccato blasts. This was the dance routine which was maintained throughout the entire ceremony and is a variant which is unique to the Shoshone alone.<sup>5</sup> The song lasted about five minutes, after which each dancer returned to his place. After a rest of several minutes, the orchestra again started the music and the dancers resumed their exertions (plate 13, upper left).

The leadership of the singing devolved upon men who were accepted

<sup>5</sup> Compare Leslie Spier, *The Sun Dance of the Plains Indian* (Anthropological Papers, American Museum of Natural History, Vol. 16, Part 7:471, 1921). "The sun dance step is invariable among the tribes; the line of dancers remains in one place while they rise on their toes with a springing motion."

as capable leaders. The leader held no rattle or other insignia of his right to assume the place. The drummers were any males who cared to participate. The songs were without meaning; they were started at a high pitch and gradually descended the scale with short syllabic sounds. At the end of each song the voices of the women carried on for a few seconds after the men had stopped.

After dark the camp announcer entered the lodge with an armfull of willow branches. These were given to the women singers who held them upright in their hands, jerking them up and down in time to the beat of the drum (approximately one hundred and sixty strokes to the minute). A roughly laid fire was set inside the entrance to the lodge. The fire-tending was assumed by an unimportant person and was not made the occasion for coup counting as is done by the Wind River cognates and other tribes of the Plains.<sup>6</sup>

The singing and dancing were carried on with vigor until midnight approached: then interest lessened until in the early hours before dawn the ceremony very nearly stopped. The active dancers were reduced to one and the orchestra to three. All others were sleeping. There were twenty-one dancers in the lodge, fourteen men and seven women, several having joined the dance after dark; it being permissible to enter before midnight of the first day. Activity again awakened with the approach of morning light. At five o'clock the announcer rounded the camp calling the people to the sunrise ritual, which began within ten minutes after.

In this rite the dancers lined up before the center pole in a double row with the medicine man in the center. They faced the east with their whistles in their mouths, holding their down feathers in outstretched hands (plate 13, lower left). The medicine man held out a votive otter skin. The drum beat in slow cadence. The voices of the singers were low and soft. Almost imperceptibly the drum-beats and singing increased in intensity as the light in the east grew stronger, to burst into full-throated song with the notes of the whistles piercing the morning air in long shrill blasts as the arc of the sun crossed the horizon. There followed another *na'βaβuk<sup>wi</sup>* by everybody present. After a moment of relaxation the dancers advanced to form a kneeling group about the embers of the fire. The four prayers which had been sung at the raising of the center pole were repeated, being given in solo by any dancer who cared to do so, not all being offered by the same singer. After each song the whistles were blown in four long, drawn-out sighs, followed by a period of silence. At the end, the medicine man arose

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<sup>6</sup> Cf. *Specter, op cit.*, page 475



Hákan'dika Shoshone Sun Dance UPPER, A windbreak and conical brush lodge in the camp circle, MIDDLE, Raising the center pole; LOWER, The dance lodge



Hikan'dika Shoshone Sun Dance. Upper Left, Dancers in action, Lower Left, Medicine man praying, with offerings, Right, Dancer greeting the rising sun.

and walked about the center pole to stand facing it towards the east. Arms extended, palms down, he uttered a short prayer asking for the well-being of the people, with the additional request that the sky be a little overcast that day, so that they might not suffer too early in the dance! There was more na'βaβuk<sup>wi</sup>.

An interval of two hours followed before the dancing was resumed. The time was spent by the dancers in removing the white clay body-paint which had been applied before entering the lodge. Damp cloths were supplied for this end by spectators. Individual designs in red and yellow were then substituted, each dancer doing his own painting. Designs were subdued and restricted, centering mostly about the nose and eyes. The male dancers were wearing kilts of plain or flowered calico, Mexican shawls, or light blankets wrapped about the waist and fastened with a colored sash. Women dancers wore loose calico dresses. At this point beaded belts were added to the costume by many participants, and in addition the women wore beaded head bands. A few of the men smoked their feet over a cedar smudge. This soothing act is called na'gwhin. It was permissible for dancers to leave the lodge to attend to nature, but they were carefully watched to see that they got no water.

At eight-thirty in the morning the dancing was resumed. In the forenoon various spectators went to the bottoms to gather tule stalks and two varieties of sage which were carried into the lodge and distributed among the dancers for bedding. Spectators and those helping never crossed an imaginary line about one-third of the way across the lodge floor. About noon a youth began dancing with a wide fluttering movement of the flexed arms in imitation of a flying eagle. A woman dancer was waving her eagle down feathers before her eyes: this was interpreted as a sign of thirst. Attention was called to the fact that the eye-drippings of a trachoma sufferer had ceased. Between dances the folk dropped into the irrepressible light-hearted humor of the Shoshone. Jocularities were the key-note, and banter flew across the lodge from dancers to drummers and back.

On the second morning the sunrise greeting was repeated as before, with the same songs. During the intermission, however, important alterations in the lodge were effected. Straight-growing evergreens, twelve to fifteen feet in height, had been brought in from the mountains. These were stripped bare of limbs and bark to a height of eight feet and were planted in a half-circle about the west side of the lodge, each dancer having a place between two poles. Cotton sheets and squares of flour sacks were suspended from the poles and were dropped to form a screen at times during the day when the dancer in that section wanted to rest. Pictographic accounts of

vision experiences were drawn upon the screens of the medicine man and medicine woman. That of the man depicted a broad stream in blue running down the left side. On the land beside it and facing a representation of the medicine man was an otter. In the upper right corner was a spread-eagle on a stump. The pictographic account of the woman showed the sun rising over a range of jagged mountains in green. In the foreground stood a woman holding her horse by the halter rope. Before her stood the buffalo who gave her power.

The center pole was further decorated. Starting about eighteen inches from the ground a band of white clay was applied to a height of five feet. Further, the imaginary line across the front of the lodge was made very real by the erection of a heavy fence of quaking aspen. An entrance about ten feet across was left open before the center pole. The avowed purpose of the rail was to prevent the dancers from falling out of the sacred space when they fainted.

Finally, the poles of the screen were painted with white or yellow clay. The medicine woman added blue spirals to her poles, which were interpreted as representing the Thunder, another of her powers.

In the course of the day the dancers continued to add to the finery of their personal adornment, and a few of them produced fans of eagle wings. When the sun was high, the medicine man stood before the center-pole staring at the sun, holding before him his otter-skin as he prayed in silence (plate 13, right). Occasionally individual dancers stood before the pole with their hands upon it in order to soothe their burning bodies, for the pole and the ground about it give off a cool and moist feeling.

As the thirst of the dancers increased, informants said the buffalo head took on life to the dancers. Illusory effects of the buffalo moving, winking his eyes, and shaking his head in the manner of a bull are commonly experienced. The symbolism of the buffalo is associated with his ability to withstand thirst for long periods, as well as the fact that the bull leads the herd to water. Furthermore, it is maintained that the buffalo was the source of strength and life as the food-giver of the people and hence deserved a place of prominence in the worship. This is, of course, a rationalization of a trait which was taken over with the ceremonial complex, for among the Seed-eaters the buffalo was never a main economic stay.

Just about noon the medicine man confided to the band chief that he had dreamed they should bring the dance to a close at three o'clock the next day. The chief then announced this information to the assemblage. The chief was also given to haranguing the dancers from time to time, a rôle in which he was joined by a couple of old men. Late in the afternoon



a male dancer stood at the pole, and facing the sun in the west unfurled a small white banner upon a slender stick with eagle down feathers at either end. A sun, colored in green, surrounded by an inner circle of blue and an outer circle of red, from which alternately long and short rays in red streamed forth, was the emblem borne by it (plate 13, lower left). He prayed with this for the duration of a dance. That night an old man prayed with a flute outside the lodge. He moved slowly from the south around to the north, repeating a simple tune over and over. It was an individual prayer and not an essential part of the ceremony.

The sunrise ritual was repeated as before on the third morning. On this day two women dancers fainted and the climax of the dance was attained. There was great excitement when the faltering steps of the dancers were first noted. The drummers played and sang with a frenzy, and spectators emitted "war whoops."

With the collapse of a dancer activity ceased immediately, save that the face of the prostrate one was covered with a few sage leaves. Then the medicine man stepped forward and motioned for the body to be moved back to the dancer's place behind the screen. It was there covered with grass. He then called all the dancers out into the center of the lodge where they lined up facing the sun, men in front, women behind. The medicine man stood in the rear. After his prayer there followed more *na'βaβuk<sup>wi</sup>*. A deep religious satisfaction had replaced the morbid excitement of a moment previous, for it is held that the power of the sun comes down to revive the unconscious dancer and its good spreads to all present. It is also a real blessing to the fallen one. Visions usually appear during the period of unconsciousness, and upon revival all traces of thirst, hunger, and fatigue are claimed to have disappeared. Frank R. in such a faint was visited by a vision of Christ holding a glass half filled with a cool, red liquid and accompanied by an eagle; it entirely revived him. The women dancers remained unconscious for more than half an hour.

As the day wore on fewer and fewer dancers came out from the screen. At two-thirty the orchestra simply stopped and the dancing was done. Old men were called into the dance space to bless the dancers. Prayers were uttered with the hands cupped over the petitioner's head and at the end the old man lightly patted the body of the recipient. The prayer was to stop the thirst and to give the long life of the invoker to the dancer. A fee of a couple of dollars was paid for the service and those who could not afford it were blessed simultaneously, sharing the cost between them.

A prepared bucket of water and clay, blessed by the medicine man, was passed among the dancers, each receiving a quarter-cupful to induce vomit-

ing. The dancers quietly transformed themselves by donning overalls and shoes, dropping their old robes and some pieces of bead work at the foot of the center pole as they left the lodge. These were seized by old women. Boys rushed into the resting places behind the screens to search for trophies. It was a most unceremonious ending to a great ceremony.

The dancers bathed themselves at the creek, some also washing their feathers, after which they repaired to their lodges for food. There was great hilarity in the camp, gambling at cards and the hand game. Boys took over the dance lodge, imitating the dancers in movement and whistle. They were not molested until they started to play the drum, too. A ghost dance was called for that night, but there was no response.

The great feast took place during the afternoon of the following day. Strips of fresh beef were boiled and quantities of bread and canned goods were at hand. A substitute revival of the buffalo tongue trait was innovated by Jack Ramsay who procured steer tongues for the male and cow tongues for the female dancers. Part of the dance lodge was torn down to provide material for a wind screen before which the feast was laid. Except for portions pulled off to provide fuel for the camp-fires, the rest of the lodge was left to fall into decay.

In turning to the question of origin and history some interesting factors are revealed. That the sun dance had never been practiced by the Lemhi Shoshone at the time of his visit to them in 1906 was interpreted by Professor Lowie as evidence of its recent introduction among the Fort Hall Indians.<sup>7</sup> The ceremony in its present form was borrowed directly from the Wind River cognates. This acquisition occurred only thirty years ago. The medicine man, w3'f3kΛp, "Bear—," had failed in the cure of a tribeswoman because of the inadequacy of his powers. He was then told in a dream visitation to attempt a sun dance as he had seen it performed on the Powder River in his youth. He induced his people to participate, and the first ceremony was held on the Fort Hall Reservation in 1901. The cure was a success and the dance has been held yearly since that time with some interruptions due to interference by Indian Agents in the past. The Lemhi Shoshone after their transfer to Fort Hall and also the Bannock participated. After some years of joint participation on the part of the last named groups, their attitudes of social superiority towards the H3kan'dika caused them to separate and hold their sun dance apart from the latter.

That the sun dance is full of vitality is evidenced by its spread to the Shoshone on the Western Shoshone Reserve at Duck Valley, Nevada, in

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<sup>7</sup> R. H. Lowie, *The Northern Shoshone* (Anthropological Papers, American Museum of Natural History, Vol. 2, Part 2: 216, 1909)

the summer of 1933. These people, however, waived the tabu on food and water, for fear that they could not survive the ordeal! The initiator of the dance in Nevada came to Fort Hall in 1934 to participate in the dance there and to gain further experience. On the second day of dancing and abstinence he attempted to escape from the lodge, but was hauled back by the watchers, proving to their satisfaction that the Nevada Shoshone are weaker than they.

Both Lowie's Lemhi informants and my own stated that the ceremony at Fort Hall was taken from the Wind River Shoshone. Nevertheless, there is evidence that the dance was known to the Snake River Shoshone of the last century. The dances were said to have been held at the base of Bannock Peak in southern Idaho. Susie Yupe, age 60, had a grandfather who had officiated as leader in the ancient sun dance. This would have been about 1840. When hunting there thirty years ago, William Lipps and some companions found what they claim were remnants of the dance lodges. In small mounds about the decayed stocks of old center poles they recovered old bone beads and other relics. According to the current knowledge of the Indians, the dances of the old days were held infrequently and for the purpose of curing and divining the position of enemy bands before going east to enter the buffalo country. The leader, it is said, took a hair from the head of a dancer on the fourth night and placed it on the ground. It turned like a compass-needle and showed the enemy location. This action also guaranteed the taking of a scalp. No reasons are known why the ceremony was abandoned, nor when, none of the Indians now living having been old enough to have participated.

An origin myth is extant of a hunter who, dying of starvation, lay down to rest. He heard a singing voice and went over a hill to see. There he saw a buffalo which spoke to him and asked him if he was afraid. It promised to give him a great medicine. It told him how to build a lodge for the ceremony and how to perform the dance which was to last four days without any drinking or eating by the participants. The buffalo also gave him four songs to teach his people. These are the songs which are sung at the raising of the center pole and at the sunrise ritual. New songs are still given to the people by the buffalo in dreams.

Though the dance is now a yearly affair, it is essential that a leader receive a supernatural appointment to undertake the direction of the ceremony. There being no fraternities, leadership necessarily rests upon individual initiative. Since no bundles or fetishes exist as necessary paraphernalia, any person who receives a proper vision commission to lead the dance is eligible. Persons who have had such visions during the preceding

winter describe the nature of their dreams to the people at the community gatherings which take place every Sunday night. Several weeks before mid-summer, the people agree upon the most promising dream and acknowledge its owner as the leader of the coming dance.

The Həkan'dika permit women to participate by reason of the small size of the band. Medicine women are eligible to lead, but preference is given to men. This year a woman received a dream empowering her to dance and to use her power to cure people, but not to lead the ceremony, and she acted accordingly. Some informants said it was best to have a vision instructing one to enter the dance, but this requirement is not absolutely adhered to.

The main purpose of the ceremony is to cure ailments and to bring general welfare and happiness to the group at large. Rheumatism is most susceptible to the power of the sun dance. Though no spectacular cures were made this year for lack of patients, dancers were pointed out, of whom it was said that they were carried into the lodge the year previous because of their crippled condition. They became able to dance on the third day and have been well since. The psychic curative power of the ceremony is undoubted, although consumption and influenza are admitted by the Indians to be beyond its influence. Some persons dance for the purpose of seeking visions, thereby attaining medicine. This was probably the most important feature in earlier days.

An account of the Shoshone sun dance would not be complete without mention of the satisfactory rationalization of the symbolic paraphernalia in terms of Christian teachings. The sun is called God and the lodge is his temple. The cross-lying willow bundle is the body of Jesus and with the center pole forms the Crucifix. Through this the power of God is dispensed to mankind below. About the Savior are his twelve Disciples, represented conveniently in the twelve uprights of the outer wall. The Shoshone have limited the ceremony to three days and nights, because, by their own word, they tried a four day ceremony on one occasion and found it too exhausting. No matter—Jesus stayed three days and three nights in the Sepulchre, without food or water, emerging to the resurrection at the end. Just so do the Shoshone dancers reenact the event, gaining thereby new life and freshness of spirit.

Because of its direct diffusion from the Wind River it is possible to derive the Shoshone ceremony from an original Gros Ventre source, if Spier's interpretation is accepted; namely, "The Wind River dance resembles the Arapaho and Gros Ventre equally, but inasmuch as it lacks both bundle and fraternity organization, it was probably derived from the

later."<sup>8</sup> However, in the dance of the Wind River and the Fort Hall Indians are traits which are present in neither the Arapaho nor Gros Ventre, but which may be found in other tribes. Obviously contributions have been accepted from other sources.

Parallels and affinities for traits practiced by the Shoshone are in the main as follows: (1) The ceremony is called *ta'guwana*, "for want of water standing," and hence belongs with the group which gives the name "Thirst Dance" to the ceremony (Ute, Wind River, Plains Cree, and Plains Ojibway).<sup>9</sup> (2) Raising the center pole with crossed tipi-poles and ceremony: also by the Wind River, Crow, Blackfoot, Sarsi, Gros Ventre, Plains Cree, Assiniboine, Hidatsa, Arapaho, and Southern Cheyenne.<sup>10</sup> (3) Feints at pole-raising: also by the Wind River, Ute, Crow, Gros Ventre, Arapaho, and Southern Cheyenne. (4) The lodge is of the semi-roofed type of the Oglala. (5) The sunrise ritual appears also in the Wind River, Ute (?), Arapaho, Cheyenne, Oglala, Ponca, and Sisseton.<sup>11</sup> (6) Producing water from the center pole (I did not see this done at Fort Hall, but I was told that it had been done by powerful persons in recent years): also in the Gros Ventre, Assiniboine, and Plains Ojibway. There was no altar used by the Shoshone and, hence, it is probably absent from the Wind River and Ute ceremony, a point upon which Dr Spier was doubtful.<sup>12</sup>

On the other hand, Wind River practices upon which Spier expressed doubt, the occurrence of which is probably confirmed by reason of their appearance at Fort Hall, are the use of a prepared drink as among the Kiowa, Arapaho, Southern Cheyenne, and probably the Ponca, and vomiting as in the Arapaho and Southern Cheyenne.<sup>13</sup> The praying over the dancers by the old men at the end is a purely western addition, as it appears further only among the Wind River and Ute, with possibly the Sisseton.<sup>14</sup>

Checking the traits present in the Shoshone ceremony against the elements listed by Spier in his analysis of the total complex, it will be observed that only a vestige of the full ritual of the dance activities carried over to the Shoshone. It is clear, therefore, that the sun dance as it exists on the Fort Hall reservation is wholly peripheral, both geographically, and in point of view of attenuation of content. In the eyes of an Assiniboine observer, it is "a travesty, nothing more."

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<sup>8</sup> Spier, *op. cit.*, page 495.

<sup>10</sup> *Ibid.*, page 470.

<sup>12</sup> *Ibid.*, page 471.

<sup>14</sup> *Ibid.*, page 475.

<sup>9</sup> Spier, *op. cit.*, page 463.

<sup>11</sup> *Ibid.*, page 474.

<sup>13</sup> *Ibid.*, pages 475-76.

## AREAL AFFILIATIONS OF CALIFORNIA FOLKTALES

By A. H. GAYTON

**B**ASIC contributions to our knowledge of California mythology were made by Dixon, Goddard, and Kroeber during the early years of this century. These consist of collections of tales and of discussions of relationships between various tribal mythologies. In 1907 Kroeber brought their findings together in a paper which graphically characterized folktale types throughout California and drew them into a common cultural perspective.<sup>1</sup>

During the nearly thirty intervening years the record of California mythology has been greatly enlarged, as has that of the contiguous regions, particularly the Basin. The additions from northern California, southern Oregon, the Basin, and the lower Colorado River region are many. New data from the southern half of California are less plentiful. There the major evidence consists of four unpublished collections: the superb Tubatulabal tales of Mrs Erminie W. Voegelin, a full roster of Owens Valley Paiute tales of Dr Julian Steward, a series of Yokuts texts taken by Dr Stanley Newman,<sup>2</sup> and some Yokuts and Western Mono tales recorded incidentally to an ethnographic investigation by the present writer. Unfortunately the mythology of the Shoshonean and Yuman tribes of extreme southern California remains relatively unknown.

Kroeber's early paper on California mythology defines the characteristics of that area alone, though some recognition is made of resemblances to adjoining or more distant regions. While he describes those features of the tales which are common to the central portion of the state, he stresses the differences between the North Central and South Central sections. In the light of accumulated data and with emphasis on local traits relieved, the affiliations of folktales within the so-called California culture area with those of surrounding areas become more evident, as will be demonstrated herein. These affiliations show that the essential differences between North Central and South Central mythology lie in the nature of their external relationships. The present paper, the by-product of a specific comparative study of a particular tribal mythology,<sup>3</sup> does not presume to offer the evidence of exhaustive research: it indicates generalities which subsequent analytical studies may or may not substantiate.

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<sup>1</sup> Kroeber, *Indian Myths of South Central California*. References to the contributions of Dixon, Goddard, and others are to be found therein, pp. 170-98.

<sup>2</sup> I am deeply indebted to these investigators and to Mr Willard Park, Dr W. W. Hill, and Dr Leslie Spier for the use of their unpublished folktales.

<sup>3</sup> *Yokuts and Western Mono Myths*, ms.

Areal distinctions based on mythology, as laid out by Kroeber in 1907, coincide with the three ethnic areas of California as conceived by him both then and later.<sup>4</sup> These consist of a small Northwestern coastal area focusing among the Hupa, Yurok, and Karok; a Southern area covering the south extremity of the state and inhabited by Shoshonean and Yuman speaking peoples; and a third area, the great Central portion including all the coastal and inland territory intervening between the other two. It is the latter region which is again subdivided into North Central and South Central sectors.

The differences on which his delimitations are based are several, but primarily the determining factor is the nature of creation myths, or the lack of them. The Northwestern area lacks creation myths, as do the tribes of the extreme south, but in the Central region they are well developed; in the North Central part a high god or one-who-is-above creates the world, whereas Eagle, assisted by other animals accomplishes this task in the South Central half. In the Northwestern area the origin of culture is attributed to a great culture hero, also in part to a conquering adventurer, both members of a proto-typical yet supernatural race of long since departed people. In the Southern area the advent of worldly phenomena, both physical and cultural, are ascribed to an Earth-mother, her brother-lover and their son (a "dying god"), or to two brothers who are culture givers. The North Central area in part attributes the establishment of present day customs to their creator, and in part subscribes to the notion, held in the South Central region as well as elsewhere in North America, of natural or cultural origins resulting from thefts and controversies conducted by animal predecessors, usually Coyote. In the realm of novelistic tales the Northwestern area has many hero stories, particularly those of the from-rags-to-riches type. In the Central region less elaborate adventure stories exist: the North Central region possesses Evil-Father-in-Law, Lost Brother, Dug-from-the-Ground, Bear and Deer, and the Loon Woman tale, the South Central region possesses tales of a Prairie Falcon hero who rescues his wife, loses his eyes, or has other simple adventures, Mikiti, and the tale of a man who followed his wife to the land of the dead (Orpheus myth).

Thus too briefly are Kroeber's areal distinctions summarized.<sup>5</sup> They are

<sup>4</sup> Indian Myths, 169, 170, Handbook, 898 ff., fig. 73. In the present paper the terms Northwestern, Southern, Central, North Central, and South Central California refer to Kroeber's folktale or culture areas; the terms northern and southern California to geographic halves of the state.

<sup>5</sup> Discussed in full in his Wishok Myths, 85-90; Two Myths, 309 ff., and Indian Myths, 195-98. See also Kroeber, Patwin, 303.

well founded and are largely corroborated by later folktale material. The new material, however, filling in geographically many gaps in the earlier data, either extends the old limits, as of the little Northwestern area, or modifies their definition, as in the Central region.

The Northwestern area Kroeber characterized by its type of culture hero, lack of creator concept, and belief in a proto-typical, supernatural race<sup>6</sup> on the basis of Hupa, Yurok, and Karok mythology.<sup>7</sup> The Wiyot he regarded as somewhat peripheral because they possessed a high-god or creator as well as a culture hero.<sup>8</sup>

It seems to the writer that the distinction between creator and culture hero is not clearly definable in this region. The Hupa culture hero did not create the world, nor did he create people of sticks or feathers in the usual North Central Californian manner, but aside from this there is little to distinguish his constructive or transforming activities from those of the creators to the immediate south.<sup>9</sup> There, in the mythology of Kato, Yuki, and Pomo,<sup>10</sup> the creator-culture hero-transformer theme is developed in a detailed literary form. This has no equivalent in Central Californian tales, except in those of the Modoc, Achomawi, and Maidu which also exhibit the attenuated transformer concept of the Southern Plateau (compare e.g., Klamath, Wishram, Sanpoil, or Okanagon<sup>11</sup>), wherein the creation of the earth and man appear to be but two events among many others taken in the Kato, Yuki, and Pomo heroes' stride. The transition from the superhuman hero of the Northwest to the superanimal hero of the Pomo is best exemplified by the Yuki myths in which Coyote is not merely an agreeable companion to the hero, Taikomol, but is himself the creator in an analagous cycle of tales of different content.<sup>12</sup> The Pomo creative hero is Coyote, though Wolf too is given prominence. The entire treatment of this preëminently California-Basin figure is in the Northwest manner: a fool he is at times, but he creates people, institutes customs, carves salmon from wood or releases them, causes a deluge when angered, destroys monsters; in short,

<sup>6</sup> Wishosk Myths, 91.

<sup>7</sup> Kroeber's Karok myths are available only through references to them in Du Bois and Demetracopoulou's *Wintu Myths*, pp. 397-401. J. P. Harrington's brief collection is an illuminating addition to Karok mythology.

<sup>8</sup> Wishosk Myths, 94 ff.; Reichard, *Wiyot Grammar*, 151, 153 ff.

<sup>9</sup> Goddard, *Hupa Texts*, 96.

<sup>10</sup> Goddard, *Kato Texts*, 183, 184; Kroeber, *Yuki Myths*, 906-12, Barrett, *Pomo Myths*, 84 ff.; A Composite Myth, 37-51.

<sup>11</sup> Spier, *Klamath Tales*, Sapir, *Wishram Texts*, 3-47; Curtiss, 8, 106 ff.; Ray, 157-77; Cline, ms.; Boas, *Kutenai Tales*, 88-125, 281 ff.

<sup>12</sup> Kroeber, *Yuki Myths*, 906-17, 918-26.



is the hero of a series of stories reminiscent of the great hero cycles to be found to the north.<sup>13</sup>

Coyote also serves the Patwin as a creator-culture hero, and in such a manner as to indicate that Patwin creation tales are derived largely from the Pomo. The Central California earth diving theme and Falcon hero also figure in the myths.<sup>14</sup>

Traits which were thought of by Kroeber as specifically Northwestern have a distribution well beyond the bounds of the Hupa-Yurok-Karok unit. Like the Yurok and Wiyot culture heroes, Kumush (Old Man) of the Modoc attempts to destroy his son in order to obtain his daughter-in-law: this is known from the Puget Sound region.<sup>15</sup> The tale of two girls sent to seek a husband is recorded from the Modoc, Achomawi, Yana, and Wintu of the interior, as well as the Karok, Wiyot, Takelma, and Coos on the coast, and other tribes to the north.<sup>16</sup> The story of a scabby or abandoned boy who becomes rich is similarly shared by Karok, Chilula, Wintu, and Modoc,<sup>17</sup> as is that of the rescue of a stolen brother, by Coos, Tillamook, Hupa, Karok, Wintu, Yana, Shasta, Achomawi, Modoc, and Klamath.<sup>18</sup> The Loon Woman tale follows largely the same distribution.<sup>19</sup> The story of Coyote's failure to obtain power, recorded from the Karok, is rendered in almost identical terms by the Southern Okanagon.<sup>20</sup> Antelope and the Coyote Boys, told by the Wintu, is the theft of a hoop tale known from the Puget Sound-Southern Plateau region.<sup>21</sup>

Five or ten siblings as a character group is a favorite formula of the Kato, Chilula, Hupa, Wintu, and Modoc in California, and Klamath, Coos, Tillamook, and Wishram among many others to the north.

In short, in this group of tales, the whole northern quarter of California,

<sup>13</sup> Barrett, Pomo Myths, 45 *passim*; Boas, Tsimshian Mythology, 565 ff., 618 ff.

<sup>14</sup> Kroeber, Patwin, 303, 304.

<sup>15</sup> Curtin, 12. Also, for example, Wishram and Coast Salish. Curtis, 8, 132; Haeberlin, 400; Adamson, 389.

<sup>16</sup> Curtin, 27; Dixon, Achomawi and Atsugewi, 163; Achomawi Myths, 283; Sapir, Yana Texts, 133; Du Bois and Demetracopoulou, 341; Harrington, Karok Indian Myths, 23; Reichard, 143; Sapir, Takelma, 64; St. Clair, 35; Boas, Kutenai, 308.

<sup>17</sup> Harrington, *op. cit.*, 16; Goddard, Chilula Texts, 359; Du Bois and Demetracopoulou, 320; Curtin, 17.

<sup>18</sup> St. Clair, 32; Boas, Traditions of Tillamook, 136; Goddard, Hupa Texts, 154; Du Bois and Demetracopoulou, 398, note 45 [Kroeber, Karok, 3763], 311; Sapir, Yana Texts, 214; Curtin, 300 ff.; Spier, Klamath Tales.

<sup>19</sup> Demetracopoulou, Loon Woman Myth, 102, 103, Spier, *op. cit.*; Lowie, Assiniboine, 160.

<sup>20</sup> Powers, 35; Chene, ms.

<sup>21</sup> Du Bois and Demetracopoulou, 302, Adamson, 390; Boas, Kutenai, 299.

from coast to Sierras, aligns with the coastal and inland area of Oregon and Washington.

Elements of Northwestern tales extend south and east beyond the Hupa-Yurok-Karok group. An adventurous journey across the ocean, sometimes resulting in the theft of foodstuffs, occurs not only in Salmon River Shasta and Wiyot myths as noted by Kroeber,<sup>22</sup> but also in those of Modoc, Wintu, Kato, Yuki, Pomo, Patwin, and Nisenan.<sup>23</sup> Found also in Pomo mythology are the concepts of thunder as a Thunderbird, and of a water serpent or monster, and of many supernatural non-animal beings. The contest theme is strongly developed and frequently has an evil father-in-law for the motivation, as on the Northwest Coast.<sup>24</sup> In literary style Kato, Yuki, and more particularly, Pomo myths embody a richness of imaginative detail which points to northern affiliation rather than southern or eastern: magical devices abound such as magic flute, surrogate tokens, lightening of loads, snapping or guarded doors.<sup>25</sup>

Before leaving Pomo mythology, which will be considered again from the Central California viewpoint, the absence of certain tales found east of the Coast Range in northern California should be noted, namely, Loon Woman, Rolling Skull,<sup>26</sup> Child-Stealing Ogre, Shabby Suitor, and the popular trickster tales of Bungling Host, Long Penis, and Coyote and his Daughters.<sup>27</sup> The Pomo trickster tales are nearly all local forms differing in content from their analogues of the Basin and Southern Plateau.

The origin myth is again the determinant of Kroeber's Southern folktale area.<sup>28</sup> The general form is characteristic of the Yuman group in Arizona, their linguistic relatives in California, and the neighboring Shoshoneans, viz., the Maricopa, Yavapai, Havasupai, Walapai, Yuma, Diegueño, Serrano, and Cupeño.<sup>29</sup> This epic myth tells of two quarreling brothers who

<sup>22</sup> Wishok Myths, 93, Indian Myths, 180, in 3.

<sup>23</sup> Curtin, 180, Du Bois and Demetrapoulou, 321; Goddard, Kato Texts, 207; Kroeber, Yuki Myths, 912, Barrett, Pomo Myths, 46, 55, 191, 279, 375, Kroeber, Patwin, 304, Valley Nisenan, 276, possibly also Coast Miwok, cf. Merriam, 203.

<sup>24</sup> Titles of tales referring to the hero's source, Dug-from-the Ground, are really misnomers, as miraculous births are commonly attributed to heroes of contest tales. Several Evil Father-in-Law stories of northern California are obscured by titles referring to this incident of relatively local distribution, cf. Du Bois and Demetrapoulou, 291, 397, notes 9, 10, Barrett, Pomo Myths, 463, Boas, Tsimshian Mythology, 794 ff.

<sup>25</sup> Barrett, Pomo Myths, 460.

<sup>26</sup> Represented by one aberrant tale, *ibid.*, 231.

<sup>27</sup> Present are Coyote and Mother-in-Law, and Coyote Disguised as Old Woman, *ibid.*, 241, 244.

<sup>28</sup> Two Myths, 309 ff.

<sup>29</sup> Spicer, Yuman Tribes, 345, Gifford, Northeastern and Western Yavapai, 349, 402.

emerge from the waters to make people, the sun and moon, and to institute customs: one of the brothers proves to be a "dying god" whose death involves several important episodes. The Luiseño and Mohave accounts, as given by Kroeber,<sup>30</sup> are closely analogous. The major innovation in both is the change of the siblings to an incestuous brother and sister: from the latter all things of the world are born. In the light of other accounts it seems possible that this modification is due to an assimilation of the inherently Shoshonean male-female progenitor concept common to the adjacent Chemehuevi, Tübatulabal, Western Mono,<sup>31</sup> Washo, Shivwits, and others (see below), although the rendering is in cosmic terms suggestive of Pueblo affiliations.<sup>32</sup> The Pass Cahuilla combine both ideas by having the two brothers born from a nebulous mother and father.<sup>33</sup>

One tale centering among the Yumans but having a wider distribution is also found in the Southern area: this is Flute Lure or the Sun's Boys recorded from Serrano and Diegueño and also from at least Havasupai, Yavapai, Maricopa, Shivwits, and Southern Ute<sup>34</sup> in the Southwest.

Little is known of other tales from the Southern area. Those recorded from the Luiseño, Cahuilla, and Diegueño are largely of a nondescript type which might form the background of any myth collection from the Pacific slope or Basin. The story of Takwish, the wife-stealing meteor, is a local variant of the stolen-relative theme popular throughout California and the Basin. The form prevailing over the entire southern half of California and also known in the southern Basin has the loss of a wife as its motive: Cahuilla, Tubatulabal, Yaudanchi Yokuts, Owens Valley and Kaibab Paiute.<sup>35</sup>

The Serrano, more adequately represented than any other group of

Spier, Havasupai, Nos. 1-8; Kroeber, Walapai, 12, 245; Harrington, A Yuma Account, 328; Waterman, 338; Benedict, 1, Strong, 268

<sup>30</sup> Two Myths, 312, 315

<sup>31</sup> The Luiseño Earth-mother, Tamaiaiwot, the Mother of All (Kroeber, Two Myths, 912), is specifically paralleled by the Wobonuch Western Mono who localize the home of their Mother of All, Tabiyawet, near Huntington Lake in the Sierra Nevada Mountains (Gayton, Yokuts and Western Mono, ms)

<sup>32</sup> Kroeber discusses the Pueblo affiliations in the Handbook, 788-92.

<sup>33</sup> Strong, 130.

<sup>34</sup> Benedict, 2; Du Bois, Story of Chaup, 217; Spier, Havasupai, No. 9, Gifford, North-eastern and Western Yavapai, 353, 404, Spier, Yuman Tribes, 367; Lowie, Shoshonean Tales, 76, 190. Also Navaho, Pima, and Papago, and probably related to the twin war god tales of the Pueblos: Matthews, 105, Densmore, 54; Herzog, ms.

<sup>35</sup> Hooper, 364; Voegelin, ms; Kroeber, Indian Myths, 221, Steward, ms; Sapir, Texts of Kaibab, 367; cf. Stephen, Hopi Tales, 21, for a Pueblo example.

southern California, and their Shoshonean relatives to the north and east, the Kitanemuk, Chemehuevi,<sup>36</sup> and Tubatulabal of the San Joaquin Valley, have so many Basin tales and elements in their mythology that they constitute a Californian appendage to the Basin mythologic area. The non-Basin tales of the Tubatulabal are held in common with their Yokuts neighbors, particularly the Yauelmani and Paleuyami of the southern valley.

This brings us within the Central California region. Again taking the creation tale as a criterion, we find that here interest is centered on the making of the world, and is embodied in a myth postulating an original world of primeval water, in which an already existing creator (usually Eagle or another raptorial bird) solves the problem of earth-making by the familiar earth-diver method. It is known to the Tubatulabal, Wobonuch and North Fork Western Mono, Salinan, Yokuts of the central valley and foothills, Southern Miwok, Patwin, and Northwestern Maidu (as an incident), and Wintu.<sup>37</sup> The primeval water and earth-diver incidents are far flung in general distribution, but in California are concentrated in this rather isolated group completely surrounded by tribes favoring deluge incidents (i.e., caused, or arriving and receding floods).<sup>38</sup>

The establishment of culture seems to hold no narrative interest for the people of Central California; the Pomo, Patwin (indirectly), and Maidu, who participate in the culture-hero-transformer area of the Northwest Coast and interior to which they are peripheral, alone have tales dealing with this subject.

The Growing Rock tale is distinctive of this region, being found among Pomo, Wintu, Miwok, North Fork Mono, Kechayi and Yauelmani Yokuts, and Tubatulabal,<sup>39</sup> although the incident of a rising rock is more widely distributed in the west.<sup>40</sup> Our knowledge of the Thunder Twin story has not been clarified by new material: it remains random in distribution and variable in plot.

<sup>36</sup> On the basis of one tale each Kroeber, *Indian Myths*, 243; *Origin Tradition*, 240.

<sup>37</sup> Voegelin, ms., Gilford, *Western Mono*, 305; Gayton, ms.; Mason, *Language of Salinan*, 82, 105, Kroeber, *Indian Myths*, 202, 204, 210, 218, 229; Newman, ms.; Barrett, *Myths of Sierra Miwok*, 4; Kroeber, *Patwin*, 303, 304; Dixon, *Maidu Myths*, 39, Du Bois and Demetracopoulou, 287.

<sup>38</sup> Reichard, *Literary Types*, 274.

<sup>39</sup> Barrett, *Pomo Myths*, 307; *Myths of Sierra Miwok*, 22; Du Bois and Demetracopoulou, 370; Powers, 366; Gilford, *Western Mono Myths*, 355, Gayton, ms., Newman, ms.; Voegelin, ms.

<sup>40</sup> For example, Navaho, Paviotso, Ute, Maidu, Yana, Wasco: Hill, ms.; Park, ms.; Kroeber, *Ute Tales*, 272; Dixon, *Maidu Myths*, 79, 81; Sapir, *Yana Texts*, 208; *Wishram Texts*, 264.

It is notable that these tales by no means include all the tribes of the Central area in their distribution, which clusters about the exact middle of the state and excludes those tribes mentioned above as having northern affiliations. The Prairie Falcon hero and Eagle chief are favorite characters in this nuclear group; single contests are preferred to multiple tests; revival is accomplished by submersion in water.

It is difficult to find other tales or elements that belong strictly to the area designated by Kroeber as Central California. The Lizard Hand motif, an argument between Lizard and Coyote over the shape of the human hand, occurs as far north as the Karok, southward through Central California, and continues in closely analagous form with the Cahuilla and Maricopa.<sup>41</sup> The tale of a controversy over the origin of death is reported from one end of the state to the other as part of a continuous distribution ranging over all the surrounding regions.<sup>42</sup> The episode of the subsequent death of the proponent's child is not found south of the Yokuts and Tübatulabal in California. More general controversies over the method of childbirth, or mode of life, either in anecdotal or incidental form, are scattered over the greater part of California<sup>43</sup> and elsewhere in western North America.

The determination of the length of the seasons is recorded from such California tribes as were in contact with the Southern Plateau and Basin where it is popular, namely, from the Yana, Modoc, and Atsugewi.<sup>44</sup>

The tales of the Theft of Fire and the Theft of the Sun are found in Central and Northwestern California, though not in the Southern area. However, their distribution continues northward on the Pacific coast and eastward into the Basin. The Theft of Fire tale has a form common to California and the Basin, as opposed to that involving arrow-chain and war on the sky incidents found in the Southern Plateau-Puget Sound region.<sup>45</sup> The California-Basin form again divides into two groups. A single thief who enters the fire-keepers' home and steals fire while they sleep is credited with the feat by the Yokuts, Western Mono, Sierra Miwok, Pomo, Yana, and Northeastern Maidu<sup>46</sup>—a southern and eastern grouping. The

<sup>41</sup> Harrington, Karuk Indian Myths, 26, Barrett, Pomo Myths, 470; Strong, 135; Spier, Yuman Tribes, 346; Thompson, 288, note 59.

<sup>42</sup> Cf. Thompson, 284, note 51; Du Bois and Demetracopoulou, 299, 300, 398, notes 32, 33; Barrett, Pomo Myths, 457, Voegelin, ms; Steward, ms; Strong, 135; Kroeber, Patwin, 308.

<sup>43</sup> Random examples: Karok, Pomo, Maidu, Western Mono, Cupeño, Yuma: Harrington, Karuk Indian Myths, 26, Barrett, *op. cit.*, 102; Dixon, Maidu Myths, 46 ff; Gayton, ms; Strong, 268; Harrington, A Yuma Account, 330.

<sup>44</sup> Sapir, Yana Texts, 211; Curtin, 58; Dixon, Achomawi and Atsugewi, 171.

<sup>45</sup> Recorded in aberrant form from the Tolowa [?]; Powers, 70.

<sup>46</sup> Kroeber, Indian Myths, 219, Gayton, ms; Gifford, Miwok Myths, 285, 332, Barrett, Pomo Myths, 310, Sapir, Yana Texts, 32, 171; Dixon, Maidu Myths, 65.

second form, wherein the thieves mingle socially with the fire-owners and the stolen embers are brought home by relay runners, is shared by Karok, Wintu, Shasta, Achomawi, Maidu, Modoc, Ute, Northern Shoshone, Southern Ute, Shivwits, Kaibab and Owens Valley Paiute<sup>47</sup>—that is, in northern California and the Basin. A feature which is primarily characteristic of Basin and Plateau mythology, the making of a rain or snow storm to hinder someone's escape, occurs in California in the Tubatulabal, foothill Yokuts, Sierra Miwok, Maidu, Achomawi, Yana, and Modoc Theft of Fire tales, as well as in those of the Southern Ute, Shivwits, and Kaibab Paiute in the Basin.<sup>48</sup>

The Theft of the Sun myth is found in California as far south as the Yokuts: it is absent from the Basin and Oregon, its northern distribution being resumed in the southern Puget Sound region of Washington. In California the form of the tale largely follows the Theft of Fire pattern, both myths having several incidents in common. It bears but slight similarity to its analogue of the Northwest Coast.<sup>49</sup>

Acorns, pinenuts, obsidian, dentalia, and a harpoon are the objects of theft in tales shared by a few tribes of northern California and the Basin, namely, Wiyot, Wintu, Achomawi, Atsugewi, Owens Valley Paiute, Paviotso, and Moapa.<sup>50</sup>

The last several tales we have been discussing include practically all of Central California as part of continuous distributions covering major parts of western North America. It will be remembered that Kroeber subdivides the Central area into North Central and South Central sections. This may be reemphasized as follows.

The creation of man and the founding of tribes is unformulated in the southern half of Central California. The concept of the creation of man from sticks (secondarily, feathers or mud) prevails over the northern portion with a coastal extension southward to include the Coast Miwok and

<sup>47</sup> Powers, 38; Du Bois and Demetracopoulou, 398, note 41 [Kroeber, Karok, 3799, 3866], 304, Dixon, Shasta Myths, 13; Achomawi and Atsugewi, 165; Dixon, Maidu Myths, 66, 91; Curtin, 52; Kroeber, Ute Tales, 258; Lowie, Northern Shoshone, 245; Shoshonean Tales, 6, 118; Sapir, Texts of Kaibab, 392, Steward, ms.

<sup>48</sup> Voegelin, ms.; Kroeber, Indian Myths, 219; Gayton, ms.; Gifford, *op. cit.*, 285, 333; Dixon, Maidu Myths, 67; Achomawi and Atsugewi, 165; Sapir, Yana Texts, 173; Curtin, 57; Kroeber, Ute Tales, 259; Lowie, Shoshonean Tales, 118; Sapir, Texts of Kaibab, 393.

<sup>49</sup> Cf. Du Bois and Demetracopoulou, 398, note 35; Barrett, Pomo Myths, 600; Gayton, ms.; Boas, Tsimshian Mythology, 641.

<sup>50</sup> Reichard, Wiyot Grammar, 173, Du Bois and Demetracopoulou, 301, 302, 305; Dixon, Achomawi and Atsugewi, 160, 174; Steward, ms., Park, ms.; Lowie, Shoshonean Tales, 160, 217, 221

Salinan, and ranges into the Basin to the Northern Shoshone and Southern Ute in weak form.<sup>51</sup>

Two favorite tales, Bear and Deer and the Orpheus myth, are almost mutually exclusive in distribution in California; the first northern, the second southern, as Kroeber pointed out.<sup>52</sup> However, neither is characteristically Californian save in the form of local variants. The first is found not merely in North Central California, but is reported for nearly all the tribes of northern California including the Yurok and Modoc; from the Klamath, Takelma, Kathlamet, Coast Salish, Snohomish, Comox, Thompson, and Kwakiutl to the north; from the Northern Shoshone, Southern Ute (aberrant), North Fork Mono, Tübatulabal, and Serrano of the Basin region; and from several of the Pueblos.<sup>53</sup> The Orpheus myth, distributed from the Maidu southward to the Gabrielino, has Paviotso and Navaho as nearest non-Californian cases in its continent-wide distribution.<sup>54</sup>

Trickster stories are weak or absent from the southern half of the state, with the exception of the Tübatulabal and Serrano who possess a number of Basin myths. Coyote, the rascally hero of the central Pacific region, is usually found in the major rôle. His scattering of deer by creating a stench is recorded for Yokuts, Owens Valley Paiute, Southern Ute, and Yavapai.<sup>55</sup> Kroeber records a Yauelmani Yokuts tale related to Tübatulabal, Owens Valley Paiute, and Paviotso myths, containing a few Coyote incidents of the trickster or dupe type.<sup>56</sup>

Several tales of northern California involving Coyote's trickery are well known from the Basin and elsewhere. Probably the most popular is Coyote and his Daughter, analyzed by Schmerler. Nineteen of her twenty-nine cases have a provenience west of the Rocky Mountains: to her distribution, which includes Wintu, Yana, Maidu, North Fork Mono, the major Basin tribes, and the Serrano, may be added Hupa (distorted), Owens

<sup>51</sup> See Barrett, *Pomo Myths*, 468, 469, for references to Pomo, Coast, Lake, and Sierra Miwok, Wintun, Yana, and Yuki; Mason, *Language of Salinan*, 105; Dixon, *Achomawi and Atsugewi*, 159, 169; Du Bois and Demetracopoulou, 378; Lowie, *Northern Shoshone*, 278, *Shoshonean Tales*, 3, 4. See also for Skagit, Nutka, and Bella Bella, Haeberlin, 419; Boas, *Tsimshian Mythology*, 913; *Bella Bella Tales*, 34, 35, 39.

<sup>52</sup> *Indian Myths*, 198.

<sup>53</sup> Jean Sapir, 259; Marriott, ms.; Spier, *Klamath Tales*; Sapir, *Takelma*, 117; Boas, *Kathlamet Texts*, 118; Adamson, 407; Haeberlin, 442; Lowie, *Northern Shoshone*, 253; *Shoshonean Tales*, 58; Gifford, *Western Mono*, 357; Voegelin, ms.; Benedict, 16. For Thompson, Comox, and Pueblo references see Ruth L. Bunzel's footnote to Dangel, 307, fn. 1.

<sup>54</sup> Gayton, *Orpheus Myth*.

<sup>55</sup> Gayton, ms.; Steward, ms.; Lowie, *Shoshonean Tales*, 17; Gifford, *Northeastern and Western Yavapai*, 412.

<sup>56</sup> Kroeber, *Indian Myths*, 231; Voegelin, Steward, and Park, works cited.

Valley Paiute, and Yavapai.<sup>57</sup> Coyote and the Swimming Girls (or Long Penis), usually with the Coyote as Doctor episode following, is recorded from northern California (Chilula, Shasta, Wintu), the Basin (Northern Shoshone, Paviotso), to the south among Yavapai, and is well known in the Southern Plateau.<sup>58</sup> Coyote and his Mother-in-Law is reported from Tubatulabal, Paviotso, Yana, and Pomo;<sup>59</sup> Coyote's Penis Baby is an incident in Tubatulabal, Paviotso, Moapa, Maidu, Yana, and Wintu tales.<sup>60</sup> These stories are rare or absent in the Northwestern area of California. Nevertheless, one trickster story is characteristic of this coastal region; the one wherein Coyote disguises himself as a baby and floats down a stream to intrigue women: Pomo (aberrant), Yuki, Kato, Hupa, Klamath, and Northern Shoshone. It is found as an incident in salmon release tales continuously northward into the Southern Plateau.<sup>61</sup>

The Rolling Head (Skull, Rock) theme, which is variously a basic or incidental myth element over a large part of North America, is significant in its Californian distribution. Like many of the items cited above its distribution embraces the northern portion of the state and the Sierra Nevada region contiguous to the Basin: Pomo and Wappo (both aberrant), Wintu, Yana, Modoc, Shasta, Maidu, North Fork Mono, Tubatulabal, Paviotso, Ute, and Northern Shoshone.<sup>62</sup>

A great number of incidents and details, such as ogre with pestle, ogress with burden basket, transformation to stone, fastened-hair trap, kill-all

<sup>57</sup> Schmerler, 196-207; Goddard, Hupa Texts, 239; Steward, ms.; Gifford, *op cit.* Also Okanagon, Sanpoil, and Navaho: Cline, ms.; Ray, 174; Hill, ms.

<sup>58</sup> Goddard, Chilula Texts, 362; Dixon, Shasta Myths, 34; Du Bois and Demetracopoulou, 386-88, Lowie, Northern Shoshone, 250, Park, ms.; Gifford, Northeastern and Western Yavapai, 400. Also, for example, Wishram, Skagit, Nespelim, Sanpoil, Okanagon, and Nez Percé. Sapir, Wishram Texts, 11, Haeberlin, 393, Teit, Okanagon Tales, 71; Ray, 175; Cline, ms.; Spinden, 189. For Coyote as Doctor references see Du Bois and Demetracopoulou, 401, note 130, to which may be added Yuki, Modoc, and Tillamook: Kroeber, Yuki Myths, 938; Goldman, ms.; Boas, Traditions of Tillamook, 14.

<sup>59</sup> Voegelin, ms.; Park, ms.; Sapir, Yana Texts, 112; Barrett, Pomo Myths, 241.

<sup>60</sup> Voegelin, ms.; Park, ms.; Lowie, Shoshonean Tales, 177; Dixon, Maidu Myths, 89; Sapir, *op cit.*, 158; Du Bois and Demetracopoulou, 386.

<sup>61</sup> Barrett, Pomo Myths, 245, Kroeber, Yuki Myths, 939; Goddard, Kato Texts, 219, Hupa Texts, 125, Spier, Klamath Tales; Lowie, Northern Shoshone, 275. Other examples, Tillamook, Wishram, Snohomish, Nez Percé, Thompson: Boas, Traditions of Tillamook, 145, Sapir, Wishram Texts, 3, Haeberlin, 404, Spinden, 139; Teit, Thompson Tales, 7; cf. Boas, Kutenai Tales, 301.

<sup>62</sup> Barrett, *op cit.*, 231, Radin, 141; Du Bois and Demetracopoulou, 362, Sapir, Yana Texts, 124, 202, Dixon, Shasta Myths, 21, Maidu Myths, 97, Curtin, 189; Gifford, Western Mono, 351, Voegelin, ms.; Park, ms.; Kroeber, Ute Tales, 260; Lowie, Northern Shoshone, 262; Shoshonean Tales, 201, 203. For general distribution see Thompson, 343, note 238.



arrow, vulnerable spot, underground traveling and so on, are distributed over northern California and held in common with contiguous regions to the north and east. They fail to appear in a central California nucleus composed of Patwin, Miwok, all the Yokuts tribes, and the Salinan, probably the Costanoan and Coast Miwok, and in part, the Pomo. To this area of absence may probably be added the remaining tribes of southern California, with the exception of the Serrano, though the lack of evidence will not permit final conclusions to be drawn.

Beside some tales shared with northern California, as already pointed out, there are a few typical of the Basin which impinge upon the mythology of certain California tribes along the Sierran highland. The folktales of the Shoshoneans of the Great Basin are best known through Lowie's representative collections. The area is marked by its uniformity, though analysis would, of course, reveal definite cleavages.

Here little or no interest is shown in the creation of the world, whereas the origin of people is described in two tales common to nearly all the Shoshonean groups. The essential element is the belief in male-female conception: people are not manufactured by a creator from some tangible substance or by fiat. The tale has two variants. In one, an original old woman and her daughter (both usually with toothed vaginas) dwell in the center of a lake; Wolf, Coyote, or "a man" marries the daughter; their children, born in a jug, are released too soon; the quarrelling children are dispersed to become different tribes. This form is found among the Chemehuevi and Owens Valley Paiute in California, and the Kaibab Paiute, Shivwits, Moapa, Paviotso, Washo, Southern Ute, and Northern Shoshone of the Basin.<sup>63</sup> The Old Woman is known to the Northeastern Maidu and the Wobonuch Western Mono.<sup>64</sup>

The second form of the tale is of a woman who, after many misadventures, among them escape from a rolling skull, arrives at the home of Wolf, Eagle, or "a man," who becomes her husband and the father of mankind. This variant is known to the Tubatulabal, Wobonuch Western Mono (aberrant), North Fork Mono, Owens Valley Paiute, Paviotso, and Modoc; that is, the marginal region west of the Basin.<sup>65</sup>

There are no culture establishment tales in the central Basin. Cottontail is an indifferent transformer in one story.<sup>66</sup> To the south the Yumans

<sup>63</sup> Kroeber, *Origin Tradition*, 240; Steward, ms., Sapir, *Texts of Kaibab*, 358; Lowie, *Shoshonean Tales*, 103, 157, 209, 216; Northern Shoshone, 236, Dangberg, 439.

<sup>64</sup> Dixon, *Maidu Myths*, 50; Gayton, ms.

<sup>65</sup> Voegelin, ms.; Gifford, *Western Mono*, 317; Steward, ms.; Lowie, *Shoshonean Tales*, 200-209; Curtin, 137, 140.

<sup>66</sup> Southern Ute, for example: cf. Lowie, *Shoshonean Tales*, 59.

have drawn Wolf and Coyote into their origin accounts.<sup>67</sup> The Basin, like Central California, attributes a few specific origins to theft or controversy: thus fire and pinenuts are obtained by thefts conducted by Wolf or Coyote, and death and the length of seasons by arguments between Wolf and Coyote (see above).

Several novelistic tales centering in the Basin are shared with Yuman and Californian neighbors. Probably the most popular is one wherein Wolf and Coyote overcome Bear Woman, though Coyote's rape of Bear costs him his back muscles; thereafter the brothers engage in a battle; Coyote causes Wolf's capture and death, but recovers his brother's scalp by a skinshifting ruse and restores him to life. The tale as a whole or in part is known to the Arizona Yumans (Maricopa, Havasupai, Yavapai, and Walapai), to the Serrano, Chemehuevi, Tübatulabal, Owens Valley Paiute, and Northeastern Maidu of California, and to the Kaibab Paiute, Shivwits, Moapa, and Paviotso of the Basin.<sup>68</sup> The skinshifting device is used in the same manner by the Yana, Wintu, Hupa, Coos, and their neighbors of the Southern Plateau.<sup>69</sup> Coyote and Porcupine's contention over game is known in California to Modoc, Maidu, and Owens Valley Paiute.<sup>70</sup>

The contest theme, as developed in California, must be omitted from this discussion. It appears both in California and the Basin in a number of forms, producing an intricacy of relationships that can only be disentangled by careful analysis, and which will not permit of summary characterization. The same may be said for the variety of bungling imitation tales emanating from the northern half of California and the Basin.

This revaluation of folktale material from California, cursory though it is, points out the following facts. If the material is looked at from a Californian point of view, the areal divisions and characteristics laid down by Kroeber may be retained with certain additions and qualifications. The Northwestern area is less exclusive: some of its most typical traits extend southward in a coastal strip as far as the Pomo, and are found inland as far east as the Modoc. It shares several important tales with the major

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<sup>67</sup> Spier, *Yuman Tribes*, 353; Havasupai, No. 39; Kroeber, Walapai, 247, 250; Gifford, *Northeastern and Western Yavapai*, 351.

<sup>68</sup> Spier, *Yuman Tribes*, 359; Havasupai, No. 39; Gifford, *Northeastern and Western Yavapai*, 369; Kroeber, Walapai, 255, Benedict, 12; Kroeber, *Origin Tradition*, 240; Voegelin, ms.; Sapir, *Texts of Kaibab*, 338; Lowie, *Shoshonean Tales*, 92, 161; *Northern Shoshone*, 239; Park, ms.; Dixon, *Maidu Myths*, 104.

<sup>69</sup> Sapir, *Yana Texts*, 216; Du Bois and Demetracopoulou, 314; Goddard, *Hupa Texts*, 155, St. Clair, 33; Haerberlin, 408; Boas, *Kutenai Tales*, 302.

<sup>70</sup> Curtin, 272; Dixon, *Maidu Myths*, 83; Steward, ms.

groups of North Central California: the Wintu, Yana, Shasta, Modoc, and Achomawi. The Southern area, inadequately represented save for two definitely Yuman myths, does not include the Serrano, which affiliates with the Kitanemuk and with the Tübatulabal of south central California in the possession of many typically Shoshonean myths. The now reduced Central region is indeed divided against itself; the area as a whole having little more in common in the way of major myths than the Earth-Diver Creation myth, Theft of Fire, Theft of the Sun, Growing Rock, and the Lizard Hand episode.

If, however, the material is viewed from the standpoint of areas adjacent to California, the local divisions disappear, to emerge as parts of larger wholes in a somewhat different alignment. Practically all of northern California has attributes which relate it to the myth area immediately to the north. Features which seem to be diminished phases of Northwest Coast mythology and specific likenesses to the folktales of the Sahaptin and Interior Salish (already commingled from southern Puget Sound southward through Oregon) continue into Northern California as far south as the Pomo on the coast, and the Wintu and Northern Maidu in the interior.<sup>71</sup> The Basin mythologic area makes inroads into California. In the northern half as far west as the Coast Range to include the Wintu, Yana, Achomawi and Northeastern Maidu, there is a strong admixture of Basin elements and tales.<sup>72</sup> The penetration continues southward along the backbone of the Sierra Nevada mountains, covering the Washo, the Shoshonean speaking Western and Eastern Mono (Owens Valley Paiute), and becomes the dominant factor in the mythology of the Tübatulabal, and of the Kitanemuk, Serrano, and Chemehuevi so far as it is known. The extension of Yuman tales includes primarily the Yuman tribes of extreme southern California and their immediate neighbors, the Cahuilla, Cupeño, and Luiseño.

This leaves a comparatively discrete nucleus in central California composed of the Miwok, all the Yokuts tribes, the Salinan, and probably Costanoan, Coast Miwok, and Patwin, which appears to be rather aloof

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<sup>71</sup> It is worth noting that the Pomo, Wintu, and Maidu also represent approximately the southern limit of many other Northwest Coast, Puget Sound, and Plateau elements as shown in Spier's analysis of the cultural situation surrounding the Klamath (cf. Spier, *Klamath Ethnography*, 224-325).

<sup>72</sup> The mythologic situation in northern California was anticipated by Lowie when he wrote, "The affinities [of myths] are not exclusively Basin-Californian, they must be understood to embrace the tribes of Washington and Oregon and to some extent of the Canadian Plateau as well" (*Cultural Connections*, 156).

from the surrounding regions. Although two of its major tales, the Earth-Diver creation of the world and the Orpheus myth, are well known elsewhere in North America, they do not serve as immediate links with any closely neighboring area.

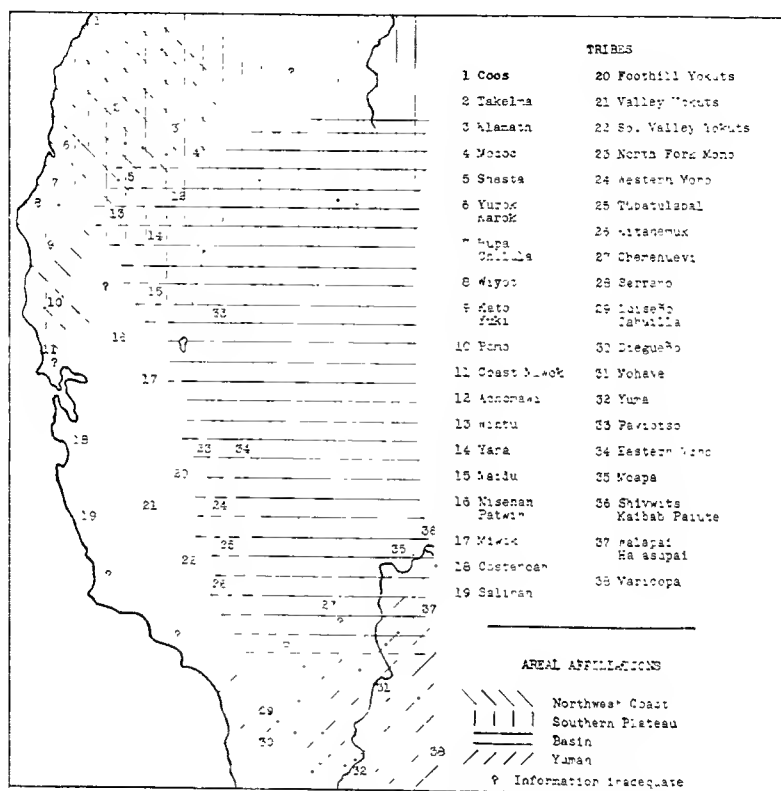


FIG. 1. Map indicating areas in California which share folktales and tale elements with adjacent regions

It must be remembered that in this attempt to envisage intra-California differences in terms of broader cultural wholes, the picture is based on the grosser and more obvious likenesses. However, it seems probable to the writer that an examination of local miscellaneous tales and elements would show cleavages along the same general lines. For example Yuki tales are more like those of the Pomo than those of the Chilula, yet both Yuki and Pomo resemble Chilula more than they do Sierra Miwok or Wintu. Tubatu-

labal has many traits closely shared with the Yokuts, but it contains a great number of Basin elements absent from the myths of these valley neighbors. Taken as a whole the mythology of certain tribes is palpably transitional: Pomo, Maidu, and Tübatulabal hold particularly interesting areal positions. Their mythologies demonstrate the impossibility of drawing sharply defined boundaries.

In any group of tales under consideration, the concepts upon which they are based extend far beyond the horizon, and as attention is turned upon plots, incidents, and elements the view narrows until only local details are in focus. Yet none of these constituents have identical distribution. As in any other phase of culture, the variable provenience of its components makes areal delimitations of mythology a matter of arbitration. The accompanying map is offered with this qualification.

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- JAFI     Journal of American Folk-Lore
- MAFLS    Memoirs of the American Folk-Lore Society
- RBAE     Annual Report of the Bureau of American Ethnology
- UCPAAE   University of California Publications in American Archaeology

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## THE COMPARATIVE LINGUISTICS OF UTO-AZTECAN

By B. L. WHORF

THE comparative linguistics of the Uto-Aztec stock is now entering upon its second stage.<sup>1</sup> It first began with the researches of Buschmann and was notably advanced by the work of Kroeber, who gave form and definition to what had before been a haze of little known tongues perceived to be somehow related. This first stage may be described as finding out what we had to deal with. One of its leading ideas was classification of the stock into sub-groups and attempts to grade degrees of linguistic kinship. Much of the phonetic material came to light during this stage, albeit not clearly expressed until Sapir's "Southern Paiute and Nahuatl" appeared in 1913, not long after which came Mason's further contributions to the subject in his "Tepecano."

If the key-word of the first stage was classification, that of the second stage has become structure. In the first stage we compared words willy-nilly, looking for resemblances to appear. On the basis of much rough and quick comparison we attempted to arrange our hodge-podge of facts into something like order. Eventually a feeling for the basic structure of Uto-Aztec speech emerged; these languages had a characteristic structure of word and stem, as Semitic and Bantu have. Sapir's work was definitely geared to the idea of structure, the first to be so. By now this typical structure is sufficiently well known to assume of necessity a central place in careful linguistic comparison. It no longer suffices merely to compare words; we must try to compare homologous structures, and to make the comparison in such a way that it will throw further light on structure. To do otherwise would be as inept as, for instance, to compare two word-lists, one in Ethiopic and one in Hebrew, without reference to the triconsonantal stem structure of Semitic and its characteristic technique.

What, then, is this characteristic Uto-Aztec structure? Its central fact is a stem of the form CVCV with a verb-like meaning, modified by suffixes, by certain changes in and apocopation of the second stem vowel, by several types of reduplication, and to a relatively small extent by pre-

<sup>1</sup> The linguistic orthography used in this paper follows "Some Orthographic Recommendations" by six American linguists (*American Anthropologist*, Vol. 36, pp. 629-31, 1934). The deviations from the older system used by Americanists are the use of *c* for *t*s, *č* for *t*c, *ɕ* for *d*z, *ʃ* for *d*ʒ, *ʕ* for *ç* (English *ch*), *ʕ* for *h*, *ʔ* for *ʔ* (glottal stop), *kʷ* for *k*w, and *ɲp*, *ɲt*, etc., for *np*, *nt*, etc., since in some Uto-Aztec languages the nasalized stops are single phonemes. It has also been necessary to use the symbols *o* for open *o*, *ɛ* for front *e* and *ɛ̃* for nasal in position of *ɛ* (*ɛ̃* means "any consonant" and *V* "any vowel," the specialized variants of these, like *õ*, are explained in the text).



fixes. Vowel-length helps to distinguish a stem. In certain languages the first consonant ( $C^1$ ) may be zero ( $C^1_0$ ), but this  $C^1_0$  is derived from a Uto-Aztecans consonant ( $ʔ$ ,  $h$ ,  $p$ ,  $w$ , or  $y$ ). Certain languages may indeed reduce any one of the four positions to zero within a limited range of cases. The position  $C^2$  may be filled by a wider range of reconstructions than  $C^1$ . A nasal preceded by homorganic stop may occur in the formula CVCV, counting as a single C, and so likewise a consonant preceded by  $h$  or  $ʔ$ . Examples of such typical stems are: Aztec *teki* 'cut', Hopi *tiki* 'cut', Tübatulabal *tīha* 'be cut up', S. Paiute *tīya-ni* 'cut meat', <UA *\*teke*; Az. *čaya*—'disperse, form latticework', Tübatulabal *ca ay(i)*—'be meshed', Ho. *ca'ya* 'sift' <UA *\*ca'ya*; Az. *eeka-* 'wind', Cora *a'ka* 'wind', Tub. *ihk(o)-* 'wind blows', Ho. *hi'ka* 'wind blows' <UA *\*he'ka*; Tüb. *ya<sup>n</sup>₂(ř)-* 'sit down'. Ho. *ya asa* 'sit' (pl.), Tarahumara *yasa-* 'sit', Mayo *yesa-* 'sit', Papago *dah-* 'arrive' <UA *\*ya<sup>n</sup>sa*.

The first vowel ( $V^1$ ) may be primarily one, two, or three moras in length (e.g., *pana*, *pa'na*, *pa'ana*), but  $V^2$  is primarily short (one mora). An ancestral connection between moras and stress-accent crops up in various languages; it is operative in Southern Paiute but more or less petrified in most others. The principle of it was that the second mora of the word received primary stress and alternate succeeding moras secondary stresses (*pana'*, *pa'na*, *pa''ana*). This pattern is overlaid and often quite superseded by the later-developed stress patterns of the various languages, but often the consequences of the older pattern are apparent in such things as the loss of one-mora vowels that were unstressed in the original scheme. Within this scheme varied patterning of the length-and-stress rhythm evidently went hand-in-hand with grammatical processes. Alternation between full stem (CVCV) and a reduced, often apocopated type of stem, as in Az. *ki'sa: o-kis*, Ho. *ya'asa: ya's-ni*, Heve *sek<sup>n</sup>a-t: sek<sup>n</sup>-te*, Tüb. *tu šu-: u-tuš*, *po'h-: o-ph-a*, is evidently an old and fundamental pattern.

As I have said, the basic meaning of the stem is typically that of a verb, i.e., denoting a class of event rather than a class of thing; and from this meaning nouns, names of things or qualities, are derived in various ways, as exemplified by Az. *te'si* 'grind': *tešxi* 'meal', Ho. *ʔimi* 'detonate': *ʔimikpi* 'gun', Tüb. *yihpa* 'shut': *yihpal* 'door'. This fundamental situation is not altered by the fact that we may often find a given stem represented only by its noun-derivative, which may be derived by a zero-element, as in the case of Ho. *ta ala* 'shine': *ta'ala* 'day'. Since then the noun-vocabulary of each language, barring a certain common substratum to be spoken of, is elaborated independently from the language's own stock of verb stems, the problem of comparative linguistics in Uto-Aztecans is to compare verb stems

and the processes of using them, including those of deriving from them. Uto-Aztec comparison is not a question of comparing nouns directly, except in the case of the vocabulary of nouns possessed by the ancestral speech before its separation, as fragmentarily preserved by the daughter languages. Fortunately (because the comparison of nouns involves fewer special problems than that of verbs) this common stock of nouns is fairly well preserved. It includes most animal, plant, anatomical, and kinship names, many nouns of topography and the world of nature, and a few nouns of material culture. These archaic nouns are likewise of the form CVCV or else evidently derived in the usual way from such stems, with the exception of some which are monosyllabic. For along with the main stock of CVCV stems, the Uto-Aztec languages contain another set of stems, much fewer in number but extremely common, of monosyllabic form CV. Some of these are verbs, some nouns like \**ma* 'hand', \**pa* 'water', \**ye* 'mother', \**hu* 'arrow'. Although at first sight one might suppose these must be out-and-out nouns, names of things, yet many of them appear on closer examination to be merely derivative meanings, become extremely common, of terms that primarily denote classes of events or of relationships, and are on the whole verb-like. Thus in both Aztec and Tubatulabal the stem *ma* is a verb (Az. *ma* 'catch', Tub. *ma*<sup>2</sup> 'touch') and only with nominal affixes does it become, e.g., Az. *i ma* 'his hand', Tub. *ma'n* 'his hand'.

CV-stems may be reduplicated, and then may yield CVCV-stems, like Az. *mama* 'carry'. With CVCV-stems, reduplication of several types was a regular grammatical process in the prototype speech, and remains so in many of the present-day languages. Initial reduplication as in Az. *koto-na* : *kokoto-ca*, *k'eyo-ni* : *k'ek'eyo-ka*; final reduplication as in Ho. *ro'ya* : *roya ya-ta*, *kala* : *kala'la-yki* (and petrified in stems like S.P. *sororoi*-, Tub. *cibibi*<sup>2</sup>-) are the best preserved types; but the type which consists of prefixing V<sup>1</sup>, as in Tub. *pa'abi* : *aba abi*, *hi p* : *ihi'b* is also archaic, though aside from its extensive employment in that language it shows up only by traces.

CVCV-stems are also capable of having certain changes made in V<sup>2</sup>, for grammatical reasons, as in, e.g., Ho. *so'ma* 'tie', *so mi* 'be tied'. The vowels *a* and *i* are the commonest in this position and are regularly associated with transitive and with passive or resultative ideas respectively. There are also certain much rarer and more obscure changes of the first vowel.

This completes the essential simple picture of the Uto-Aztec stem and the changes that may be rung upon it. The matter of its structure goes far deeper, however. On the phonetic side, we have to take cognizance in comparative work of various sorts of phenomena that may alter the consonants of a stem, largely because of the position of these consonants with respect

to vowels, but also involving other reasons and presenting problems still unsolved. Chief of such phenomena is spirantization of stops. Spirantization may be an active process, as it is with all stops in Southern Paiute, or it may be inactive, "petrified," the mere reflex of an active process in an earlier stage of the language. Only spirantization of *p* occurred in the prototype, but the process seems to have spread to other consonants to varying extents in many of the daughter languages, whose spirantization phenomena are thus largely parallel developments. In a language where spirantization is active, a *C*<sup>1</sup> which is a stop when word-initial regularly becomes a continuant under circumstances which place an "ordinary vowel" (to be explained later) before it. Thus S P. *pa'γiu-* 'fish', *tu''pi-* 'stone', *kani-* 'house', when second members of a compound ending in an "ordinary vowel" become *-va'γiu-*, *-ru''pi-*, *-γani-*. "Inactive" spirantization in a language is not taken account of in the grammar of the language, but only in comparative linguistics. It appears for instance when the \**C*<sup>1</sup> of the prototype gives two reflexes in two different paradigms of the descendant speech; one the reflex of a stop, the other (which may be zero) the reflex of a spirantization. Thus Tub. *kr' /-* 'bite' is the reflex of UA \**ke* 'bite', but Tub. *hali-* 'sit' is no less the reflex of UA \**kate* 'sit'; Az. *pa ti* 'melt' is from UA \**pa-* 'water,' but so are Az. *a ʔ* 'water' and *a tiya* 'melt'; Cora *pi* 'hold' is from UA \**pi* and also Cora *hisi* 'eye' is from UA \**pu'si* (cf. Tub. *pu'zi-* 'eye'); Tarahumara *toli* 'hen' is from UA \**tori turi*, as shown by Az. *totolin*, Papago *čuculi*, while Tar. *rala* (or *dala*) 'foot' is, via spirantization, from UA \**taʔa*. There is never any law or regularity about the choice of result, and one can only conclude that with some words it was a nexus-form or a form with a vocalic prefix, later lost, that handed the stem down to posterity, who rebuilt the paradigm upon this form; while in other cases, chosen by historical accident, it was the initial-form which survived.

When we come to consider *C*<sup>2</sup>, we find that it shows only "inactive" spirantization, and that in a partial way only. In Southern Paiute for instance the majority of stems have *C*<sup>2</sup> given in spirantized form, as in *oyo-* 'pine-tree' < UA \**woko*. But there are other cases, like *tika-* 'eat',<sup>2</sup> where *C*<sup>2</sup> may be unspirantized. The distinction between the two treatments of a given stop in *this* position is one upon which the languages as a rule agree; it is a distinction which goes back to the prototype speech. A kindred fact to this is that in the field of "active" spirantization, sometimes such spirantization fails to occur where we might expect it, that is after a nexus-vowel. Hence we can divide final vowels of words into two classes, "ordinary" or

<sup>2</sup> Written in phonemic form; phonetically *tuq a-*

spirantizing and those which exert an "antispirantizing" influence. If we were to make a Southern Paiute compound, "flower-house," with the stem *ʃiʔi-* 'flower', it would not be *ʃiʔiʔani-* but *ʃiʔikani-*. This attribute of anti-spirantizing power is also the reflex of something in Uto-Aztecan. In Hopi, spirantization is active only with *p* ( $p > v$ ). If we were to make such a compound as "flower-liquid" from *si-* 'flower' and *pa'ala* 'liquid', we should get not *si va ala* but *si hpa ala*. The Hopi "flower" word then has a similar anti-spirantizing power to that of its cognate in Southern Paiute. We may even get the reflex of an old anti-spirantizer in languages where spirantization is entirely "petrified." In Aztec the word *a'yauiʃ* 'mist' begins with the UA stem *\*pa-* 'water' in spirantized form. There is an Aztec word for 'snow' which is an archaic compound of some such word and the word *seʃ* (stem *se-*) 'ice, freezing' (compare S. P. *ʃi-pi-* 'cold'). This word is *sepa'yauiʃ*, showing that this compound dates back to a time when *se-* was able to exert its anti-spirantizing power on the initial *p* of the second component. Some languages, like Southern Paiute, show the anti-spirantizing to be of two kinds, one which nasalizes and one which does not. We can see in Hopi that the Uto-Aztecan stem *\*te-* 'stone' was anti-spirantizing, from compounds like *tʰpa la* 'stone cliff', but Southern Paiute shows us also that it was nasalizing, in the suffix-bearing *tʰpi-* 'stone' and the compound *tʰkani-* 'stone house', while in Tub. *tʰl* 'stone', the nasal also appears. Again, an unspirantized *C'* may be also nasalized, as in S. P. *puʔku-* 'pet', Tub. *puʔgul* 'pet', but Ho. *po hko* 'pet, dog'. We may sum up a great deal of evidence of this sort in the statement that ancestral Uto-Aztecan did not have anti-spirantizing as such (since except with *p* it did not have spirantizing as such) but had stems and suffixes of a type *CVC<sub>n</sub>*, where *C<sub>n</sub>* belongs to a limited class that includes nasals, and that these final and unknown consonants disappeared but left their reflexes in anti-spirantizing and nasalizing powers attendant upon certain final vowels in daughter languages; also that its stems of the broad type *CVCV* included the subtypes *CVCVC<sub>n</sub>*, *CVC<sub>n</sub>CV*, and *CVC.CVC.*, and that the clusters in the *C'* position gave rise to the unspirantized and nasalized consonants found in daughter languages in this position. We may denote the spirant-proof group *C<sub>n</sub>C* also as *\*C*, thus S. P. *tika* < UA *\*teʔka*, and we may denote anti-spirantizing and nasalizing powers by such formulas as *\*te<* and *\*te<sup>n</sup>*.

On the semantic side of comparative work we have to analyze carefully the ostensible meaning-content of the *CVCV* stems as it is presented to us in the descendant languages. A word in ancestral Uto-Aztecan for (finger-)nail was *\*sutu*, which has yielded by regular processes S. P. *ʃiʔu*, Cora *ʃiti*, *ʃile*, Papago *huʔi*, Az. *iste-ʃ*, etc. However, in Hopi the word for nail

is *so ki*, which can derive from nothing but a UA *\*suki*, for there are no indications of or analogies for anything like *\*sut-ki* as a parent. This pairing of two CVCV-stems *\*sutu* and *\*suki* with the same or approximately same meaning is no mere "happenstance," for examples of similar pairs (and groups) of CVCV-stems could be multiplied almost endlessly. Let me cite only one more. Az. *i* 'drink' and Tub. *i* 'drink' are shown by Hopi to derive from UA *\*hi*, but the Hopi word is *hi ko* 'drink' < UA *\*hiku*, and the Southern Paiute is *ivi* 'drink' < UA *\*hivi*; thus we find in UA at least three distinct but obviously related 'drink' stems, *\*hi*, *\*hiku*, *\*hivi*.

The analogies of compound-formation may help us here. All Uto-Aztec languages are fond of making compounds of all kinds of stems, noun or verb. Besides the ordinary creative compounding we have traditional idiomatic compound-types handed down all the way from the ancestral speech. For instance, throughout the daughter tongues, the verb 'die' (usually < UA *\*mu<sup>z</sup>ki*, but in S.P. *ya'ai-*) is used as second member of a compound with an entirely different meaning, that of feeling a sensation or being in a certain bodily state: Ho *pa nak-mo ki* 'be thirsty', Tüb. *yahta-mu g-* 'be asleep', Az. *šoko-mi ki* 'be drunk', S.P. *šipi-ya'ai-* 'feel cold', etc. The Uto-Aztec ancestry of the form is well shown by Az. *temi ki*, Ho. *timo oki*, Tub. *tumu ug-*, all 'dream', all < UA *\*temu<sup>z</sup>ki*. If the word occurred in Southern Paiute it might be *t̪iŋ<sup>z</sup>iki-*, more probably *t̪ya'ai-*. The element *\*te-* in our reconstruction can be defined no further than as that which when joined with the requisite sort of "determinative," *\*mu<sup>z</sup>ki, ya'ai-*, or the like, means 'dream'. So also in considering *\*sutu* and *\*suki*, *\*su-* can only be defined as that which with a proper determinative means 'nail', and the elements *-tu* and *-ki* answer to *\*mu<sup>z</sup>ki* and *ya'ai-*. A Southern Paiute form in *-ya'ai-*, with first member cognate to that of a Hopi form in *-mo oki*, is not a complete cognate of the Hopi form; it is a "translation cognate." So Hopi *so ki* < *\*su-ki* and S.P. *šiču-* < *\*su-tu* are probably translation cognates.

Now so many of the CVCV-stems upon detailed investigation break up in the manner of *\*sutu* and *\*suki* into ranks of determinatives, variously cross-combined, that comparative study in Uto-Aztec that gets beyond the infancy stage must work a great deal with translation cognates. Unlike *\*sutu* *\*suki* their meanings are mostly verb ideas, and they throw a most interesting light semantically, as the spirantizations and allied phenomena do phonetically, upon the structure of the CVCV-stems and the reason for the reflexes that are found in the descendant languages. The theory that is suggested by the phonetic forms of the very numerous dissyllabic stems, CVCV, CVC<sub>n</sub>CV, CVCVC<sub>n</sub>, CVC<sub>n</sub>CVC<sub>n</sub> in relation to the forms CV, CVC<sub>n</sub>

of the comparatively few monosyllabic stems, namely that the longer stems are complexes of units which occur simple or at least as simpler complexes in the short stems, is borne out by semantic analysis. Yet it is putting it rather too simply to say that the CVCV stems are compound; their inner construction is more closely fused and unified than that term implies: "synthetic" might be better.

The primary phonemes of the prototype speech may be reconstructed as: vowel-series, *a* (*a*, *a*<sup>h</sup>), *e*, *i*, *o*, *u*; stop-series *p*, *t*, *c*, *k*, *k*<sup>h</sup>, nasal-series *m*, *η*, *n*, <sup>h</sup>*s*, *η*, others *w*, *s*, *l*, *r*, *y*, *ɣ*, *h*. The vowel <sup>h</sup>*a* shows two reflexes in several languages, but they probably represent long and short *a* respectively. Varied reflexes of other vowels may also be a function of length, but this has not yet been worked out. Possibly *ā* should be added to the list. The secondary phonemes and morphophonemes include two shown above: *r* the spirantized form of *p*, and *η*<sup>h</sup>, related in some similar but not quite the same way to *m*. They further consist of the spirant-proof unnasalized set <sup>h</sup>*p*, <sup>h</sup>*t*, <sup>h</sup>*c*, <sup>h</sup>*k*, <sup>h</sup>*k*, and the nasalized set <sup>h</sup>*p*, <sup>h</sup>*t*, <sup>h</sup>*c*, <sup>h</sup>*k*, <sup>h</sup>*k*<sup>h</sup>, secondary forms of *l*, *r* (*l*, *r*<sup>h</sup>), perhaps of *n*, *w* also, and vowel-length. This statement of secondary phonemes indicates that C<sub>n</sub>C gives a separate reflex from C only when C belongs to the stop-series—though more minute investigation may well change this dictum. The special consonants in the class C<sub>n</sub> include undifferentiated nasal <sup>h</sup>*n*, and perhaps *l*, *r*, possibly no more.

And now for a list that will show the reflexes of this ancestral phonemic system in the daughter languages. In times past some of us hoped that the stock could be classified in such a way that we could summarize the situation by stating generalized reflexes for sub-groups such as "Shoshonean," "Piman," "Sonoran," from which the reflexes of the individual tongues in these groups could be derived as a second step. This hope is vain. No such groups exist. The nearest thing to one is Piman, but it is unsafe to generalize even about so small and compact a group of dialects as Piman. Hence, since any generalized table of reflexes would convey a misleading impression, and since it is out of the question to treat a large number of languages in an article of this sort, I shall list the reflexes in eight fairly representative languages: Tubatulabal (T.), Southern Paiute (S.P.), Hopi (H.), Aztec (A.), Cora (C.), Tarahumara (Ta.), Tepecano (Te.), Papago (P.). As a rule each Uto-Aztecan primary phoneme gives several reflexes in a daughter language. Some of these represent secondary developments according to the phonological laws of the daughter language, e.g., the "active" spirantization of Southern Paiute. Some of those that I am obliged to quote are more apparent than real, due to imperfectly phonemic, phonetically overexact recording. Some represent the secondary phonemes and

C<sub>n</sub>C structures of the prototype. I shall attempt only to give the commonest reflexes in each language, the most common first, and will place in square brackets those reflexes whose variation from the unbracketed forms is governed by at least approximately understood laws.

1. \*a > T. a, S.P. a, H. a, A. a, C. a, [ua], Ta. a, Te. a, P. a.
2. \*a > T. a, S.P. a, H. a, [a], A. e, a, C. a, Ta. a, Te. a, P. a.
3. \*e > T. i, S.P. i, H. i, A. e, C. e, i, Ta. e, Te. i, P. i.
4. \*i > T. i, S.P. i, H. i, A. i, e, C. i, Ta. i, Te. i, P. i.
5. \*o > T. o, S.P. o, H. o, A. o, C. u, Ta. o, Te. u, o, P. u, o.
6. \*u > T. u, S.P. u, o, i, H. o, A. i, e, C. i, ue, i, Ta. u, [o], Te. u, P. u.
7. \*p(not \*v) > T. p, [b], S.P. p, [v], H. p, [v], A. p, [zero], C. p, [h or zero], Ta. β, [p], Te. v, [p], P. v, [p], [w].
8. \*v > T. b, S.P. v, H. v, A. w, zero, C. w, [v], zero, Ta. β, Te. v, P. v, [w].
9. \*t > T. t, [d], [l], S.P. t, [r], [c], [č], H. t, A. t, [χ], [č], C. t, Ta. r or d, [t], Te. t, P. t, [č].
10. \*c > T. c, [s], S.P. c, [č], H. c, A. c, [č], C. c, Ta. č, Te. s, [š], P. s, [š].
11. \*k > H. k, all others, same as for \*k.
12. \*k > T. k, [g], [h], S.P. k, [γ], H. k, A. k, C. k, [č], Ta. g, [k], Te. k, P. k.
13. \*k<sup>w</sup> > T. w, [k], S.P. k<sup>w</sup>, [γ<sup>w</sup>], H. k<sup>w</sup>, A. k<sup>w</sup>, C. k<sup>w</sup>, [č<sup>w</sup>], Ta. w, Te. b, P. b.
14. \*m(including \*η<sup>h</sup>) > T. m, [η<sup>w</sup>], [η], S.P. m, [η<sup>w</sup>], H. m, [η<sup>w</sup>], [η], A. m, [n], [η], C. m, Ta. m, Te. m, P. m.
15. \*n > T. n, S.P. n, [zero], H. n, A. n, [η], C. n, Ta. n, Te. n, P. n.
16. \*<sup>n</sup>s > T. <sup>n</sup>s, S.P. <sup>o</sup>, H. s, A. s, C. s, Ta. s, Te. h, [r], P. h, [r].
17. \*η > T. n, [η], S.P. n, [zero], H. η, A. n, C. n, Ta. n, Te. n, P. n.
18. \*w(except for initial \*wo-) > T. w, S.P. w, [η<sup>w</sup>], H. w, [l], A. w, C. w, [v], Ta. w, Te. g, P. g.
19. initial \*wo- > T. wó-, S.P. o-, H. lo-, A. o-, C. hu-, Ta. o-, Te. go-, P. go-.
20. \*s > T. s, S.P. s, [š], H. s, A. s, [š], C. s, š, Ta. s, Te. h, [r], P. h, [r].
21. \*l initial > T. l, S.P. n, H. l, A. n, C. n, Ta. n, Te. n, P. n.
22. \*l non-initial > T. l, [n], S.P. n, H. l, [n], A. l, C. r, Ta. l, Te. r, P. t, [l].
23. \*r initial > T. l, S.P. n, H. r, A. n, C. n, Ta. n, Te. n, P. n.
24. \*r non-initial > T. l, S.P. r, H. r, A. l, C. r, Ta. l, [n], Te. r, P. t, [l].
25. \*y > T. y, S.P. y, H. y, A. y, C. r, y, Ta. y, zero, Te. d, P. d, [š].
26. \*<sup>o</sup> > T. <sup>o</sup>, [zero], S.P. <sup>o</sup>, [zero], H. <sup>o</sup>, A. zero, C. zero, h, Ta. h, zero, Te. zero, P. zero.
27. \*h > T. zero, <sup>o</sup>, S.P. zero, <sup>o</sup>, H. h, A. zero, C. h, zero, Ta. zero, Te. zero, P. zero.

What of classification of the stock into sub-groups? Only that the results of comparative linguistics since Kroeber's first classification of the

stock have tended not so much to suggest improvements in the classification as to minimize the significance of such sub-groups. Division-lines have more and more faded away. The Piman group remains distinct, but Piman should probably be considered a single language with marked dialects. Terms like "Sonoran" will always be convenient and will probably remain current as designating a geographical area within the stock, but the group of languages called "Sonoran" has no linguistic unity. Taking even the largest geographical division, Shoshonean, does it mean anything linguistically? Are there any traits that distinguish "Shoshonean" from the rest of Uto-Aztecan? If there are, I do not know what they are. Spirantization is not one. Reflex of  $C_nC$  structures that prevent spirantization is not one. A vowel scheme with  $i$  for  $e$  is not one; some southern Californian languages have  $e$ . It is not any different vocabulary or different way of doing things morphologically and grammatically.

There are of course certain well-defined small groups, e.g., Piman (Pima, Papago, Tepecano, Tepehuane), Shoshoni-Comanche, Ute-Chemehuevi (Ute, Southern Paiute, Chemehuevi, Kawaiisu), and Cora-Huichol. It is possible that another large-scale grouping may emerge when we know more.

The position of Uto-Aztecan as a specialized branch of a much larger family is a promising field for research. Its resemblances to Penutian on the one hand and to Mayan on the other, and of Mayan to Penutian, are to my mind so striking and so deep-seated as to require grouping them all into one large family, which would also include Kiowa and its relatives and Totonac of eastern Mexico.

WETHERSFIELD, CONN.



A NAVAHO SAND  
PAINTING BLANKET

By EDWARD SAPIR

AS is well known, the Navaho Indians have of late years taken to the weaving of sand painting blankets, that is to say blankets in which the usual geometrical designs are replaced by more or less faithful copies of sand paintings belonging to the great curing ceremonies known as "chants," such as the Night Chant, the Mountain Chant, and the Shooting Chant. As the actual sand paintings of the rituals must be destroyed before nightfall of the day on which they are laid down in the ceremonial hogan and as, further, it is forbidden for the "chanter" to keep a permanent record of the sand paintings which are part of his curing ritual, these sand painting blankets are, by definition, blasphemous—doubly so, indeed, for to the wrong of preserving what should be a transitory moment of holiness is added that of an illegitimate transfer of the picturing of an episode in a ritualistic origin legend from a sacred context to a mundane article of sale. The older Navaho are said to be very much opposed to these blankets but the demand of the white man appears to be more powerful than religious sentiment.

The weaver has a simple expedient for warding off the curse which follows a tampering with holy things. By deliberately changing the sand painting design here and there she feels that she absolves herself from the charge of blasphemy. The blanket decoration looks like a genuine sand painting to the white man but to the gods and instructed Navaho the departures from ritualistic accuracy put the woven blanket into the class of profane objects. No curse need follow the weaving—at least, so it is hoped.

The blanket figured in this paper<sup>1</sup> was purchased by the writer in the summer of 1929 at Crystal, New Mexico, one of the less frequented trading posts on the Navaho reservation. It was made by Manuel Denetsone's wife, according to the trader, Mr Charles Newcomb. A Navaho, Albert Sandoval, who was then interpreting for the writer, very kindly went over the details of the design with him and pointed out what he considered to be its "inaccuracies." He was naturally unable to say which departures were intentional, which due to a faulty memory. The latter possibility is by no means to be excluded, for a weaver, like any other non-chanter, would only have sporadic opportunities for seeing any particular sand painting and might readily have failed to note minor details in the composition. It is obvious,

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<sup>1</sup> My thanks are due Mrs C. S. Ford for the drawing of the blanket design and to her and Dr C. S. Ford for calling my attention to a number of details in the design, to Father Berard Haile for one or two points of terminology.

however, that certain gross "errors" in this blanket were intentional and it seemed to be Albert Sandoval's feeling that few of them were due to mere ignorance. His comments are given for the light they throw on an interesting phase of Navaho psychology.

According to Albert Sandoval, the sand painting woven into this blanket belongs to the Male Version of the so-called Shooting Chant (na'at'oe: biká'ží).<sup>2</sup> A somewhat similar, but by no means equivalent, sand painting was published by Natalie Curtis (Burlin) in "The Indians' Book," plate opposite page 366 (edition of 1923), and is referred by her to the Mountain Chant.<sup>3</sup> The four sacred figures represent, from left to right, the Holy Young Man (diné'h diyini'), the Holy Young Woman (é'iké'h diyini'), the Holy Boy (kiyé h diyini'), and the Holy Girl ('at'é'd diyini'). The body of the Holy Young Man is all black and is covered with white zigzags representing forked lightning ('acinlǎ'is). These lightning lines should not only run down towards the feet but also up to the wrists [1].<sup>4</sup> The feet are missing [2]; they should be accompanied by "black-edged moccasins" (ké bā'h našží·d) [3].<sup>5</sup> The anklets, right-angular attachments to the missing feet, represent lightning. They should be black, not red and blue [4], and they should have four corners, not three [5]. The two bars, blue and red, at the knees should also appear at the ankles [6] and at the wrists [7]. The placing of the bars—whether blue-red or red-blue in the direction away from the face—is of importance. In the former case the chant to which the sand painting is attached is a "blessing-way chant" (hóžó'ží hatá'l); in the latter, a "fighting-way chant" (de·zlá' hatá'l). The "fighting-way chant" is ap-

<sup>2</sup> Phonetic note. The orthography employed in this paper is strictly phonemic. The characters *c* and *č* represent *ts* (of English *hats*) and "*tc*" (of English *chew*) respectively; *š* is "*c*" (of English *shoe*), *ž* is "*j*" (of French *je*), *z* and *ž* are "*dz*" and "*dj*" (of English *adze* and *judge*, approximately); *χ* is *h* (lateral affricative); *γ* is velar voiced spirant before *a*, prepalatal voiced spirant before *e* and *i* ("rubbed" *y*); *i'*, *k'*, *c'*, *č'*, *χ'* are glottalized stops and affricatives. Syllables with acute accent (e g *á*) are high-toned; unmarked syllables (e.g., *a = á*) are low-toned.

<sup>3</sup> I owe this reference to Miss Eva M. Horner, formerly a graduate student in anthropology at the University of Chicago.

<sup>4</sup> The numbers in brackets represent "errors." Mrs Burlin's sand painting shows the lightning lines from the knees to the feet and from the elbows to the wrists in figures 1, 2, and 4, and from the knees to the feet alone in figure 3.

<sup>5</sup> In Mrs Burlin's sand painting the feet are shown and underneath them are bars, bordered in black and red, which probably represent moccasins. In the song (p. 368) which the chanters sing "while dressing the runners to represent the Divine Ones" pictured in her sand painting occur the lines:

Moccasins decked with black,

Thereof he telleth.

The Navaho text has "ke-pa-nashjini" for the first line

propriate for a patient who is bit by a rattlesnake, struck by lightning, or hurt by water. The present sand painting obviously goes with a "blessing-way chant."

The skirt, skirt fringes, and belt fringes can be decorated to suit the individual fancy. The arm fringes, or pendants, must be yellow and brown, not yellow and black [8],<sup>6</sup> as in the blanket. The lower tips of these fringes should be decorated in black and red, not blue [9] and red, with a white strip on each side. The neck should always have a ground of blue and four red stripes, not three [10], as in the figure of the Holy Young Man of our blanket; the other figures are correctly represented with four red stripes at the neck. The order of the blue and red stripes does not matter.<sup>7</sup> The horizontal yellow bar at the bottom of the face represents the evening twilight (*naho'coi*). Instead of the inner red border at the top of the face there should be a bar of white [11], representing the dawn (*hayo'lká'í*). Above the yellow bar there might be a parallel one of blue, representing the sky blue (*nahode'ł'í'ž*), and across the eyes another horizontal bar of black, representing the night or darkness (*čahałxe'í*); but these middle bars are generally omitted. The white, blue, yellow, and black, if all present, are an obvious replica of the colors of the four cardinal points (east, south, west, and north).<sup>8</sup> The face-color is always brown, here represented by the ground color of the weaving, and is laid down before the other colors are put on. The ear-pendants (*ža'ł'ó'í*) are of red and blue, representing turquoise and red shell respectively, and the white line in the blue represents the string which attaches the pendants to the lower face-corners. The vertical red

<sup>6</sup> It seems reasonable to suppose that the black is a purely technical substitute for brown, as the ground color of the blanket is a brown, against which a decorative brown would not stand out.

<sup>7</sup> This discrepancy, as Dr and Mrs Ford point out, is probably due to a technical slip in the weaving. "Figure 2," they remark, "has four red stripes on the neck, as is quite plain from the blanket, although in the picture the red band melts in with the red color of the dress. If the blanket was woven from right to left, the following is a likely explanation of the differences in the neck-band representations

"Figure 4 is correctly represented with four red bands on a background of blue. Figure 3 is also correctly represented with four red bands on a background of blue, but the order is different. In figure 3 the blue band appears first immediately beneath the yellow border of the face. In figure 2 this blue band was kept in the same position as in figure 3. This necessitated, although four red bands were meant, the running of the red into the red of the dress. It thus appeared to the weaver that there were only three red bands in figure 2, a representation which was continued in figure 1."

<sup>8</sup> See "An Ethnologic Dictionary of the Navaho Language" of The Franciscan Fathers, p. 40, for the four colored lights: "first light and early dawn," "azure, the deep sky-blue at dawn or sunset," "the yellow light at sunset," and "darkness, dark light of the north."

bars on the side of the face—the connecting one should be white, as we have seen—represent a face paint of red ochre (*čí h*). All around the head (presumably only the sides and top are meant) is a border of black, not blue [12], representing the hair, and about this in turn is a concentric border of red, representing horse-hair (*hí'ya'*). The two parallel right-angled strips of white erected on the head represent string for the tying of soft breast feathers (*'ac'os*) to the hair. These feathers are represented by the heavier white bar. Under this bar is a slender line of black and spots of red and yellow (in figure 2 the red spot is missing), representing a turkey tail (*tąži'ce'*). The color scheme of this turkey tail is incorrect [13]. It should be red, blue, yellow—starting from the string—followed by the flange of white at the tip.

The second figure, the Holy Young Woman, should be corrected in a number of features as for the Holy Young Man [2, 3, 6-13]. She wears a dress of various colors—red, yellow, blue, white, and black. The order of these colored triangles does not matter, the ornamentation being purely decorative, not ritualistic, as is also the case with the skirt and fringes. Albert Sandoval considered the use of the yellow, blue, white, and black correct, but was not convinced that the red was allowable [14?], for red is the color of blood and should not be used except where ritually prescribed. If, therefore, the blanket weaver has here been in error, it is probably through ignorance rather than intent. Both the legs and the arms [15] should have a straight line of black [16], to represent the flash lightning (*haco'olyał*). The curved attachments to the feet represent the rainbow, which should have a strip of white [17] between the red and the blue (see below).

The third figure, the Holy Boy, is an exact replica of the first, aside from the four red bars of his neck, which is not a relevant contrasting feature, the reversed order of the red and blue lines inside the skirt, an immaterial ornamental variation, and the object that he should be holding in his right hand (see below). Needed corrections should be made as for the Holy Young Man [1-9, 11-13].

The fourth figure, the Holy Girl, is an exact replica of the second, aside from the different color scheme of her dress (blue, white, green, gray, red, and black, which is a purely decorative, not ritualistic, contrast, the presence of the red spot in the turkey feather and of blue-red bands on the wrists, the difference of order in the red neck-bands, and the object that she should be holding in her right hand (see below). Needed corrections should be made as for the Holy Young Woman [2, 3, 6-13, 14?, 15-17].

The circular objects of blue and black-spotted white which the divine

figures hold in their hands are all incorrect, being merely decorative substitutes for the correct objects, which quintessentially define the holy beings. These missing objects were evidently felt to be too sacred to be represented in a blanket. Albert Sandoval was not sure what these substituted symbols were meant to indicate but thought they might be baskets. In that case the three black spots might mean cornmeal or an offering of pollen. The

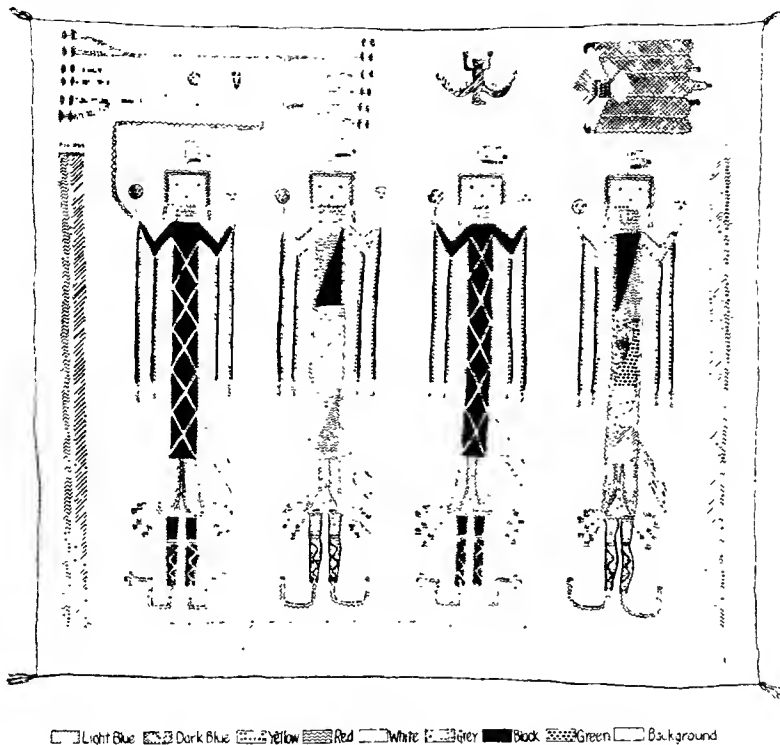


FIG. 1. Color scheme of a Navaho sand painting blanket (52½×56 inches).

Holy Young Man should hold a black bow ('altin dilxil) in his left hand [18] and a ceremonial object known as "feathered with an eagle tail-feather" ('ace' best'an) in his right [19]. The Holy Young Woman should hold a yellow bow known as 'altin celkani ("the bow of celkani," a wood doubtfully identified as "yellow mahogany") in her left hand [20] and a "feathered cane" (gisist'an=gis yist'an) in her right [21]. The Holy Boy, like the Holy Young Man, should hold a black bow in his left hand [18] but, by contrast, has a "yellow-feathered arrow" (ce' k'isdi'coi) in his right

[22]. The Holy Girl has the yellow bow of the Holy Young Woman in her left hand [20] but in her right hand she holds a "red-feathered arrow" (t'ałč'i' k'a') [23].

The border of blue and red which surrounds the four holy beings on three sides is called "a string" (a'X'ó l) and, according to the sand painting, may be interpreted as rainbow, lightning, or smoke. In this sand painting the border must be taken as the rainbow (na'c'í'lid), which goes with the represented figures, for, according to the well-known Navaho belief, the supernaturals travel through space with the flinging of the rainbow. It is colored like the rainbow-anklets of the Holy Young Woman and the Holy Girl, which are in error, however, in not having the dividing strip of white [17] which is properly shown in the rainbow border. The surrounding white of the rainbow is correct. The disconnected white bars at the ends of the rainbow, however, are merely a substitute for the head [24] and tail [25], which are respectively at the left and right ends, sunwise movement.

The figure at the upper right is the bat (ž'a'abaní), the door-guard. His representation is correct so far as it goes, but Albert Sandoval thought there was something missing [26?]. The bluebird (dóli) to the left of the bat does not belong to this sand painting at all [27].<sup>9</sup> The tentacled object at the upper left represents a tobacco pouch (ná't'ohzis). The forked lines of red, blue, and white attached to the right side of the pouch should both curve in (lower right to upper left) like goat horns [28] and should fork into three or five white lines, not two [29]. The number of middle white lines too should be three or five, not two [30]. All these corrections for the pouch strings on the right apply to those on the left except, of course, for the counter-curve of the "goat horns." At the end of each of the white lines the crescents of red and black, which are deer hoofs, should be three or five in number, not two [31]. Along the upper and lower sides of the pouch, presumably on the outside, there should be a white zigzag line for forked lightning [32]. The blue circle on the pouch represents the sun. The blue triangle represents a pipe, which is supposed to be lit by the sun. It should have a white line at the base to indicate ash [33]. The long red and blue line attached to the lower side of the pouch represents the pouch string. It should not connect with the Holy Young Man [34] but should merely run out a little from the pouch. Bat and tobacco pouch always go together in the sand paintings.

Albert Sandoval remarked that the figures of a sand painting must never touch each other. If they do, it is a sign that the people are going to have a

<sup>9</sup> Mrs Burlin's sand painting has no blue bird at the top but only two bats facing each other, corresponding to the bat and tobacco pouch of our sand painting except that the two bats are centered between the second and third figures.

light. Such a sand painting as this would take eight men working together about three hours to lay down. They start from the center and work outward.

Unfortunately we are not in a position to state categorically what is the nature of these thirty-four (and possibly other) "errors." It seems fair to assume that not all of them are intentional departures from the ritualistic norm but that some of them are due to:

1. Unfamiliarity with the minor details of the sand painting [such, perhaps, as nos. 4, 5-7, 9, 11-14, 16, 17, 27, 33, 34]. It seems difficult to believe, however, that the insertion of the bluebird [27] is a mere transfer from other sand paintings in which it is in place. Can it be that the weaver inserted the delightful, happiness-bringing bluebird as a personal good-luck token to take away the curse of handling a ritualistic design for non-ritualistic purposes?

2. Technical difficulties [e.g., no. 8, already commented on] or oversights [e.g., no. 10]. To produce a set of goat-horn curves [no. 28] in a medium such as a woven blanket would probably have demanded more care than the weaver thought worth while.

3. "Undercutting," the well-known Navaho trick of always leaving out something, however trivial, in a communication involving holy things, such as telling an origin legend, teaching the novice a chant, copying a sand painting [such, perhaps, as nos. 3, 26, 29-32].

The last, generalized, motive leads insensibly to the specific fear of copying peculiarly holy symbols without ritualistic justification. The omissions and substitutions that come under this head can hardly be regarded as other than intentional. They comprise in this blanket, with nos. 1, 2, and 15 still doubtful:

[1, 15]: lightning up to wrist of holy beings omitted, presumably because of fear of lightning in their hands;

[2, 15]: lightning interrupted at lower end by absence of feet and moccasins, perhaps to disconnect representations of holy beings from attached lightning and rainbow symbols;

[18, 19]: substitution of "baskets" for black bow and ceremonial object with eagle tail-feather in hands of Holy Young Man, to avoid significant identification of figure in blanket with figure in actual sand painting;

[20, 21]: substitution of "baskets" for yellow bow and feathered cane in hands of Holy Young Woman, to avoid significant identification;

[18, 22]: substitution of "baskets" for black bow and yellow-feathered arrow in hands of Holy Boy, to avoid significant identification;

[20, 23]: substitution of "baskets" for yellow bow and red-feathered arrow in hands of Holy Girl, to avoid significant identification;

[24, 25]: substitution of white bars, a purely decorative feature, for head and tail of rainbow, presumably to avoid significant animation of rainbow.

In brief, the holy beings, essentially characterized by continuous lightning from hand to foot, by the swift animated rainbow which is their holy symbol of progress, and, above all, by the symbolic bows and feathered objects which they hold, are deprived of their ritualistic reality. There is no true animation in the blanket design. It is a dead decoration and the weaver may reasonably hope to remain free from sin.

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THE Shawnee, an Algonkin-speaking tribe composed of five major divisions which have been notable for their wanderings over the eastern and central regions of North America during the past three centuries, possess a type of social organization that embraces many of the features characteristic of a sib system, but lacks the usual sib units. In place of either clans or gentes the Shawnee have six name groups:

*Turkey name group* (peleewomhsoomi),<sup>2</sup> representing bird life.

*Turtle name group* (kkahkileewomhsoomi), representing aquatic life.

*Rounded-feet name group* (petekoθiteewomhsoomi), representing carnivorous animals, as the dog, wolf, and the like, whose paws are ball-shaped or "rounded."

*Horse name group* (mseewiwomhsoomi), representing herbivorous animals as the horse, deer, buffalo, and elk.

*Raccoon name group* (θepatiiwomhsoomi), representing animals having paws which can scratch like those of the raccoon and bear.

*Rabbit name group* (petakineeθiwiwomhsoomi), representing a gentle, peaceful nature.

These name groups are common to the five divisions of the Shawnee tribe and each Shawnee individual belongs to a name group, not by virtue of decent, but through his personal name which is given to him shortly after birth.

#### LINGUISTIC ANALYSIS

A linguistic consideration of personal names gives specific insight into some problems which are felt to be peculiarly important by the Shawnee

<sup>1</sup> Linguistic and ethnographical field work among the Shawnee during 1933-34 was made possible by funds from the Indiana Fellowship in Anthropology at Yale University and from a Fellowship of the American Council of Learned Societies. The Shawnee are now located in northeastern (Eastern and Cherokee Shawnee) and central Oklahoma (Absentee Shawnee). Statements which are credited to specific informants are followed by the initials of the informant inclosed in parentheses except in the case of Jennie Cegar whose full name is used. Absentee Shawnee informants referred to are Billy Williams, aged 64, his daughter Mary Williams, 25, James Clark, 60, Earnest Spybuck, 65, Jennie Cegar, 84, all of the Kišpoko division; Thomas Wildcat Alford, 76, of the Pekowi division. Information concerning the Cherokee Shawnee was obtained almost exclusively from Frank Daugherty, 74, of the Mekoče division. Eastern Shawnee informants were Carrie Bluejacket, 78, Henry Turkeyfoot, 63, both of the Mekoče division, and Nancy Skye, 75, of the Pekowi division. Mrs Skye really belongs to the Absentee band, but married a Peoria and has been associated for many years with the Eastern Shawnee.

<sup>2</sup> Shawnee names are written in a phonemic orthography described in C. F. Voegelin, *Shawnee Phonemes* (Language, Vol. 11, 1935, pp. 23-37). It should be noted that the glottalic phoneme, here written h, is phonetically a glottal stop before consonants and a fricative before vowels.

even at the present time. For example, an individual who wishes to know whether or not a new acquaintance belongs to his own name group may, according to native theory, determine this by an analysis of the personal name of the new acquaintance.<sup>3</sup> It is as though one would in English conjecture on the meaning of names like "Robinson," decide that the meaning intended was "son of a robin," and that, accordingly, the bearer of the name belonged to the bird name group. Another conjecture might suggest "a robbing son," and that, accordingly, the bearer of the name belonged to the rounded-feet name group which includes reference to such marauding animals as wolves, known to have rounded feet. The Shawnee speaker assumes, as a matter of course, that the meanings of personal names and their reference to name groups are ambiguous. The "real" meaning of a name and the actual name group referred to is decided by the name-giver, and no one presumes to contradict this decision when it is made known. One feature of personal names is without ambiguity, and that is sex gender. But this creates a difficulty for the name-giver, not in "finding" feminine names but in finding appropriate names for boys.

The final element of a name determines its sex gender. The most common feminine endings are *hši* (to recline or to be in a situation or action) and *pama* (to look at something animate). Since these endings readily combine with a large number of preceding stems, it is comparatively easy to find names for girls. Common masculine endings are *škaka* or *θkaka* (one who is in an action or situation through his feet), *pawi* (to stand), *hθe* (to walk, to move by means of feet or wings), *hšimo* (to voice, having voice), *pto* (to run); all these endings except one (to voice) imply an instrumentality of feet. Preceding stems are limited to meanings compatible with the generically narrow sense of virtually all masculine endings. This limitation makes it difficult to find names for boys.

Elements which serve as masculine or feminine endings in personal names may be used in ordinary Shawnee words. Examples are:

*yehkawhšiki*—when he is cold.

*niwaapama*—I look at him.

*noowešihšimo*—I have a good voice.

*niipawi*—he is standing.

The chief difference between regular Shawnee words and personal names is

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<sup>3</sup> In this particular class of words, as in the vocabulary generally, the Shawnee speaker shows a high degree of consciousness of stem compounding. However, since accuracy in native analysis varies considerably, a study of popular etymology would need to proceed on the basis of individual cases. A rather edited version of Shawnee popular etymology is reflected in our presentation of personal names.

that the latter bear a participial meaning ("One who . . .") without necessarily having a participial form; the final element, in addition to its basic meaning, indicates sex gender which is not otherwise expressed in Shawnee. Virtually all elements of personal names are employed in ordinary Shawnee words. Thus, the feminine ending *hsi* is in ordinary words a postpositive stem followed by animate intransitive suffixes or transitive animate suffixes and the same stem, morphophonemically, may be followed by inanimate intransitive suffixes or transitive inanimate suffixes: as employed in personal names, however, the stem is not followed by a suffix.

In addition to the final elements which determine sex gender, one of the following prefinal stems is found in most personal names:

*piye(e)*—to come, to extend hither.

*pliwe(e)*—concerning plumage.

*kam(i)*—concerning an expanse of water.

Less frequent prefinal stems occur. Examples are:

*miyee*—concerning a path.

*hakwiči*—to float.

Initial stems, which precede these prefinal stems, are limited only by semantic compatibility. In a not uncommon type of personal name, the initial stem immediately precedes the final element. Examples below are followed by ordinary Shawnee words in parentheses which were suggested by informants as being "the same" as the initial stem in the personal names.

The typical morphological pattern for personal names is, then, initial stem (great choice), plus prefinal stem (limited choice, dispensable), plus final element (determines sex gender).

Examples are followed by the abbreviations F for feminine names, M for masculine names, and n. g. for the name group to which the personal name "appertains."

*mayataakwipiiwehšī* One who has curious feathers F, turkey n. g.

*mayataakamhšī* One who reclines in peculiar water F, turtle n. g.

*mayataawipiyehšī* One who extends curiously. F, turtle n. g.

*mayataapama* One who looks at queer things F, doubtful n. g.

*mayataakwihsimo* One having a queer voice. M, turkey n. g.

(*mayataakwi* he is curious, queer)

*pepekihsimo* One who calls in the dark. M, rounded-feet n. g.

*pepekitaakamhšī* One who is in dark water. F, turtle n. g.

*papekitaapama* One who looks at dark things. F, turkey n. g.

(*pepekiča* it is dark.)

*haapetaapiyeškaka* One who is coming along making his feet go in a hurry. M,  
raccoon n. g.

haapetaapama One who hurriedly looks at him. F, turkey n. g.

haapečihšimo One who calls in a hurry. M, turkey n. g.

haapečipiiwehši One who gets feathers quickly. F, turkey n. g.

haapetaakamhši One who lies in hurried waters. F, turtle n. g.

(haapeteθi he is in a hurry.)

howešipemhθe One who travels well. M, horse n. g.

wešihšimo One who has a good voice. M, turkey n. g.

wešipto One who runs well. M, rounded-feet n. g.

welepiyehši One who is in a fine pool. F, turtle n. g.

welepiiwehši One who has fine feathers. F, turkey n. g.

(howeši good, well. It is said that "wele-" is just another way of saying weši-." Phonologically, *i* before *i* becomes *š*. The initial syllable, *ho-* is frequently lost in personal names.)

waapaapiyehši One whose file, extension, is white. F, horse n. g.

waapaamiyepto One who runs in a white path. M, horse n. g.

waapihšimo One who has a white voice. M, turkey n. g.

(waapiwiikiwa white house.)

wišikaapawi One who stands firmly. M, horse n. g.

wišikipito One who runs vigorously. M, rounded-feet n. g.

wišikaapama One who looks at him strongly. F, rounded-feet n. g.

(wišikanwi it is strong.)

hoθaawaθkaka One who puts his foot in something yellow. M, rounded-feet n. g.

hoθaawipiiwehši One who has yellow feathers. F, turkey n. g.

hoθaawaakamhši One who reclines in yellow water. F, turtle n. g.

hoθaawaakwičši One who floats in the yellow. F, turtle n. g.

(hoθaaweθi he is yellow.)

haayiitaaθkaka One who stamps firmly. M, rabbit n. g.

haayiitawhši One who is held fast. F, horse n. g.

haayiitaapama One who looks at someone firmly. F, turkey n. g.

(haayiicilawi he does so firmly.)

No clear cut morphological distinction can be drawn between nicknames, which do not imply a name group, and abnormal types of personal names, which do imply a name group but have no direct expression of sex gender. The latter are said to be morphologically abnormal because "people didn't say the name right as the name giver said it." This is probably half of the story; it seems likely that what are now considered personal names not properly spoken as the name-giver intended are often nicknames which the individual acquired later in life and used to the exclusion of his original personal name. Both abnormal personal names and nicknames tend to use nouns and nominal suffixes instead of the verbal elements employed in the majority of personal names. Examples are:

miina Blackberry M, nickname

- čiipaaniθa* Ghost person. M, nickname.  
*tootiiθa* Little frog. Frenchman. M. nickname.  
*heleθeniθa* Little elephant. F, doubtful n. g., Eng. loan.  
*piliθa* Little Bill. M, nickname, Eng. loan.  
*wiliwi* Willie. M, nickname, Eng. loan.  
*mačilenawe* Mean person. F, nickname.  
*mšimhkwa* Big bear. M, raccoon n. g.

The sex indicated for these names is merely that of the individual bearing the name; if the individual were not known, the gender of the name would not be known. The following short names are conjectural as to sex, gender, name group or nickname, and meaning:

- waθkaka, škaka* (final element indicating action by feet and probably masculine).  
*čaati, čilo* (čaki small).  
*nemhši* F, probably turkey n. g. (nenemhki thunder bird).

From a list of some seven hundred personal names,<sup>4</sup> the following proportion of affiliation with the different name groups was found:

Rounded-feet n. g.	20 percent
Turkey n. g.	18 percent
Turtle n. g.	15 percent
Horse n. g.	13 percent
Raccoon n. g.	2 percent
Rabbit n. g.	1 percent

To this list must be added five percent of the personal names where name group affiliations were conjectural and twenty-six percent of the personal names where name group affiliations were unknown. Certain of these were nicknames, and counted as being outside the name group system; others called forth no response when read. To some extent, at least, the latter were personal names of individuals generally known by nicknames. The percentage of Rabbit names is very low for the last two or three generations of Shawnee; the last chief of the *θawikila* division who bore a Rabbit name

<sup>4</sup> These names are mainly of Absentee Shawnee still living, they were taken from a record entitled "The Family Register," kindly loaned to us by Mr Pfeifer of the Shawnee Indian Agency. The names were read to informants who knew most of the people named and their name group affiliations. Individuals were recorded and known by one name. Nicknames tend to but do not necessarily supplant personal names. Only a minority of individuals ever receive Shawnee type of nicknames but with the growing attendance at schools almost all young people are known by an English name as well as by a Shawnee name. English surnames were adopted some three generations ago by the Absentee Shawnee for convenience in correspondence and in the allotment system.

died some forty years ago leaving no lineal descendants and for this reason people "were loath to name their children into the Rabbit group" (TWA). It is interesting to note that at the present time several old people who feel that "fine people grow out of Rabbit" are "starting up this name group again in naming young babies" among the Absentee Shawnee.

In giving the name group affiliations of various individuals, the Shawnee often allude to members of the Turkey name group as Chicken, Eagle, Chicken Hawk, or Fowl man or woman, the old name for the Turkey group is said to have been the Eagle group. Members of the Rounded-feet group are often called Wolf, Dog, or Panther man or woman; likewise those of the Raccoon group are often called Bear men or women, those of the Horse group are often called Deer and Deer is said to have been the old name for the Horse group. Rabbit "always stands alone." In the abstract, the number of name groups is often said to be twelve, but no more than six are ever specified, and informants giving specific lists showed no disagreement. Doubling the number of name groups when the number is given as an abstracted quantity may be due to the fact that several species of animals are linked together in some of the name groups and that twelve men and twelve women hunters and cooks in the Bread dances are chosen according to name group affiliations; the ceremonial numbers are four, six, and twelve.

#### NAMING CEREMONY

The following description of the procedure for naming infants applies primarily to the Kišpoko and Pekowi divisions; divergent procedure for other major divisions is noted.

From one to nine days after a child is born, while the mother and child are still in seclusion, the father asks two old men or two old women to come to his house on the evening of the ninth day after the birth of the baby. These two men or women should be at least middle-aged and preferably well advanced in years; men or women who have had several of their own offspring die are not likely to be asked to bestow names. Name-givers may be related to the child or they may merely be family friends. It makes no difference in which name group the name-givers belong, or whether they both belong to the same name group. When both name-givers have arrived, the father tells them,

We called you here this evening because I want you to name my child in the morning; tomorrow that child will have been born ten days. You folks study about this matter over and over; think about the animals [connected with the name groups]—how they act, how they move, everything like that. Tonight think about the

turtles, chickens, the wolf, deer, coon, rabbit; study about them. When you go to sleep, keep this thought with you; maybe you'll dream about that name, some way. If you don't dream about it, maybe you'll just think about it, and so find a good name. And you must not be angry when we take only one of the names you offer; we will be glad to hear both names, when you have found them in the morning (BW).

The two name-givers retire and "offer prayer during the night to the Creator who owns the people, the names, and everything" (MW). They each pray extempore and privately, and do not use Indian tobacco which is usually associated with formularized prayers. When a name has occurred to him, the name-giver must determine to which of the name groups it belongs. When a stem in the name refers to feathers (e.g., *piiwe*) or to water (e.g., *kami*), the name necessarily belongs to the Turkey name group or the Turtle name group and the name-giver has no choice in the matter. He will nevertheless ponder over the name because the task of naming is regarded as essentially intricate and serious, not to be lightly disposed of. The name found should not be a duplicate of any name that has ever been used before either for a deceased or a living person, even though reference to the dead by personal name is not tabu. Genealogies and lists of personal names show that duplications of personal names occur only rarely.

The table below shows naming in two families not specially selected.

<i>Person named</i>	<i>Name group</i>	<i>Name-giver's name group</i>	<i>Father's name group</i>	<i>Mother's name group</i>	<i>Relation of name-giver to person named</i>	
Nancy Skye	Rabbit	Turtle	Rounded- feet	Rabbit	F's f*	
Lester	"	Turtle	Horse	Horse (Peoria)	Rabbit	F's f's ss
Juanita	"	Turtle	Turtle	Horse (Peoria)	Rabbit	M's m's ss's d
Billy Williams	Horse	?	Horse	Chicken	"	"
Mary	"	(1) Rounded- feet	Horse	"	"	F's f
		(2) Horse	"	"	"	F's f's f's b's ss
Fred	"	(1) Horse	"	"	"	F's f's f's b's d
		(2) Turtle	"	"	"	F's f's f's b's ss
Robert	"	Turtle	Chicken	"	"	M's m
Pauline	"	"	Horse	"	"	F's f's f's b's ss
Jeanette	"	Horse	"	"	"	"
Arthur	"	(1) Chicken	Chicken	"	"	M's m
		(2) Rounded- feet	Rounded- feet	"	"	Friend (JC)

\* F=father; m=mother; b=brother; ss=sister.

One name-giver of wide experience (JC) says that the child is commonly named into the name group of the name-giver if the parents do not specially request another name group; the parental request would ordinarily be to have the child in one parent's name group unless one or both parents are sickly.

The data from the Skye and Williams families and from various individuals would indicate that the child's name group coincides with a parent's or the name-giver's no more frequently than might be expected if no theoretical preference were stated.

On the morning of the tenth day the women relatives of the child rise well before daylight to finish the preparations for the naming breakfast. The mother of the child cleans herself in her secluded hut where the child was born and, bringing the child with her, returns to the family dwelling house where the family kin, both maternal and paternal, are assembled. When the mother and child arrive at the house, the name-givers, if they are women, take the infant and give it a sponge bath in plain lukewarm water; if the name-givers are men, the mother bathes the child.

Now the naming ceremony proper is enacted. The mother sits down, holding the child; the two name-givers stand while the other relatives sit or stand nearby. One of the name-givers speaks, announcing the name he has found and its name group affiliation. He then goes on to tell about the habits of the name group animal involved. When the first name-giver is through talking, the other name-giver tells about the name which he has found, as the first did. The two name-givers must offer names belonging to two different name groups; they consult on this matter before breakfast, telling each other the names found during the night. Having heard both names offered, the parents must choose one "which sounds good." If the parents do not agree in their choice, the mother's vote is decisive "because she gave birth to the child" (MW). It is perhaps not without significance that no case of parental disagreement came to our attention; one name is beyond difference of opinion superior to the other, partly due, no doubt, to the connotation of the personal name itself, but perhaps more especially because one of the name-givers is more persuasive than the other in telling about the name which he offers. The father tells the name-giver whose name has been selected that he is glad a turkey (or other) name was found; maybe it will take good care of the child; perhaps the child will grow up to be a man, then an old man, a grandfather. "We choose this name—(mentioning the personal name)," the father says. The mother then gives her child to the name-giver while the father hands the name-giver a short string of



white, finely cut beads to use in naming the child. The name-giver holds the child while he addresses the Creator at length. Exerpts from his speech are:

. . . It is the Creator's rule that we name a child after ten days. . . . We are glad to see this child this morning . . . perhaps we think that this child doesn't know anything, just as we don't, but he (she) knows more than we know, because he just came from the Creator. . . . The Creator told him to come to these relations in this world. . . . I was studying about the Creator. . . . After that I studied about the animals . . . she [Creator] put them here for us to live with, to give us name groups that way, so [the animals] can carry us to be a man or a woman some day. . . . The Creator gave these animals power so that they could be wise, smart. That's why I found a Rounded-feet name; that's what I'm going to give to this child now. . . . I hope that the Creator will take pity on me and help me to live a little longer with these people in this world, because I was studying about the Creator last night before I went to sleep, before I found this name (BW).

The name-giver then returns the child to its mother but continues in his speech.

. . . Some day perhaps this boy will be a grown man. . . . Then he will have food set out for his relatives, whenever they come to visit, just as we set food out here now. . . . At that time, this name will have carried him a long way; he'll be a young boy, then a young man, then an old man some day and they will call him grandfather. It will carry him that far, a long way. Everybody will call him—— (mentioning the personal name) (BW).

As he pronounces the name, the name-giver holds the string of beads in his extended hands.

That's what everybody will call him—— (mentioning the name again); from this morning on they will call him ——; everybody all the time will call him —— (BW).

Having repeated the personal name four times, the name-giver ties the string of beads around the child's neck. These are worn by the child until the string breaks, when they may be picked up and saved for the child by its mother; in many cases they are lost.

Some of the relatives smile and tell the child, "You have a good name, —— (mentioning the name). Well, well, we're going to eat with —— this morning; it's good." The parents tell the name-giver that they thank him for giving their child a good name. The assembled company then eats breakfast. A taste of each of the different foods is offered to the child, the mother touching her fingertip to each variety of food and letting the baby lick her finger. After breakfast the relatives give the child small gifts and say, "I

brought this for you,——(mentioning name). I heard you had come; I'm glad that you came, and I bring this to you." The child is glad to get those things; it proves to him that his relatives are glad to see him (BW).

A few days after the naming ceremony, the mother may present the name-giver with a gift, such as a shirt or a length of dress goods or even a horse. This gift is not obligatory. Among the Cherokee Shawnee, but not among the Absentee, the child is obliged to give its name-giver a dinner once a year for four successive years. A distinct symbolism is implied here: the child "supports the name-giver" for four years as payment for either his original name or his new name in the case of name-changing. The Cherokee Shawnee, and formerly the Eastern but never the Absentee, sometimes name groups of children after the spring or fall Bread dances as a variant procedure to the individual naming.

Illegitimate children are named in the same fashion as legitimate children save that the father of the child is of course absent.

The naming ceremony is occasionally held at peyote meetings among the Absentee. After "morning water" has been drunk at sunrise the child is named before breakfast with the same procedure as naming at home.

#### NAME CHANGING

Name changing follows the general procedure of original naming in attenuated form but introduces one significant concept: the original name, regarded as unsuccessful, must be psychologically and symbolically discarded before the new name is given. A name is unsuccessful when the individual bearing the name is sickly, never when the individual is healthy. Either the parents or a shaman decide that the name is at fault.

The following accounts show that personal names were unsuccessful because the parents had been negligent in the original naming; because of this the original name did not "agree" with the individual bearing it. Theoretically, duplication might make a name unsuccessful because a person bearing the name of one dead is also likely to die and a person bearing the name of a living person is likely to die when his namesake dies, but this is a danger which may be deliberately risked. In most accounts of re-naming, only one name-giver is asked to give the new name.

... We hadn't gone by the rules in naming Arthur the first time; we just sent for two old women who stayed overnight and gave Arthur his name, but we didn't have any breakfast for his kinfolk. We just played and gave the name, as though we didn't care about the rules. But when Arthur got sick we made up our minds that we'd do it better, and so we had him given a new name at a peyote meeting (BW).

. . . Then my father asked Jim Clark what name had been given him for Arthur during the peyote meeting that was just over, and Jim told my father, just as Tony had. My father thought over the two names. The name Jim had offered was the same name as a second cousin of ours had. My father said that Jim's name was the same as our cousin's, but he said that he like the sound of it, and that he realized it was hard for a man to get a new name at a meeting, where he was being kept busy all night. And my father also said that he knew and trusted our cousin who already had that name, and felt the same name wouldn't hurt Arthur, so he chose Jim's name. Then he asked my mother if she thought it was a good name, and she said, "Yes." So my parents took the name, because they liked it better than the one Tony had offered (MW).

In another case NS's daughter's child and another child received the same name: both were about the same age but lived some distance apart and were named by two different old women who had no knowledge of what the other was doing. NS's only comment on her grandchild's name was, "It's a good name." Neither child had its name changed. In the account of name changing which follows, duplication of names is not involved.

Old Lady French came up to the house to change Freddy's name. She took Freddy down to the river and when they got down there she talked to Freddy and told him, "The reason why I bring you here now is this: your name doesn't fit you; it makes you sick. Now we're going to wash it off. Grandfather water is going to take your name off you." Then she talked to the water. "Grandfather, I bring this child here for you to wash off his name. It doesn't fit him or take care of him. Now I'm going to give him another name. The Creator put you here, Grandfather water, in this world for this purpose, for you to take care of your grandchildren, the people, as long as the world exists, as long as the sun rises and Mother earth remains. The Creator knows everything that is happening all the time. I ask you to help us this morning; the Creator will be glad of this. She will know about it, because the Creator knows everything that happens day and night, all over the world. And you, Grandfather water, you have power from the Creator. That's why you can help us, because you have that power given you by the Creator, to take care of people here in this world." Then she put Freddy in the water, all the way in. It was not necessary to use tobacco praying to Grandfather water. Some old people use it and some don't, because they haven't any Indian tobacco. Then she took the child home. When they came back to the house, she kept the child beside her and told us, "I've brought this child back. I have talked to Grandfather water and I told him what to do for us. I asked him to help us. I hope Grandfather water will accept my words. If he does take my words, what I asked him to do, this child here will grow to be a man some day. He'll get old some day, so you people will call him grandfather. I have asked Grandfather water to carry this child along that far. When this child grows up, he will help the people in worshipping according to the Creator's rules. That's what I've told Grandfather water. So Grandfather water took this child's

old name off. Now I'm going to give him a new name. He will be called mawatipiyeškaka. He will be a Turtle man. [Any other name group except the one to which the child previously belonged might have been chosen.] People will be calling him mawatipiyeškaka every day from now on, they will always call him mawatipiyeškaka; that's what he will be known by, mawatipiyeškaka. And that name may carry him until he is grown up, it may carry him until he is old and is called grandfather" (BW).

#### FUNCTIONS OF THE NAME GROUPS

An individual shares in some degree the characteristics of the animals connected with his name group. It has been known for ages past, according to Jennie Cegar,

that Rounded-feet man is of a bad disposition; Horse man is less so, though he kicks like a horse; the best disposed people are the Turtle people. Rabbit is the most docile and fine people grow out of Rabbit; it never bites.

A definite emotional rapport exists between members of a name group and the name group fauna because "the animals of your name group know that you have a name connecting you with them" (MW). Personal advantages are expected and vague obligations are assumed in these relationships, for

if the Indians have names pertaining to animals, as the animals don't get sick, neither will the Indians; they will always be in good health. The members of a name group must follow their animals, perhaps these animals will help them through life, although they couldn't give a person any one thing like long life or power to doctor or success in war (MW).

The name groups function primarily as friendship groups in which all the members of the group have the privilege of boasting about the animals associated with their own name group and belittling those pertaining to other name groups. Examples of teasing show, however, that often no direct reference is made to the name group animals and that the raillery between two or more persons belonging to different name groups may be merely personal. Boasting and teasing is indulged in at all times save when some serious business is afoot, such as a council, a peyote meeting, or a funeral. Fellow members of a name group, spoken of as partners, do not tease one another and should not "talk bad" (gossip maliciously) about one another. A person present during a contest of wits between one of his partners and a member of any other group should help his partner by entering into the conversation and supporting his partner's jibes with a few of his own. Originality and ingenuity are required in the defense of one's own name

group and in the ridicule of others. A pointed and subtle thrust is greeted with vociferous cheering; a dull and trite remark is received in critical silence.

The question of name group affiliation enters into the consideration of an individual's eligibility to fill certain rôles in social, religious, and political activities. Here the relationship between the name group animal and the bearer of a name referring to that animal is again emphasized.

In the matter of burial, the man or woman in charge of a funeral and the two men or women gravediggers and corpse-handlers must belong to different name groups from that to which the deceased belonged (FD, JC, NS, BW). NS even attempted to specify from which name group funeral officials would be chosen;<sup>5</sup> FD said that persons from "any of the other name groups" would do. Among the Absentee Shawnee formerly, the man who delivered the formularized speech at the grave was not in charge of the funeral as is often the case now, and it made no difference whether the speaker and the deceased belonged to the same name group or to different ones, the prime consideration being that the speaker know the formula letter perfect (Jennie Cegar).

At the present time all animal tales are told indiscriminately by members of any name group but in former days members of a particular name group preferred to tell stories which selected that name group's animals as the actors or heroes of the stories. Spencer<sup>6</sup> goes so far as to imply that the cycle of wildcat and rabbit tales was told only by members of the Rabbit name group. MW said this was a matter of preference in telling stories, not a matter of exclusive privilege.

One of the methods formerly used to secure rain was to hold a council in order to select a suitable Raccoon man who was given Indian tobacco by the chief and told to go to a spring to offer it to the water for

our Grandfathers, the Thunderers. The Coon man goes to the spring alone. There he talks to the water and tells it what to say to the Thunderers. . . . He puts the

<sup>5</sup> She formulated it as follows.—

<i>Name group of deceased</i>	<i>Name group of male gravedigger</i>	<i>Name group of female corpse-handler</i>
Rabbit	Rounded-feet	Rounded-feet
Turtle	Rounded-feet	Rounded-feet
Horse	Coon or Rabbit	Coon
Turkey	Coon or Horse	Coon
Coon	Rounded-feet	Rounded-feet
Rounded-feet	Coon (sometimes)	Coon, Rabbit, or Turkey

<sup>6</sup> Joab Spencer, *The Shawnee Indians* (Transactions, Kansas State Historical Society, Vol. 10, pp. 382-402, Topeka, 1908), p. 396.

tobacco in his left hand, doubled up; when he gets through talking he slides his left hand slowly down into the water and frees the tobacco. Then he returns home and the rain comes when he has asked for it to come (BW).

Whenever a group of travelling Shawnee came to a considerable body of water, all Raccoon men of the party were made to stay in the rear while the group crossed the water, because, "if the Raccoon men didn't stay behind, the water might rise up and drown the people" (BW).

The custodian of the Kišpoko division's bundle or palladium has to be either a Turtle or Turkey man; Turtle is preferable. When rain is needed the old man in charge of the bundle takes it out in the woods, opens it, and offers prayer, calling for rain. . . . This old man has to be a Turtle man. Turtle lives in the water and so do the Thunderbirds; that's why it rains quickly when he takes it out (JC).

For the two yearly Bread dances two men and two women from each of the six name groups must be chosen as the hunters and cooks. The male and female leaders of these two groups of twelve persons must be Turtle or Turkey for the spring Bread dance, and Horse or Rounded-feet for the fall Bread dance. The spring Bread dance is a prayer for the crops and general fertility and is "called" by the women (FD): it is felt that Turtle and Turkey, because they lay eggs, are peculiarly fertile. The fall Bread dance is called by the men: this is the hunting season when horse, deer, and the rounded-feet animals play conspicuous rôles. At the present time, if it is impossible to secure a man or woman belonging to the right name group to act as leader for a Bread dance, a member of another name group may be "borrowed" for the occasion (ES). The spring Bread dance ushers in a series of spring ball games which are played between men and women. When the ball is put away for the season in June a Turtle or Turkey man and woman take active parts in the ceremony, but the other name groups are also represented. The Men's dance is given during August by the members of the Kišpoko division. The night before the dance the Turtle man who is the custodian of the Kišpoko bundle

brings the bundle over to the dance ground and leaves it in the thickets far off. Early the next morning he takes a Turtle, a Turkey, a Horse, and a Rounded-feet man with him and goes down to open the bundle and puts bunches of feathers contained in the bundle on these four men (BW).

Every major division of the Shawnee has (or had) as a palladium a sacred bundle. Great harm will befall the people to whom the bundle pertains if it is transported in any fashion except slowly. Because Turtle is a

slow-moving animal, a Turtle man is best qualified to carry the bundle. This is true for all Shawnee (NS, BW, JC, Jennie Cegar). In one instance men from the Turkey name group were given preference over Turtle men in transporting a bundle, but for a particular reason. When the Absentee Shawnee moved from Oklahoma to Kansas during the Civil War, they knew they were in danger of being followed and attacked by their pro-Southern neighbors, the Creek, accordingly, the head chief of the Absentee chose two men from the Turkey group to take charge of the *ṭawikila* division's bundle on the trip north, "because a turkey is ready to fly quickly" (Jennie Cegar). If necessity arises for transporting the *Kišpoko* bundle, three Turtle or Turkey men are put in charge. It is unthinkable that members of any other name group should be concerned.

A Rounded-feet man can't touch the bundle, unwrap it or have anything to do with it; something would happen; the people who belonged to that bundle would be destroyed. At a pinch a Turkey man can substitute for a Turtle man and handle the bundle, but no matter how good a man is, if he isn't in either of these two name groups he can't take care of the bundle (JC).

A Shawnee chief must lay aside his office of chief if he is to lead a war party: his chiefly office is a distinctly peaceful one. Accordingly, a chief should be a Rabbit man: this is a peaceful name group (FD). "Most of the time they give a Rabbit name to a chief's child" (BW). It is not necessary, however, to have chiefly blood in one's veins in order to possess a Rabbit name and conversely, a chief may belong to another name group. The present Pekowi chief is a Horse man.

Some of the name groups are linked in pairs. Turtle and Turkey on the one hand, Horse and Rounded-feet men on the other, are "friends." Racoon men are opposed to Turtle men. Neither the linkage nor opposition of particular name groups has any significance except in the joking that is carried on between the members of different name groups:

Horse and Rounded-feet men versus Turtle and Turkey men can help one another in joking together. Coon and Rabbit just stand alone, but Coon and Turtle joke each other all the time because when coon was running along the river he found turtle's eggs and ate them, this didn't suit turtle very well. That's why Coon men and Turtle men joke so much together (BW).

All informants agree that inheritance of property is in no wise determined by name group membership, that competitive games are never played between members of different name groups acting as units, that a traveler did not necessarily stop for shelter or hospitality at the home of one of the same name group, that name groups had no officials, no sacred

bundles, and never foregathered as units, that individuals never decorated their persons or their personal possessions with representations of name group animals as such. Swanton<sup>7</sup> speaks of "small poles . . . analogous to . . . totem poles" which among the Delaware and Shawnee "were erected in the four corners of their medicine-lodges." We could not obtain any data confirmatory of this statement. Informants described the Shawnee medicine lodge as a very small, hastily constructed and impermanent dome-shaped affair, only large enough to contain the doctor and patient. Descent from the name group animals is not claimed.<sup>8</sup> An individual is free to kill the animal or animals affiliated with his own or any other name group. The name groups are not exogamic units.<sup>9</sup> A Horse man, for example, may marry a Horse woman or a woman from any other name group. The name groups did not enter into the False Face performances as formerly given by the Cherokee Shawnee (FD). There are no origin myths attached to the name groups. Instead, it is merely stated that "name groups were given to the Shawnee by the Creator when she was living on this earth during the First Creation" (JC).

#### EARLIER DATA AND SOME COMPARATIVE OBSERVATIONS

In his brief discussion and description of Shawnee social organization, Lewis H. Morgan<sup>10</sup> makes many statements at variance with those contained in the present paper. Morgan visited the Shawnee in Kansas in 1859 and 1860;<sup>11</sup> the Absentee Shawnee were in Indian Territory at that time and his information was obtained from members of what are now known as the Eastern and Cherokee Shawnee groups. Since pre-Revolutionary days the Eastern Shawnee have intermarried and been intimately associated with a so-called Seneca group. These two groups lived together at Lewistown, Ohio, moved to Kansas together (TWA), and at present occupy adjoining

<sup>7</sup> John R. Swanton, *Totem Poles* in *Handbook of the American Indians North of Mexico* (Bulletin, Bureau of American Ethnology, 39, Pt. 2, 1910), p. 795.

<sup>8</sup> One informant only, NS, stated that the Shawnee claim descent from the name group animal.

<sup>9</sup> NS said that "the Shawnee used to watch and not let members of the same name group marry, but they stopped watching long ago. When they did follow this rule, the children might belong to either parent's name group, or to a different name group." NS's statements relative to descent and exogamy were denied by all other informants, some of the more sophisticated informants called the name groups "clans" and equated them to the clan or gens groupings of neighboring tribes, but everyone except NS denied they were exogamic units.

<sup>10</sup> Lewis H. Morgan, *Ancient Society* (Chicago, n.d.), pp. 172-74.

<sup>11</sup> Lewis H. Morgan, *Systems of Consanguinity and Affinity of the Human Family* (Smithsonian Contributions to Knowledge, Vol. 17, Washington, 1871), p. 217, fn. 1.



allotments in northeastern Oklahoma. This must not be taken to mean that the social organization of the group in question had become an aberrant mixture of Shawnee and "Seneca" traits: in fact HT, an Eastern Shawnee, gave us in 1934 the same information regarding the number of name groups, their names, etc., that Absentee Shawnee informants had given. It is possible, however, that Morgan secured at least part of his information from Shawnee informants whose culture was more "Seneca" than Shawnee. James Mooney<sup>12</sup> quotes from Morgan on the subject of Shawnee "gentes," which were said to be thirteen in number, named for various animals, and *not unilateral*.<sup>13</sup> These "gentes" and our name groups are the same units. Morgan's Loon, Buzzard, Owl, and Turkey gentes are, according to our informants, associated with the Turkey name group which includes in the scope of its reference everything connected with birds of any species. Other gentes animals of Morgan are likewise included in the reference of one or the other of the six name groups. Still there is nothing inherently improbable in supposing that the six name groups of the present day represent a consolidation of a former larger number; as we have mentioned (p. 622), in the abstract the number of name groups is often said to be twelve. But that the Shawnee ever had thirteen name groups was consistently denied by all informants, some of whom were so far advanced in years as to bridge the gap between Morgan's time and ours.

In several of his statements Morgan confuses the name groups and the major divisions; the latter each have a chief, messenger, and other officials which are lacking in the name groups.

The most striking and certainly the only original feature of Shawnee name groups is a negative one: the name groups are not unilateral in an area where unilateral systems of social organization are to be expected. The total configuration of positive and negative traits constituting the name group complex and a cursory examination of the distributions of its component traits shows that none of its positive traits are limited to the Shawnee, the following similarities to features of the complex have been noted among a few Eastern and Plains tribes.

*Nomenclature.* Animal nomenclature for sib or phratric groups predominates throughout eastern North America and extends beyond this area. Among the Unami Delaware<sup>14</sup> Wolf phratry is called "Rounded-

<sup>12</sup> James Mooney, *Shawnee, Tecumseh*, in *Handbook of the American Indians North of Mexico* (Bulletin, Bureau of American Ethnology, 30, Pt. 2, 1910), pp. 537, 714.

<sup>13</sup> Morgan, *Ancient Society*, p. 173.

<sup>14</sup> M. R. Harrington, *A Preliminary Sketch of Lenape Culture* (*American Anthropologist*, Vol. 15, 1913), p. 213.

Feet;" the Rounded-feet name group of the Shawnee includes wolf, Raccoon and Rabbit occur as names for clans among Prairie Potawatomi<sup>15</sup> and various Southeastern tribes.<sup>16</sup>

*Number of the name groups.* Variable for sib or phratric groupings in this area, but Prairie Potawatomi<sup>17</sup> have six phratries, five of which show correspondences to animals represented in the Shawnee name groups.

*Name feast.* Found among Sauk,<sup>18</sup> Prairie Potawatomi,<sup>19</sup> Winnebago,<sup>20</sup> and such distant tribes as the Assiniboine.<sup>21</sup> Sauk and Prairie Potawatomi name feasts show greatest similarities to Shawnee naming breakfasts.

*Bead necklace as token.* Chickasaw,<sup>22</sup> Yuchi<sup>23</sup> (very similar). Seneca "hang the name around the neck" but do not actually use a necklace.<sup>24</sup>

*Payment to name-giver.* Prairie Potawatomi,<sup>25</sup> Ioway,<sup>26</sup> Winnebago,<sup>27</sup> Assiniboine,<sup>28</sup> Blackfoot.<sup>29</sup> Among Prairie Potawatomi child's parents give name-giver four gifts.

*Sex gender expressed in names.* Probably widespread: specified for Delaware,<sup>30</sup> Prairie Potawatomi,<sup>31</sup> Ioway,<sup>32</sup> Seneca.<sup>33</sup>

*Name refers to totem animal.* General for Creek, Siouan, and Algonkin tribes,<sup>34</sup> but for Creek only the most important set of busk titles contains

<sup>15</sup> Alanson Skinner, *The Mascoutens or Prairie Potawatomi Indians* (Bulletin, Public Museum, Milwaukee, Vol. 6, 1924), pp. 17-18.

<sup>16</sup> John R. Swanton, *Social Organization and Social Usages of the Indians of the Creek Confederacy* (Forty-second Annual Report, Bureau of American Ethnology, 1928), p. 115.

<sup>17</sup> Skinner, *loc. cit.*

<sup>18</sup> Alanson Skinner, *Observations on the Ethnology of the Sauk Indians* (Bulletin, Public Museum, Milwaukee, Vol. 5, 1923), p. 16.

<sup>19</sup> Skinner, *Mascoutens or Prairie Potawatomi*, p. 23.

<sup>20</sup> Paul Radin, *The Winnebago Tribe* (Thirty-seventh Annual Report, Bureau of American Ethnology, 1923), p. 128.

<sup>21</sup> Edwin Thompson Denig, *Indian Tribes of the Upper Missouri* (Forty-sixth Annual Report, Bureau of American Ethnology, 1930), p. 516.

<sup>22</sup> F. G. Speck, *Notes on Chickasaw Ethnography and Folk-Lore* (Journal of American Folk-Lore, Vol. 20, 1907), p. 57.

<sup>23</sup> F. G. Speck, *Ethnology of the Yuchi Indians* (Anthropological Papers, University of Pennsylvania Museum, Vol. 1, 1909), pp. 93-94.

<sup>24</sup> William Fenton, ms. notes.

<sup>25</sup> Skinner, *Mascoutens or Prairie Potawatomi*, p. 23.

<sup>26</sup> Alanson Skinner, *Ethnology of the Ioway Indians* (Bulletin, Public Museum, Milwaukee, Vol. 5, 1926), p. 198.

<sup>27</sup> Radin, *loc. cit.*

<sup>28</sup> Denig, *loc. cit.*

<sup>29</sup> Clark Wissler, *The Social Life of the Blackfoot Indians* (Anthropological Papers, American Museum of Natural History, Vol. 7, 1912), pp. 16-17.

<sup>30</sup> Harrington, *loc. cit.*

<sup>31</sup> Skinner, *Mascoutens or Prairie Potawatomi*, p. 24.

<sup>32</sup> Skinner, *Ioway*, p. 193.

<sup>33</sup> William Fenton, ms. notes.

<sup>34</sup> Speck, *Yuchi*, p. 93.

name of animal totem;<sup>35</sup> data for Sauk<sup>36</sup> closely resemble that for Shawnee.

*Name change because of sickness.* Natchez,<sup>37</sup> Hidatsa,<sup>38</sup> Crow,<sup>39</sup> Pomeroun Arawak, Carib.<sup>40</sup>

*Boasting about totemic animal.* Yuchi<sup>41</sup> (myths told). Creek joking relationship involves clanship.<sup>42</sup>

If we regroup the traits in the above list, we see that similarities in detail between the Shawnee and neighboring tribes take on a certain definite clustering. Such traits as nomenclature of the name groups, the number of name groups, the name feast, payment to the name-giver, and the reference in the name to a totem animal are found chiefly in the Central Algonkin area, the closest parallels occurring among the Prairie Potawatomi and Sauk. On the other hand, such traits as bead necklace as a token, name change because of sickness, and boasting about a totem animal are found chiefly in the Southeastern area, with the closest parallels occurring among the Yuchi, Chickasaw and Natchez.

This distribution of component traits in Central Algonkin and Southeastern tribes points to the following interpretation. Shawnee name groups are basically Central Algonkin; it is even possible that the nucleus of the complex was a part of Shawnee culture at a remote time when sibs were only weakly developed, if at all, among the Central Algonkin. With such a nucleus the various Central Algonkin tribes must have developed their special sib systems while the Shawnee instead elaborated the common nucleus into a name group system. It may have been that this elaboration took place only after the Shawnee moved southward, for features of the Shawnee name groups which are atypical in the central area are most at home among Southeastern tribes.

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<sup>35</sup> Swanton, *Social Organization of Creek Confederacy*, p. 101.

<sup>36</sup> Skinner, *Sauk*, p. 17.

<sup>37</sup> John R. Swanton, *Religious Beliefs and Medical Practices of the Creek Indians* (Forty-second Annual Report, Bureau of American Ethnology, 1928), p. 624

<sup>38</sup> Robert H. Lowie, *Notes on the Social Organization and Customs of the Mandan, Hidatsa and Crow Indians* (Anthropological Papers, American Museum of Natural History, Vol. 21, 1924), p. 51

<sup>39</sup> Robert H. Lowie, *Social Life of the Crow Indians* (Anthropological Papers, American Museum of Natural History, Vol. 9, 1912), p. 215

<sup>40</sup> Walter E. Roth, *An Inquiry into the Animism and Folk-Lore of the Guiana Indians* (Thirtieth Annual Report, Bureau of American Ethnology, 1915), pp. 305-306.

<sup>41</sup> Speck, *Yuchi*, p. 72.

<sup>42</sup> Swanton, *Social Organization of Creek Confederacy*, p. 168

# A STUDY OF MAYA MOULDMADE FIGURINES

By MARY BUTLER

## INTRODUCTION<sup>1</sup>

**I**N spite of the relatively unknown and unexcavated state of most of the Maya area, there are in museums a sufficient number of Maya figurines to warrant an attempt to classify them. The scarcity of specimens, especially whole ones, and the uncertainty, in many cases, as to their provenience, introduces a subjective element into classification and conclusions that is unfortunate, but so far unavoidable. The careful work that is being carried on at present in the Maya field will undoubtedly correct or check the tentative results offered here.

Maya figurines are one phase, distinct by reason of style, of the figurine complex that stretches from southern Arizona into South America. Although it is not possible to draw a hard and fast line between mouldmade and modelled figurines, it is possible to divide them into those conforming to moulded technique, some of which may have been done entirely by hand, and those conforming to modelled or "Atchaic" technique, some of which may have been cast in a mould. It is with the former class that this paper proposes to deal, confining the discussion to an examination of human figurines in the Maya area that conform to mouldmade technique.

It would not be advisable however to discuss one class of Maya clay figures without considering briefly the whole group. The mouldmade figurines form the bulk, such as it is, and the most varied part of the material. They range in time from the Old Empire through the Puk-Labná period,

<sup>1</sup> I wish to express my gratitude to the American Museum of Natural History and the Museum of the American Indian, Heye Foundation, of New York City, the Museo Nacional de Arqueología, Etnología y Historia, of Mexico, the Museum of Merida, Yucatan, the Peabody Museum of American Archaeology and Ethnology, Cambridge, Mass., the University Museum of Philadelphia, and Don Rafael Regil of Merida for the use of unpublished material; to Dr A. M. Tozzer of Harvard University for the help and advice given me in preparing this paper, to Dr S. G. Morley of the Carnegie Institution for photographs of Calakmul stelae, to Mr H. B. Roberts of the Carnegie Institution for information about Yucatecan figurines.

Abbreviations used in this paper are as follows:

AM American Museum of Natural History

MAI Museum of the American Indian, Heye Foundation

MM Museo Nacional de Mexico

PM Peabody Museum

UM University Museum

JC Photographs in the possession of Dr A. M. Tozzer of figurines in the collection made by Mrs William M. James of Merida, Yucatan, which is now dispersed

and in space from Copan in the south to Labná in the north. "Archaic" figurines, presumably earlier than mouldmade ones, occur sporadically in the Maya area, and are only twice (at Uaxactun, Guatemala, and Cerro Zapote, Salvador) connected definitely in horizontally stratified deposits with the early strata. Degenerate types of figurines, modelled by hand, occur in Yucatan and late in British Honduras. Large modelled figures attached to braseros are characteristic of the late phases of Maya culture in the northwest and on the east coast of Yucatan, and are found also in Tabasco and in the Highlands. They doubtless represent the god or guardian spirit to whom one was offering incense. The modern Lacandon Indians of the Peten make a degenerate form of figure brasero, consisting of a bowl with crudely modelled human head on the rim (Tozzer, 1907).

#### MODELLED FIGURINES. EARLY

The terms "early" and "late" are assigned merely to permit a distinction between the bulk of the modelled figurines, referred to as "Archaic," and those which are apparent degenerations of finer technique.

Although there may have been no single Archaic culture, spreading agriculture, pottery, and crude hand-made figurines from Mexico to Peru, there were undoubtedly early cultures responsible for such figurines found in various localities. These figurines possess features in common, but they are the features that would characterize the work of any people trying a new art. The crudely made bodies, with little or no clothing, and heads that seem even cruder since eyes and mouth are shown by slits or holes punched in applied pellets, justify the label "Archaic." In contrast to the mouldmade figurines, "Archaic" ones seldom have the added function of whistle. They are usually small clay figures, pure and simple, and may have had a more directly religious meaning than their more sophisticated successors.

Although, in the Maya area, "Archaic" figurines have been found in Vera Cruz (AM), at Uaxactun in the Peten (Ricketson, 1929), throughout the Highlands of Guatemala (Lothrop, 1926; Gamio, 1926-27; UM), in the Uloa Valley (PM, MAI), and in Salvador (Lothrop, 1927; Spinden, 1915), only at Uaxactun, Miraflores in Guatemala, and Cerro Zapote in Salvador do they appear in horizontally stratified deposits. At Uaxactun they occur in the earliest level. At Miraflores, Dr Gamio found the material to be Archaic in the lower levels, Archaic mixed with Historic Maya in the top level.

At Cerro Zapote where Archaic figurines occur, in varying forms, above and below a layer of ash, Lothrop places them at the beginning of a tentative Archaic, Chorotega, Maya, Pipil series. Little can be said at present

about the "Archaic" figurines from Vera Cruz or the Highlands. Mention should, however, be made of the resemblance of some of the Huastec figurines to those from the Uloa Valley.

The "Archaic" figurines from the Uloa Valley form an individual and interesting group. They are unusual in the sophistication and high degree of artistic skill shown, making the mouldmade figurines from the same place seem crude by comparison. The bodies are those of nude women seated, usually, with legs crossed; sometimes with one hand to the face, sometimes pregnant, sometimes holding a child. The heads, which far outnumber the bodies, are conventionally treated, with holes in the center of shallow grooves for eyes, and slits for mouths. They show, however, an occasional feeling for portraiture, and a wide range of hairdressing. The hair appears to be so shaven as to provide a variety of designs that may indicate the status of the wearers. The possibility of these figurines showing not people with ornamentally shaven heads, but people wearing wigs that give such an appearance, is suggested by a line that invariably runs across the forehead, showing apparently the edge of a tight-fitting cap in slightly higher relief than the face. The heads are so meticulously rendered that such a constant element cannot be ignored, and the idea that some of the Maya, like the ancient Egyptians, may have shaved their heads and worn wigs, is strengthened by finding, in both the Usumacinta region and British Honduras, headdresses which seem to start above the natural hairline. Stelae 8-11 from Seibal show a very artificial arrangement of straight hair, and on a stela from Calakmul a head covered with tightly coiled curls gives the idea further support.

What data we have imply that the "Archaic" situation is essentially the same in the Maya area as in Mexico, and that the makers of such modelled figurines preceded the more sophisticated workers who made their figurines in moulds. Different techniques seem to have prevailed in different sections, but these may be found to represent different phases of a more or less homogeneous culture, as in the Valley of Mexico. In the regions peripheral to the main Maya area, such as the Uloa Valley and Vera Cruz, we have no sure time element involved. There are also modelled figurines from the Highlands, archaistic rather than archaic, that show definite links between different districts and probably were contemporary with the mouldmade figurines of the Old Empire (UM).

#### MODELLED FIGURINES. LATE

Finally there are the crude modelled, sun-baked figurines from British Honduras which follow the mouldmade ones and are the last flickers of

figurine-making on the eastern coast of Yucatan (Gann, 1918, pp. 60-61; pls. 8-9, 22). They consist of male figures, each wearing a loincloth, button labrets at the corners of the mouth, and a headdress which may be either a flat cap or a mitre-like cap with a long queue behind reminiscent of a type of headdress found on the stelae at Palenque and Copan. These men are standing, holding either a fan or a knife; seated, practicing self-mutilation; or crouching, with shield and spear.

#### BRASERO FIGURES

Large modelled figures of coarse clay, averaging forty centimetres in height and attached to braseros, occur in Yucatan, Tabasco, and the Highlands of Guatemala. From their association with late remains on the east coast of Yucatan, and their non-appearance at Old Empire sites, such braseros are considered to be of relatively late date. The brasero figures considered here fall into three groups, which conform to the differences in provenience. Some sherds from Piedras Negras, apparently pieces of other types of figure brasero, are too fragmentary to do more than suggest that this kind of vessel goes back to the Old Empire.

On Tabascan censers a male human figure is seated on top of the lid (PM). Two types are shown: one showing a face more or less realistically modelled, with disk earplugs, and the head usually set in bird or animal jaws. The other has eyes made by an incised *pi* sign, a horizontally oblong pendant hanging from the septum of the nose, often round ball earplugs instead of the usual disk ones, and a headdress which fits in a square around the face and is more or less tubular above it. The latter type recalls in feeling, though there are no actual identities, large figures from Teotitlan del Camino (Danzel, pp. 61-62; cf. also Dieseldorff, 1926, fig. 178). Some of the brasero figures of this type in the Museo Nacional of Mexico come from Tabasco, others from the Totonac area in Vera Cruz. Both types of men wear a costume of which the main characteristic is a jacket held together in front by a round or oblong brooch with two applied pellets on it. They carry shield and spear or atlatl. The main portion of the headdress is a sort of chimney acting as vent for the incense smoke. On the front of this is usually a bird flying down; behind the chimney is a semi-circular crest. On the crest is applied a series of hands, with two round pellets applied to the back of each.

The East Coast censer consist of a lidless jar with a standing man attached to the side (PM; Gann, 1918, pp. 119-20; pl. 20). The style is realistic, well-modelled; the types shown are two: an old man with head thrust forward, two snag teeth, and sunken cheeks, and a young man with a com-

plete upper set of teeth and an unlined face. The costume consists of disk earplugs, often with a tassel, a round collar with scallops projecting at intervals (cf. Piedras Negras, Stela 35, Lintel 2; Maler, 1901, pls. 28, 31), a braided oblong pectoral, fringed leggings, and a tall headdress with corn or braided symbols. We have the young man also in the diving position characteristic of the East Coast culture, with a plate of copal balls between his hands.

In the Highlands there are a few examples of a third kind of censer, consisting of a seated figure holding in his arms a straight-sided bowl for incense (MAI;UM).

In this region we also find the most varied development of censers in which the human body is subordinated to the vessel. Hourglass censers often bear on one side a human head, with its body telescoped into down-hanging arms and legs (UM). In another type a face with cruller twist over the nose, possibly the Sun God, forms one side of the jars and the low, straight-sided bowls (UM, PM, MAI). A conventionalized head and out-curving arms mask a cylindrical jar on cylindrical tripod feet (MAI). In even such a brief view of the fragmentary brasero material, we see that there are various distinctive treatments which at present seem to be localized in different parts of the Maya area. What evidence there is suggests a comparatively late date for the material discussed under this heading.

#### MOULDMADE FIGURINES

Mouldmade figurines have an average height of fifteen to twenty centimetres, though larger and smaller ones occur. They are made of relatively well-fired clay, which varies in quality, and ranges in post-fired coloring from buff to orange. They were probably originally painted, some realistically, some arbitrarily, but few traces of this remain today. The majority probably represent cult devotees, a few definitely portray deities. The distinction which exists between genre and cult figurines cannot be brought out in a paper of this sort, since genre figurines are usually single isolated specimens, rather than types. We can say, however, that such figurines, showing unmistakable scenes from daily life, occur in every region.

We do not yet know definitely the purposes for which clay figurines were used by the early Maya. A great many of them have whistle forms, as a result of placing vents in the back and adding a mouthpiece at the lower back of the figure. This may well have developed incidentally from the technique of making figurines in a mould, and does not necessarily mean that the figurines were primarily made to be used as whistles. In the Usumacinta-Highland region figurines occur that are pierced from side to side through head or shoulders, for suspension, probably as an amulet or



an offering at a shrine (PM, UM). With the exception of a broken figurine found on the lid of a cyst burial at Palenque (Holmes, 1895-97, pl. 23) and those found with skeletons in the Uloa Valley (Popenoe, p. 74) clay figurines so far have not appeared in burials or votive caches in the Maya area, although tiny jade and shell ones are frequent enough (J.E. Thompson, 1931, pl. 31, Piedras Negras Preliminary Reports, ms.). Discarded figurines were used, with potsherds, as a foundation for stucco relief decoration on the buildings of the Usumacinta region (Piedras Negras Preliminary Reports, ms.). What evidence we have, then, points to their use as whistles, probably in religious ceremonies (cf. Sahagun, p. 121), as amulets to be worn, or offerings to be hung. They may also have served as household gods; they do not seem to have been used in burials.

Figurines that can be identified as Mayan are found in the modern Mexican states of Vera Cruz, Chiapas, Tabasco, Campeche, and Yucatan, as well as in British Honduras, Guatemala, Honduras, and Salvador.

If we attempt to date mouldmade figurines approximately, on the basis of their occurrence in archaeological excavations, their earliest known appearance would seem to be at Piedras Negras, an Old Empire city in the Usumacinta Valley, where dated monuments range from 514 A.D. to 830 A.D. according to the Goodman-Thompson correlation, 260 years earlier by the Spinden correlation. Their latest appearance is at Labná, a Yucatecan city belonging architecturally to the Puk-Labná period, approximately the beginning of the second millenium A.D. The fact that, though frequent in the Puk, such figurines have not been found at later sites in Yucatan suggests that the use of small clay figures may have been to some extent replaced by the use of stone or wooden ones (cf. Landa, pp. 128, 158).

In attempting a classification of these clay figurines, style is taken as the primary criterion, with type of person shown, the subject matter, as secondary criterion. There is a certain amount of localization in each, but style seems to be the surer historical determinant.

A factor which must be mentioned before discussing the material is that of headform. There are three distinct headforms shown in the figurines belonging to a style which we shall refer to as X and in the major art forms of painting and carving. Style X is a realistic, finely executed rendering in the round of the human body that is in distinct contrast to the conventionalizations of surrounding styles. Headform designations are applied tentatively to figurines of other styles, allowing for the possible influence of conventionalization. These headforms will be referred to in the text as A, B, and C.

Headform A (fig. 1, a) has an oval face, narrowing from the jaw to the flattened forehead, which slopes back from the eyebrows at an acute angle.

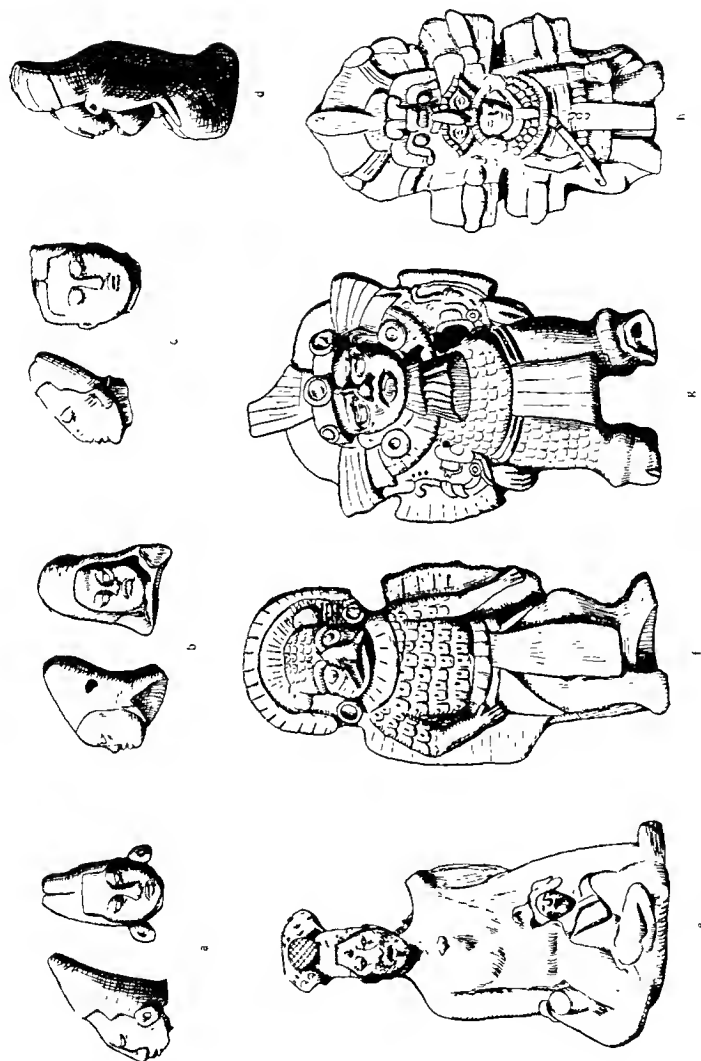


FIG. 1 a, X, headform A, Piedras Negras, UM, Cat. No. L. 16-968, b, X, headform B, Piedras Negras, UM, Cat. No. L-28-121; c, X, head form C, Piedras Negras, UM, Cat. No. L-28-127; d, X 1, Jonuta, PM, Cat. No. C-2600; e, X 2, Compeche, Tozzer, 1927, pl. 4; f, X 3a, Palenque, MAI, Cat. No. 7574; g, X 3b, Lago de Catemaco, Ver., MM, Fig. No. 18; h, X 3c, Yucatan?, Merida Museum. (a, b, c, features somewhat restored.)

This slope is usually prolonged, sometimes at a slightly altered angle, by a high headdress or wig, of which the front and back surfaces are wider than the sides. The chin is rounded and slopes back from the lower lip, and the projecting mouth is half open. There are two groups of figurines with Form A heads that are short and rounded. One comprises a few simply dressed people without headdresses, in Style X from the Highlands (Dieseldorff, 1926, fig. 24; Seler, 1904, figs. 18: c,f, 19: b) and in the Tabascan style (Spinden, 1913, pl. 17: 4). The other consist of Style X heads from Labná, which have low, close-fitting headdresses (PM). Headform A is that commonly referred to as the Maya type or Palenque type, and corresponds to Joyce's "classical" category (Joyce, 1933, p. xvi). It is predominant in stone carvings and in codices, as well as in figurines, from Guatemala to Yucatan.

Headform B (fig. 1, b) has a square, chubby face with spreading, flattish nose, below a normal or a bulging forehead. The head is usually short and rounded. It corresponds to that classified by Joyce as "chubby" (Joyce, 1933, p. xvi). Only in the Usumacinta region do we find it prolonged into a rounded cone sloping back above the prominent forehead. This conical shape may be due to a headdress similar to one characteristic of Form A heads. Form B is infrequent, associated at present chiefly with two more or less widely distributed figurine types, both showing stout elderly men. It occurs, as well, in Copan sculptures, and is essentially the type recognized in recent publications by Vaillant and tentatively assigned by him to the Olmecs.

The face of headform C (fig. 1, c) is roughly square, widest at the cheekbones, and sweeping down in a full curve to a pronounced, almost prognathous chin. The head is short and rounded, the face broad and flat, and the forehead perhaps artificially depressed. Form C is rare in figurines of the realistic style, occurring usually on single specimens not mentioned here, but its occurrence may be significant. A figurine with such a head from the Highlands is similar to one from Piedras Negras; one stela, 35 (Maler, 1901, pl. 28), among all the carvings of people with A heads at Piedras Negras, shows a person with a Form C head.

Our knowledge of the Maya is still too fragmentary to allow of sure conclusions as to the significance of this variation in headform, but it is a factor that cannot be ignored in considering Maya figurines. Whether a figurine head has the form A, B, or C will be shown by the corresponding initial in the heading under which it occurs.

These figurines, then, will be considered by styles, discussing under each the main types, with mention of individual specimens that may be of in-

terest in their relation to other forms of art. Taking this approach, we find two general styles occurring throughout the Maya area, not confined to any one district. One of these, called here Style X, is that most characteristic of and most common in Maya mouldmade figurines. The other, referred to as Style Y, occurs sporadically. In contrast to these are styles that are apparently local in character: one belonging to the Highlands, three others, from Campeche, Tabasco, and Vera Cruz to the coast of the Gulf of Mexico, and another, from the Uloa Valley to the Caribbean coast.

#### GENERAL STYLES: STYLE X

Figurines in this realistic style, already described on page 641, show a highly developed artistic sense and technique, and are the finest from pre-Columbian America. This style shows the same approach to and treatment of the subject as are shown in the bas-reliefs and painting of the Old Empire. It is characteristic of figurines from the middle Usumacinta drainage, of the handful of mouldmade figurines from the Peten, and of most of those from the Highlands. It reaches as far as Copan, and Lubaantun in British Honduras; it is prominent in Tabasco, and persists along the west coast of Yucatan into the Peninsula itself where it characterizes the heads found at Labná in the Puk. A refinement of study that will be possible only with the accumulation of well-documented collections of figurines from the entire area may show local variations of treatment within this widespread style. At present, there is a certain amount of localization of types and of costume which permits some geographical distinctions to be drawn. The style persists in time from Piedras Negras to Labná. The implication is that it is most characteristic of the Maya Old Empire, and continued into the Maya period of power in Yucatan.

#### USUMACINTA

*X 1. Hunchback. A* (fig. 1, d). The figure of a seated hunchback shows a man wearing loincloth, neckbar, and high caplike headdress. A specimen in gray clay from Jonuta varies somewhat from those in red-orange clay from Piedras Negras. The hunchback is found figured in clay and stone from Tennessee to Costa Rica. Bernal Diaz refers to hunchbacks acting as buffoons (Diaz del Castillo, I, p. 230) and Landa mentions the popularity of jesters and buffoons among the Maya. One wonders whether the Maya, like ourselves, believed that a hunchback brought good luck.

*X 2. Woman with Adult child. A.* This is a woman with a child whose face is that of a wrinkled old man (fig. 1, e). She wears a high, square-ended headdress, with an incised line like a central parting; the headdress is straight across the forehead, and falls away in steps at the sides of the

face. She may be standing, wearing only a skirt, carrying a dog under her arm, and leading the smaller figure, or seated, in an undecorated, low-necked robe, with the smaller figure across her knees. Although these two persons may represent merely a mother and child, the wrinkled face of the latter suggests the religious connotation of goddess and worshipper.

Thompson refers to similar figures from Lubaantun, which are as yet unpublished (Joyce, Clark, and Thompson, p. 310). A woman with a smaller figure at her knee occurs also in two poses in the characteristic style of Campeche; in the Highlands of Guatemala, and in the Uloa Valley; among the Chiriqui, in Nicaragua and Venezuela to the south: in the "Archaic" of Vera Cruz and the Huastec, and among the Aztecs to the north. The only specimens, however, that suggest goddess and worshipper are those in the Maya Usumacinta area. This may be because they are the only ones executed in a style realistic enough to show the adult characteristics of the minor figure. But it is interesting to recall the reliefs on the Palace at Palenque that show a woman holding in her arms a child whose right leg ends in a snake's head. Spinden (1913, p. 51) calls this child a prototype of the Manikin Sceptre God, probably a form of God B-K of the codices, and the reliefs may perhaps point, from another angle, to a Mother Goddess cult in this region.

*X 3. Owl Man.* There is a definite owl cult which seems to extend from the Usumacinta into Vera Cruz, and in the latter region shows connotations of the Serpent Bird, that mythical creature, found on Old Empire stelae and lintels, who is probably connected with the Plumed Serpent (Spinden, 1913, pp. 60-62). One sub-type shows a man in a complete bird costume, of which the salient feature is a Horned Owl mask headdress, X 3a (fig. 1, f). One of these figurines, from San Andres Tuxtla, has along each arm a snake head without lower jaw. The figure resembles in everything but headdress that shown in Figure 1, g. According to Maudslay, as quoted by Spinden (1913, p. 60), "the most essential character of the (Serpent Bird) design seems to be the presence of a conventional snake's head (without a lower jaw) in place of or overlying the bony structure of the bird's wing."

Another sub-type shows a man wearing a bird costume, but no mask, X 3b. While the costume shown bears no especial owl attributes, it is identical with that worn by men with owl masks. It does not appear with any other bird head, although it occurs once topped by a jaguar mask (MM). It seems reasonable therefore to consider it at present as an owl costume, especially as one such man, from Vera Cruz, has a snake head along each arm (fig. 1, g) like the owl figure described above.

Three standing men have, each, a nimbus of feathers surrounding the whole figure and an owl mask used as the main element in the tiered headdress, X 3c. One of them wears a headdress in which a snake headdress is superimposed on an owl one (fig. 1, h). This points again to the Serpent Bird combination, and the costume as a whole suggests that the figurines, of unknown provenience, are Old Empire ones.

Three other men have vertical flanges along their arms that can only be meant to be wings, X 3d. One (fig. 2, a) is seated wearing a headdress that seems to have developed from an owl head (see below), the others are standing. While these may represent some other form of birdman than the owl, this bird is the only one that can be identified in anthropomorphic form among these figurines.

Mention should also be made of two headdresses containing bird elements which seem to derive from the owl. One of these, which apparently shows a conventionalized owl beak and large filleted eyes, occurs from Palenque to Yucatan (fig. 3, c). It is very like a conventionalization of a snake head that is found on jades (PM) and on Piedras Negras stone carvings (Maler, 1901, pls. 20, 22). The bird head, however, can be distinguished by minor differences in the nose element, and by the three sets of feathers that project at top and sides, representing tail and wings. This headdress also occurs with abbreviations (fig. 2, a) and variations (fig. 2, b), and in the Campeche style and the Vera Cruz "Archaic," as well as Style X.

The other headdress which may have the same source consists of a balloon-like turban above two filleted eyes and a beak element (Joyce, 1933, pl. 2: 9, 10, 13). It also occurs on Lintel 4 at Piedras Negras (Maler, 1901, pl. 32) and probably again in the bulbous turbans on polychrome pottery from the Highlands (Gordon, 1925, pls. 2, 8).

*X 4. Nude Fat Man. B.* This is a fat man, usually standing, whose hands rest at the sides of his paunch. He wears only a loincloth, and usually has a large bead or plaque applied to his chest, and a turban or pointed cap on his head. One wears a bird headdress (fig. 2, b).

A similar standing figure has been found in jades from the Sacred Cenote at Chichen Itza (PM), and the type has been recognized by Beyer (1930) among Toltec figurines and paintings. Standing fat men from Teotihuacan are very like the Maya ones. Their loincloths are of a different cut, and one man wears long ear ornaments of the kind that Beyer finds characteristic of his Fat God among Toltecs and Totonacs. Their headdress consists of two filleted rings on the forehead, a vestigial beak above, and a crest flaring off to the side (Seler, 1915, p. 461, fig. 52), and seems to be a variant of the owl headdress described under X 3. Two such filleted



FIG. 2. a, X 3d, Guatemala, UM, Cat. No. NA 10979; b, X 4, Tabasco, MAI, Cat. No. 4/1158; c, X 5, Lubaantun, Joyce, Clark, and Thompson, pl. 18 2; d, X 6, Lubaantun, Gann, 1925, p. 222; also drawn, Joyce, 1933, pl. 6: 5; e, British Honduras headdress, Lubaantun, Joyce, Clark, and Thompson, pl. 20: 1; also shown, Joyce, 1933, pl. 1: 1, 4, 5, 8; pl. 10 2. f, X 7, Lubaantun, Joyce, Clark, and Thompson, pl. 20; also drawn, Joyce, 1933, pl. 6. 8; g, X 8, Coban, Dieseldorff, 1926, fig. 30; h, X 9, Mexico, UM, Cat. No. 15080; i, X 10, Chamá Dieseldorff, 1926, fig. 49. The drawings in Joyce's article, made with the originals at hand, show more detail, notably a baby on the back of the kneeling woman of X 7.

rings on the forehead are noted by Seler as characteristic of the puffy-cheeked heads, similar to Form B, often with eyes closed, that he classes together (Seler, 1915, pp. 510, 516, 520). The Teotihuacan figures of standing fat men are the only ones of this class that do not have a Form B face, square and chubby. Theirs are typically Toltec—a triangle with a rather bulging forehead as base. Such a face, older, fat and sagging, might assume a square outline, with bulging cheeks and forehead.

One Maya headdress, more Mexican ones, suggest some connection of the Nude Fat Man with the owl cult.

#### BRITISH HONDURAS

X 5. *Ball Player*. A type so far unique at Lubaantun is that identified by Joyce as the ball-player (fig. 2, c).

X 6. *Man-in-Litter*. A man is carried, apparently in a hammock slung between two poles, on plaques from British Honduras (fig. 2, d). The same thing is shown on a polychrome vase from Chama (UM). A similar graffito on a wall at Holmul shows a different type of litter (Merwin and Vaillant, p. 90).

X 7. *Woman-at-Metate*. This is a woman, wearing a headdress typical of the district, kneeling at a metate (fig. 2, f).

#### HIGHLANDS

X 8. *Dancer*. A. This is a man wearing necklace, loincloth, and high mask headdress with feathers flaring at top and sides (fig. 2, g). His headdress has snake or corn attributes, and is probably associated with fertility rites. He may be seated, with arms gracefully outstretched, or standing in a dancing posture that recalls the carved stelae of Naranjo, Cankuen, Quirigua. This dancing pose also occurs on one of the Owl Men (fig. 1, f), some of the Clothed Fat Men, X 10, a man from Jaina (Spinden, 1913, pl. 17: 11) and a figurine in the Vera Cruz style (MM).

#### VERA CRUZ

X 9. *Winged Man*. C. This is a type that is difficult to identify, a man, standing, with outstretched arms, shoulder wings, and a thick tubular collar or roll of fat at his neck. Several specimens show the man to be fat; two wear a definite feather costume. It is impossible to say whether there is any connection between these figures and the Clothed Fat Man, X 10. The general effect of the specimen shown in Figure 2, h, suggests an insect.

#### GENERAL

X 10. *Clothed Fat Man*. B, A. The chief characteristic of this type is the fat man's costume of a fitted dress with a loincloth over it (fig. 2, i).



The material of the jacket and trousers is shown by parallel horizontal lines with vertical nicks between them. Although this might be a conventionalized rendering of the feather costume of the Owl Man, it might also show another use of the quilted cotton clothing that the later Maya and Mexicans used for armor (Diaz del Castillo, I, p. 5). Headdresses are usually low caps of one form or another, although one man has fillets and feather that recall the Mexican bird headdress described under the heading X 4 (MM).

Most of these men have Form B heads. They are usually standing; sometimes in a dancing pose, sometimes holding a fan, sometimes with the right hand held to the face. There are two occurrences of this type in other styles (PM); once in that characteristic of Campeche, once in the Tabascan style. The occurrence of one of these figures at Lubaantun presumably establishes the type in the Old Empire; identification of the Form A head of a specimen from Merida with several heads made from a Labná mould suggests that the figurine came from that city, and that the type persisted into the second millenium. There are, however, differences in dress or equipment that distinguish the few Yucatecan examples. A similar figure, apparently bound to a post, appears carved in stone at Descilná, Yucatan (Follett, p. 395).

*X 11. Seated Woman. A.* A woman seated apparently with legs crossed under her, has a high, fairly simple headdressing (fig. 3, a). She wears a necklace, a pik or long skirt, and a huipil or short-sleeved blouse, that may or may not cover the shoulders. Her left hand rests on her knee, and in her right she usually holds a fan, sometimes a dog or a child. Sometimes she is old and wrinkled, once she has a pointed dog's face. One specimen supports her raised left elbow by her right hand.

Headless women from the Usumacinta district sit with hands in their laps, wearing low-necked robes whose long sleeves fall away in points from the wrists. One of these, from Piedras Negras, has a Form A head, another, from Campeche (?), has a large, bald head with wrinkled face.

*X 12. Cross-legged, hands on knees. A.* There are quite a number of Maya figurines that show a person seated in this position. These fall into various groups. One is determined by a peculiar technical feature, X 12a. The line from neck to hand is so curved that what bend there is comes in the upper arm rather than at the shoulder. This causes the stiff arms to stand away from the body. Three women wear necklaces of large beads, and capes low off the shoulder, similar to a cape found on Zapotecan braseros (fig. 3, b). A man, wearing a loincloth that wraps high around his waist, has the same pose and arm technique.

Another group, X 12b, consists of men seated in the same way, but with elbows bent. Those from the Usumacinta wear capes or bead collars and,



FIG. 3. a, X 11, Salinas, Dieseldorff, 1926, fig. 38, b, X 12a, Hama Campeche, Chis., Gann, 1926, p. 242, c, X 12b, Palenque, MAI, Cat. No. 7621; d, Sun God?, Chajcar, Dieseldorff, 1926, fig. 174; e, Diving God, Piedras Negras, UM, Cat. No. L-39-145, f, X 13b, Camela, PM, Cat. No. C-5152; g, X 13c, Progreso, Yuc., AM, Cat. No. 30 0-1930; h, Group, Isla del Carmen, Camp, MM, Cat. No. 3-74; i, Group, Isla del Carmen, Camp, MM, Cat. No. 3-73.

usually, some variant of the owl headdress (fig. 3, c). Those from the Izabal district show older men, simply dressed.

*X 13. Whistles.* Many Maya figurines are whistles, with a mouthpiece inconspicuously attached to the back of the figure. There are, however, three specialized whistle forms associated with Style X. One of these is a human head with the mouthpiece set vertically in the top, X 13a. In the others, the representational element is subordinated to the functional. One, X 13b, is roughly cylindrical, bulging at either end, and with one side moulded into a human face and torso (fig. 3, f). The mouthpiece is set in the end, above the head. The other kind illustrates a type to which many of the figurine heads from the Puk probably belong, X 13c (E. H. Thompson, pl. 12: 18). Its ultimate source of inspiration may have been the pan-pipe, since it has two pipes of different lengths, closed at the lower end. A human head is moulded on the side below a flattened mouthpiece (fig. 3, g).

There are occasional personages, and animals, usually anthropomorphic, that may represent gods. One is a tiny, finely-done Diving God, with Form A head, and arms and legs broken off (fig. 3, e). This comes from Piedras Negras, and serves to show that the concept of the descending god, associated with Mexican and late Mayan cult objects, such as *braseros* from the east coast of Yucatan, belonged also to the Old Empire. Bizarre heads may represent the Sun God (fig. 3, d). Clothed humans with animal heads include, beside the Owl Man, X 3, dog-, jaguar-, and deer-headed persons, and a possible monkey-man. I would suggest that these show masked people representing gods in religious ceremonies, while unclothed jaguar-people, in various poses—one holding a small struggling human figure (PM)—represent the god himself. This suggestion would be supported by Stela 10 at Piedras Negras (Maler, 1901, pl. 19), where a priest or ruler is seated before a huge standing jaguar, who stretches protecting arms above him.<sup>2</sup> The Jaguar, lord of the Maya month Pop, is important also in Mexico and Peru. Tozzer and Allen, 1910, point out the general religious importance of the dog, its possible connection with Mars, and the importance of the owl.

There are four single specimens, figurine groups, that should be mentioned for their connection with stone reliefs from Piedras Negras. One of these, from Lubaantun, shows a man holding by the hair two smaller figures, one on either side of him (Joyce, 1933, pl. 5: 1). Such grouping oc-

<sup>2</sup> While Maler (p. 56) says that the carving on the upper half of Stela 10 is destroyed completely, the author when at Piedras Negras in 1933 was able to make a drawing of the stone which shows clearly the jaguar head and outstretched arms of the large standing figure whose legs are shown by Maler's photograph of the lower half of the stela

curs on Stela 4 at Piedras Negras (Maler, 1901, pl. 14). Another shows a man seated in a niche, to which he has mounted by a short flight of steps (fig. 3, i). He wears a headdress showing a snake snout with the three-member bird elements behind it. On top of the niche leans a god with a snake head behind each elbow, and what seems to be a dumpy bird above his head. This is in many points similar to Stelae 6, 11, 14, 25, 33 (Maler, 1901, pls. 15, 20, 22, 26), which show figures associated with the Serpent Bird and Two-headed Dragon, seated in niches reached by steps. Another figurine group shows a man seated on a flat table with inscribed oblong legs that are narrower at the bottom than at the top (fig. 3, h). Below this dais sit two attendants, stressing the resemblance to Lintel 3 (University Museum Bulletin). The fourth figurine group, showing the Jaguar God protecting a king or priest and his two attendants (fig. 4, a), presents the same idea as Stela 10, in slightly different form.

#### STYLE Y

Style Y is a crude, bold treatment of figures in the round that pays little attention to detail. The figurines grouped under this heading have a certain unity of feeling, and a superficial resemblance to Mexican figurines. Style Y does not, however, have the homogeneity of Style X. It is one of those expedients to which a classificatory system must resort. From the differences apparent in the illustrations in Figure 4, one would be tempted to group the figurines of Y 1a and Y 2a into a Jonuta style, those of Y 1c and Y 2b into a Peninsular style, the half dozen red-orange heads from Piedras Negras (fig. 4, g) into a Piedras Negras style. The scarcity of specimens and their diversity of provenience makes this unwise. It seems better to leave them at present in a group bound together by the characteristics mentioned, and by their distinction from other Maya figurines.

Style Y occurs in the Highlands, along the Usumacinta, in Vera Cruz, and the Yucatecan peninsula. We have no data adequate for dating it, although the appearance at Piedras Negras of the heads mentioned above places them in the Old Empire.

*Y 1. Hands at sides.* A woman with hair parted in the middle and hanging on her shoulders wears ear disks and a skirt, or a skirt and short triangular cape (fig. 4, b), Y 1a. This simple hairdressing and costume occurs on a few figurines of Style X, one of them a girl carrying water (Dieseldorff, 1926, fig. 24; Seler, 1904, figs. 18: c,<sup>3</sup> f; 21: b), and on one of the Tabascan style (Spinden, 1913, pl. 17: 4), and probably denotes a slave or peasant. There is

<sup>3</sup> Also shown by Spinden 1913, fig. 200.

a group of men, Y 1b, who wear usually only a loincloth and ear ornaments (fig. 4, c). Two of the specimens from San Andres Tuxtla, however, show a

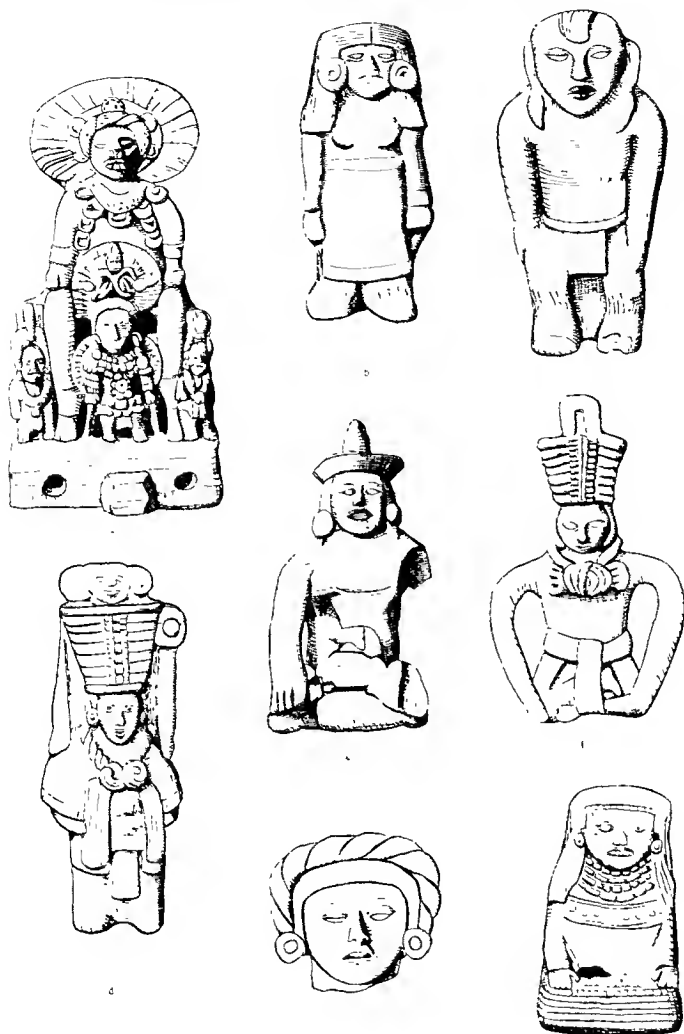


FIG. 4 a, Group, Jaina<sup>2</sup>, James Collection; also shown, Spinden, 1913, pl. 17-10; b, Y 1a, Palenque, MAM, Cat. No. 7633; c, Y 1b, El Meco, Yuc., MM, Fig. No. 88, d, Y 1c, Yucatan, MM, Fig. No. 31; e, Y 2a, Jonuta, MM, Fig. No. 68; f, Y 2b, Campeche, PM, Cat. No. C-5213, g, Head, Piedras Negras, UM, Cat. No. L-17-380; h, H 1, Coban, Dieseldorff, 1920, fig. 65 (fig. 33)

man in what seems to be protective armor, with a close-fitting, open-faced helmet. There are others, Y 1c, who have a unique kind of high headdress, and a short cape, usually of feathers, fastened by a bulging breastknot (fig. 4, d).

Y 2. *Cross-legged, hands on knees.* Two people wearing little hats with pointed crowns are so seated, with arms stiff, Y 2a (fig. 4, e). Others, Y 2b (fig. 4, f), wearing a costume like that of Y 1c, but without the cape, have arms bent out above the elbow in the same manner as those of X 12a (fig. 3, b).

#### LOCAL STYLES

##### HIGHLAND STYLE

This is a highly conventionalized style, which produces very crude figures of which the lower part is absorbed into a columnar base. Although the figures seem mouldmade, they give the impression of having been described by crude incising, and probably derive from a stone prototype.

H 1. *Seated Woman. C.* They show so far only one type, a woman, probably seated with knees crossed (fig. 4, h). She has a round, undecorated head, and wears huipil, pik, and necklace. Sometimes she holds a dog, sometimes possibly a child.

##### GULF COAST STYLES

Three styles along the coast of the Gulf of Mexico, from Campeche, Tabasco, and Vera Cruz respectively, are tied together by a common type, a figure standing with hands raised to the shoulder.

##### CAMPECHE STYLE

The figure, of red clay, slipped with white, is squat and heavy, with the lower part of the body often rendered as a block, ending in a flat base, the back is merely a slightly convex, smoothed piece of clay. The face is broad, flat, expressionless, conforming to the type described under Headform C, and the headdress is usually low and wide, the central hair parting often shown by a shallow groove. The impression given is that the emphasis of the artist has been on fitting a figure into a preconceived oblong form rather than on faithfully representing a human being.

C 1. *Hands raised.* Figures standing with hands raised include men and women. Their costumes consist of necklace and short skirt, fitting close around the waist, or long skirt and cape with a rounded point that is very like the women's costume from Oaxaca (fig. 5, a). Textile design is indicated, but without the meticulous emphasis found in Vera Cruz.

C 2. *Hands down.* Another pose shows the arms downstretched, elbows

slightly bent, and arms resting against the body (fig. 5, b). One of these figures has a smaller one at the level of the knees. Two others wear capes quite plain except for an inverted triangle in the front containing a head,



FIG. 5 a, C 1, Yucatan, Dieseldorff, 1926, fig. 50; b, C 2, Fuhrmann, pl. 59; c, T 1, Fuhrmann, pl. 60; d, T 2, Tecolpa, MM, Fig. No. 50; e, T 3, MM, Fig. No. 5; f, VC 2, Dicha Tuerta, Ver., MM, Fig. No. 79; g, VC 1, Cerro de las Mesas, Strebel, 1889, pl. 32:45.

apparently that of the Sun God. A similar costume, showing a Tlaloc head, occurs on two (?)<sup>4</sup> armless, possibly broken, figurines in this style. This type of costume, then, may belong to persons dedicated to various cults.

#### TABASCO STYLE

This seems to be a modification of the Campeche style from contact with Style X. The figure, though rendered as in Campeche, is not so squat and heavy. The head is that of Form A, usually with a high squared headdress, hair being indicated by widely-spaced, incised lines, a tied headdress by broad fillets of clay. Tabascan figurines tend to be of red-orange clay.

*T 1. Hands raised. A.* The most frequent type shows a woman standing with hands raised to shoulder level, forearms close to the sides (fig. 5, c). Her costume is simple and stylized, usually a long skirt and a cape with deep rounded point; textile patterns are shown only in the border. She often has a trefoil applied to her forehead, and tattooing around mouth and chin. Such facial decoration is seen again on a robed suppliant with stepped hairdressing on Lintel 26 at Yaxchilan (Maler, 1903, pl. 58).

*T 2. Standing Woman. A.* This shows a standing woman with hands resting at her waist, wearing a poncho-like upper garment so arranged as to hang to the waist in front, to the skirt hem behind, falling like long sleeves over the forearm (fig. 5, d). The textile design on one of these garments, is, as we see from stone reliefs, one very characteristic of the middle Usumacinta, and in strong contrast to the textiles on figurines from Vera Cruz. Where the latter show an unbroken surface of small elements, stylized, often derived from naturalistic motives, the former has only a large Maltese cross at intervals. A headdress unique with this type is an elaborate textile one with frills and tassels bulging in all directions. These women hold a bag, a fan, a disk with incised decorations. A simply dressed woman in Style X, probably from Jaina (JC), also holds with both hands a similar disk, which may represent a bunch of flowers. A large stone figure, similar to these Tabasco figurines, comes from near Xochicalco in the Valley of Mexico (Scler, 1888, p. 108, fig. 64).

*T 3. Hands turned out.* This shows a standing woman whose arms rest at her sides, with hands turned out (fig. 5, e). She wears a tunic bordered with a stepped design similar in derivation to that on the costume of T 2. She wears leggings, and a turban headdress reminiscent of one often found on figurines from the Uloa Valley (PM). A figurine in a similar pose in Style

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<sup>4</sup> The specimen shown by Dieseldorff, 1926, fig. 51, may be the same as that in the Peabody Museum. Also illustrated by Spinden, 1913, pl. 17. 12.



X comes from the banks of the Usumacinta (MM); another in the Vera Cruz style comes from Estanzuela, Vera Cruz (MM).

#### VERA CRUZ STYLE

The body is in the round, crudely rendered, but realistic, while the head has the characteristic Totonac face, broad, flat, and smiling, showing only two front teeth with their outer corners filed away.

*VC 1. Hands Raised. C.* The Vera Cruz hands-raised type often has the upper arm extended in a straight line from the shoulder, so that the bent elbows raise the hands to the level of the head; sometimes the arms are outstretched (fig. 5, g). Occasionally one hand holds a knife. The figures so far seem all to be those of men, wearing a narrow breast band with a skirt or loincloth, or a skirt and tight fitting jacket. Skirts are fairly long and set low about the hips; the textiles have elaborate stylized designs, geometric or snake-derived, painstakingly reproduced. Hairdressing is simple, and low

*VC 2. Hands down. C.* Some of these people stand, like those of Campeche, 2, with the arms downstretched, elbows slightly bent, and arms resting against the body (fig. 5, f).

#### CARIBBEAN STYLE

At the other extreme of the Maya area, we find the distinctive style of the Uloa Valley.

#### ULOA STYLE

The artistic and sophisticated "Archaic" figurines from here have already been considered. They probably preceded, and certainly excelled, the mouldmade style. The chief characteristics of the latter are the grooved eyes adopted from the "Archaic" technique, and the laying of emphasis on face and headdress, with a very summary treatment of the body. Faces are broad and flat; bodies usually nude; headdresses low and wide; and the whole feeling is bold, crude, and conventionalized.

*U 1. Arms bent. C.* The most frequent pose is standing, with hands on the abdomen (fig. 6, a). Five of these figures are unusually small, and show a person in a pointed cap with side flaps. The arms are sometimes greatly exaggerated.

*U 2. Second Face. C.* A standing figure with a second face on the abdomen shows a man with a mask on his girdle, or a mask pouch hanging from it (fig. 6, b).

*U 3. Seated Woman. C.* A seated woman with outstretched legs holds some object in her hand. This is sometimes a pot of copal, but usually is too

waterworn for recognition. Once she has a smaller figure at her knee (Spinden, 1913, pl. 17: 2).

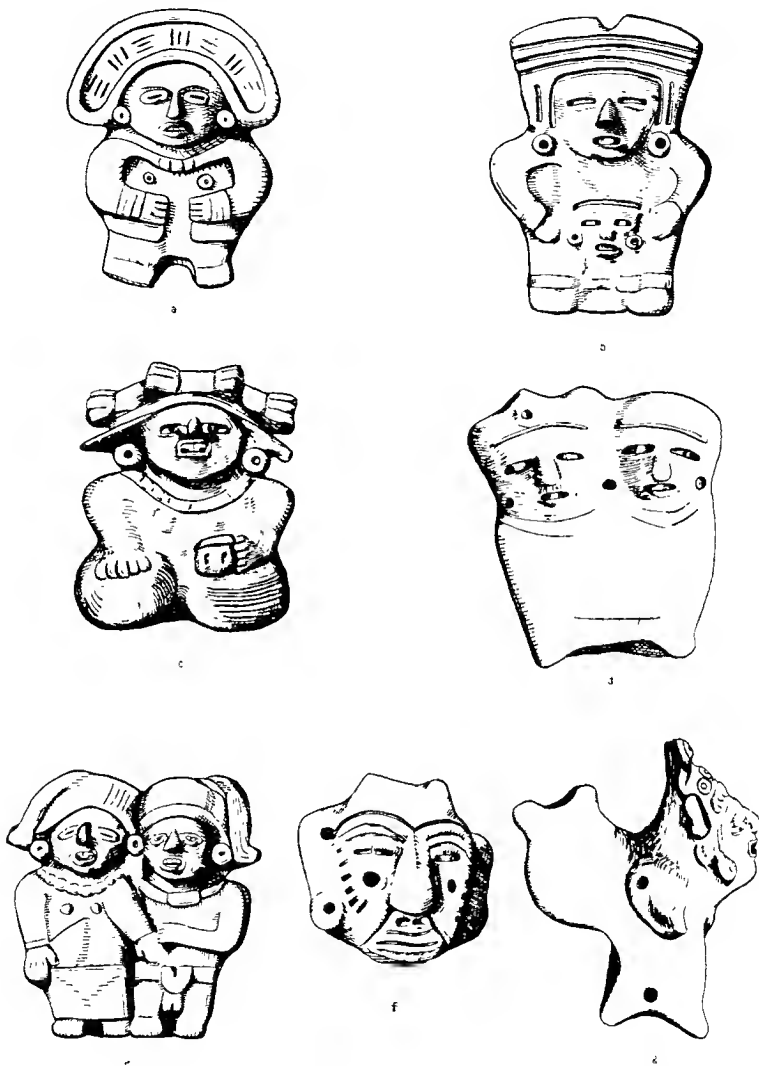


FIG 6 a, U 1, Uloa Valley, PM, Cat No. C 1760 (features somewhat restored); b, U 2, Uloa Valley, MAI, Cat No 18 3218; c, U 3, Uloa Valley, MAI, Cat. No. 4,4090; d, Double figure, Uloa Valley, PM, Cat No C 1292; e, Double figure, Uloa Valley, MAI, Cat. No 18 3201; f, U 4a, Whistle, Uloa Valley, MAI, Cat No. 16 4613; g, U 4b, "Porter" whistle, Uloa Valley, PM, Cat No C-10982

*U 4. Whistles.* There are in this style two specialized whistle forms. The first is an old man variant of the head whistle referred to under X 13a, U 4a (fig. 6, f). The other is what might be called the "porter" whistle, U 4b. It shows men with more or less elaborate heads, larger than their crude bodies. They bend forward under the weight of the large bundle on their backs, which is the whistle mouthpiece. A third leg holds the figures upright. The heads show old and young men, and vary in their degree of skill (fig. 6, g).

There are occasional sets of double figures that are of ethnographical interest. One may represent acrobats, since it shows a man holding on his shoulder another man upside down. Another shows a man and wife, with marked contrast between the two in costume, headdress, teeth, and eye technique (fig. 6, e). Another, like a plaque in Style X from the Highlands (Seler, 1908, pl. 1: 2), showing two young men standing side by side, may perhaps represent the Divine Twins of Quiché mythology.

#### CONCLUSIONS

We find, then, two general styles of mouldmade figurine in the Maya area, and five local ones. Style X can, from distribution and archaeological data, be associated in space and time with the Maya Old Empire, and from frequency can be considered characteristic of it. It persists into Yucatan and the second millenium A.D. Style Y is infrequent. It can be placed in space, along the Usumacinta, in Vera Cruz and the Peninsula, but it has no corresponding anchor in time.

The local styles along the Gulf Coast can be compared on the basis of the common type of standing figure with arms raised. In the Vera Cruz district this is modified by the addition of a characteristic Totonac head. In Tabasco it is modified, presumably by contact with Style X, and has a Form A head with high headdress. Only in Campeche do we find the type in a style showing apparently no outside influence. Most of the Campeche specimens of this type come from the island of Jaina, which was known as a centre of pilgrimage. This suggests an indigenous origin, in a cult centering in Jaina, for this Campeche style, and for the type, the two subsequently spreading together, with the style being modified in each place diffused to by the style predominant there. The implication, from the Tabascan style, and the Cycle Nine dates found in Jaina and southern Campeche, is that these styles also began in the Old Empire. We have no check on how long they lasted.

These styles may be said to fall into two groups; Style X and the Tabascan and Vera Cruz styles on one hand, Style Y and the Campeche

and Highland styles on the other. In the first group, Style X is taken as characteristic of the Usumacinta section of the Maya Old Empire, the other styles as its modification by contact with the second group. The influences represented by this second group may be Mexican, or may reflect some current or series of currents within the Maya area as yet unreported in other forms of art.

The unique character and geographical position of the Uloa Valley style suggests some people coming up from the south into a sphere of Maya influence.

Considering types irrespective of style, common American types represented in the Maya area are the mother-and-child, found from the Valley of Mexico to Venezuela, with a special religious implication in the middle Usumacinta area, and the hunchback, known from Tennessee to Costa Rica. There are far-flung similarities of pose and detail that may or may not be significant. For example, figurines can be found from the Valley of Mexico to Ecuador, showing a person holding one hand to the face. A very long vertical flange for the ear, with a hole at top and bottom occurs on figurines from Copan to Vera Cruz.

Types found in Mexico and the Maya area include the Diving God, anthropomorphic animals, and the nude Fat Man.

Types characteristic of the Maya area are the Owl Man, the clothed Fat Man, the person seated with feet tucked under him (X 11, X 12, Y2, H 1), the Dancer, the Ball-player, the persons standing with arms raised or downstretched with elbows bent.

While the dancer, in the type referred to under X 8, is confined to the Highlands, the standing pose there described occurs elsewhere, and its appearance on one of the Owl Men figures (fig. 1, f) emphasizes the ritual character of this type. The Owl Man is found along the lower Usumacinta and in Vera Cruz. Only Vera Cruz Owl Men have attributes of the Serpent Bird, although other figurines seem to show the same association of ideas. The association of a costume with possible owl derivation with the clothed Fat Man; the appearance on the Toltec nude Fat Man of a headdress that probably represents an owl; the possible connection between the clothed Fat Man and the Winged Man; the relationship of the clothed Fat Man and the nude Fat Man, the relation of the B head form to both, and their relation to Toltec figurines; these are problems that cannot be solved at present. The owl cult was of real importance among the Old Empire Maya; so was the Fat Man, a figurine type which was connected not only with the owl, but with the Valley of Mexico.

The persons standing with arms raised, or downstretched with elbows

bent, occur only along the coast of the Gulf of Mexico from Campeche to Vera Cruz; the Ball-player appears only at Lubaantun.

There is a low, broad headdress characterized by a horizontal roll of twisted fabric, which is found only in Style Y and the Campeche and Uloa styles, and is always associated with a Form C head (fig. 6, g).

In regard to headform, we can safely call Form A the characteristic Maya headform, through its constant use to represent these people in major and minor arts. It is found in every section of the Maya area. High headdresses characterize the heads from Old Empire sites; low headdresses those from Yucatan.

Heads of B form are not so frequent. They are found along the Usumacinta, in the Peten, British Honduras, and in the Highlands. If the suggested association of such a physical type with a northern tribe, such as the Olmecs, should be borne out by further excavations, its appearance among the Maya might have some historical significance. One of the two types to which most of the whole specimens with B heads belong, the nude Fat Man and the clothed Fat Man, seems to have northern affiliations.

Form C heads are more common on the peripheries of the Maya area than in its center. This headform occurs occasionally in Style X, notably in the Vera Cruz Winged Man, and characterizes the heads of Style Y and the Highland, Campeche, and Uloa styles. The suggestion that this headform represents actual physical characteristics, and is not, for the most part, due to conventionalization present in the styles cited, is supported by two facts. One is the appearance, in Style X in Tabasco (PM) and Lubaantun (Joyce, 1933, pl. 2), and in the Campeche style (Schellhas, pl. 17: 2) of a Form C head with the kind of woman's hairdressing where hair, parted in the middle and drawn down over the forehead, is piled in coils on top of the head. The other is the retention of the A headform in the Tabasco style, and of the Totonacan head in the Vera Cruz style.

Considering Maya mouldmade figurines geographically, we find that along that part of the Usumacinta, the middle stretch, where important Old Empire cities occur, all but about a dozen of the figurines are in Style X. Apparently unique here are a figurine type, the woman-with-adult-child, and two kinds of headdress. One is the simple stepped headdress often found on Form A heads (fig. 1, a); this occurs with any number of additions and variations throughout the Old Empire, but the plain, unadorned variety has been so far found only in this region. The other headdress is the sugarloaf variety of B head described on page 643.

Although there is a surprising lack of mouldmade figurines in the Peten, another centre of Old Empire culture, what specimens there are fit into the

Usumacinta types. Almost all the figurines from the Highlands are in Style X, there are, outside it, only a dozen specimens of a local style, and a few examples of Style Y. The Dancer type belongs to this region. Throughout it, and down to the mouth of the Usumacinta, Style X headdresses, usually on Form A heads, include turbans (figs. 1, d, 5, e), high-pointed hats (Dieseldorff, 1926, fig. 40), variations of the stepped headdress or hairdressing (figs. 3, e, 5, d), and what seems to be a tight, close hood, over a high head (fig. 1, b).

All the British Honduras specimens considered are from Lubaantun, and seem, from the photographs and drawings published, all to conform to Style X. There are three figurine types, and a kind of headdress, that are so far unique here. The types are the Ball-player, the Woman-at-the-metate, and the Man-in-a-litter, of which the last two are the only types of Maya figurines that can definitely be called genre, though single genre figurines occur elsewhere. The headdress is associated with high Form A heads, and seems to be a woman's (fig. 2, e).

In Tabasco and the lower Usumacinta we find Style X, Style Y, and a local style. Unique types are the Style Y woman with arms at her sides, and the Tabascan style woman who holds with both hands a probable bouquet. Figurines in all three styles from Jonuta, the site from which comes most of the archaeological material from this district, are usually made of an orange clay that is also characteristic of Jonuta pottery.

Following the Gulf coast west, we find in Vera Cruz the Winged Man and Serpent Bird figures in Style X, and the hands raised and elbows bent type in a local style. A pose unique here is a figure standing with arms outstretched to the sides.

Following the Gulf coast north, we find in Campeche a distinctive local style, probably centering in the island of Jaina, and some specimens of Style X. Style Y is represented by figures in high headdresses and bulging breast-knots. The technical characteristic shown in the arms of seated figures (X 12a, Y 2b) may belong to the southern Campeche district. Along the Gulf coast, we get the unique hands-raised and hands-down types, central hair parting shown by a shallow groove, and distinctive kinds of dress, of which a woman's costume of skirt and cape with rounded point (fig. 5, a, b, c) is related to south Mexican costume. In Yucatan, Form A heads are almost invariably short, with low headdresses and hairdressing. Many of those from Labná have, between the eyebrows, an object resembling a vertical bar with a shorter one run transversely through it (E. H. Thompson, pl. 12: figs. 6-9, 14) which occurs also on a large stucco head from Comalcalco (Blom and La Farge, 1926, fig. 94).

In the Uloa Valley, mouldmade figurines occur only in the local style. Unique here are the woman seated with legs outstretched before her, and the double figure, both of them types characteristic of countries to the south.

Two pieces of negative evidence apparent in a study of Maya figurines may be wiped out by further digging. At present they lead automatically to that ever-recurring question, "Where did the Maya come from?" The two points are the lack of figurines at sites where they would be most expected, and the lack of an intermediate style between the "Archaic" and Style X, characteristic of the Old Empire.

Holmul and Uxactun are the only two sites in the Peten where careful excavation has been undertaken. The first has no figurines, the second only "Archaic" ones which occur in an early stratum containing distinctive pottery. The pottery of the next period develops from the first, but shows the influence of Holmul.

"Archaic" figurines do not occur at Piedras Negras, the only site where systematic excavation has been attempted in the middle Usumacinta. Nor is there any evidence of early occupation of the site, corresponding to the earliest levels at Uxactun. We have from the first a very fine type of figurine that is obviously the product of a sophisticated, artistic culture, belonging to a people with the headform that we think of as typically Maya. It appears fullblown and grown, presenting a problem unparalleled in Mexico, where there is a fairly steady development from "Archaic" through Toltec to Aztec. Where are the roots of this type of figurine? At present we would say that they are not necessarily in the Maya "Archaic," since at Uxactun we get a continuous development from the "Archaic" that produces no mouldmade figurines. There must have been introductory stages to the Maya mouldmade figurine, but as yet we cannot trace them.

The evidence considered here bears out the distinction suggested by J. E. Thompson (1932) between the cities of the Peten and those of the Usumacinta, and the spread of influence from the Usumacinta-Chiapas district up the west coast into Yucatan. It also supports any correlation of the Christian and Maya calendars that shortens the span of Maya power, since styles and types characteristic of the Old Empire persist with so little change into Yucatan and into the second millennium.

#### APPENDIX I PROVENIENCE OF MOULDMADE FIGURINES, BY DISTRICTS

##### MIDDLE USUMACINTA

Piedras Negras*	Xupa
Yaxchilan	Zona Sala
Palenque*	Libertad

## APPENDIX I — (Continued)

HIGHLANDS-HIGH USMACINTA		TALASCO
Camela	Chipal*	Jonuta
Cancuen	Chihuatal*	Monte Cristo
Chama*	San Cristobal Verapaz	Hama Campeche
Chajcar	Jalpemech	Cintla
Coban*	Salinas	Tecolpa
Chixoy	Chisec	Frontera
Ratinlixul*	Sabalam	
Kixpek*	Izabal	VLRA CRUZ
Chacula	El Baul	Lago de Catemaco
	Chicuc	San Andres Tuxtla
PETEN		Cerro de las Mesas*
Nakum		Alvarado
Naranjo		Cocuile
Flores		Rio Papaloapam
BRITISH HONDURAS		CAMPECHE
Lubaantun*		Jaina
Tzimin Kax*		
HONDURAS-MOCTAGUA		YUCATAN
Copan*		Labná*
Quirigua*		Uxmal
		Chichen Itza*
HONDURAS-ULOA		Kichmook
		Hacienda Cozumel near Merida
Banks of rivers in the Uloa Valley		

\* Sites where excavations have been undertaken

## APPENDIX II

TYPE	REFERENCE	PROVINCE	NUMBER	TOTAL
X 1	Mexican Museum	Palenque	1	5
	University Museum	Piedras Negras	3	
	Peabody Museum	Jonuta, Tab.	1	
X 2 <i>Standing</i>	Blom, 1926, fig. 166	Zona Sala, Chis	1(2)	3(4)
	Gann, 1926, p. 242	Palenque	1	
	(NB. These may be the same specimen.)			
<i>Seated</i>	Fogg Museum*	Campeche	1	2
	Peabody Museum	Palenque	1	

\* Illustrated by Tozzer, 1927, pl. 4



TYPE	REFERENCE	PROVINCE	NUMBER	TOTAL
X 3a	Heye Museum	Palenque	1	9(10)
	Peabody Museum	Jonuta	1	
	Mexican Museum	Jonuta	3	
		Yucatan	1	
		San Andres Tuxtla	1(2)	
	Seler, 1915, pl. 49*	San Andres Tuxtla	2	3
	Mexican Museum	Jonuta	1	
		Riveras del Usumacinta	1	
		Lago de Catemaco, Ver.	1	3
	c	Merida Museum	Yucatan	
	James Collection	Yucatan	1	3
	d	University Museum	Guatemala	
X 4	Heye Museum	Tabasco	1	8
	Mexican Museum	Jonuta	1	
	Mexican Museum	Tenosique	1	
	American Museum	Jonuta	2	
		Tecolpa	1	
		Tabasco	2	
		Campeche	1	
	James Collection	Campeche <sup>2</sup>	1	
	Beyer, 1930, p. 83	Toniná, Chis.	{ Mexican Parallels	
	Seler, 1915, fig. 52, pl. 33	Teotihuacan		
	San Miguel Amantla			
X 5	Joyce, 1933, pl. 7: 1-22, 8: 1-5	Lubaantun	27	27
X 6	Joyce, 1933, pl. 6: 1, 5, 6, 10	Lubaantun	4	4
X 7	Joyce, 1933, pl. 6: 8, 9	Lubaantun	2	2
X 8	University Museum	Chamá	1	8
	Dieseldorff, 1926, figs. 11, 12, 13, 20, 30, †	Chajcar	3	
		Coban	3	
		Chicuc	1	
X 9	University Museum	Mexico	1	5
	Mexican Museum	San Andres Tuxtla	3	
		Jonuta	1	13
X 10	Peabody Museum	Camela	1	
		Nakum	1	
	American Museum	Tabasco	1	
	Heye Museum	Izabal	1	

\* Also shown by C. Seler-Sachs, pl. 7(3)

† Also illustrated by Seler, 1904, fig. 21: a, b

TYPE	REFERENCE	PROVINIENCI	NUMBER	TOTAL
	University Museum	Coban	1	
		Highlands of Guatemala	1	
	Mexican Museum	Tabasco	1	
	Gann, 1926, p. 228	Lubaantun	1	
	Dieseldorff, 1926 figs. 48, 49	Chisee	1	
		Chamá	1	
	Lehmann, p. 42	Yucatan	1	
	Schellhas, pl. 17: 5*	Yucatan	1	
	Rejil Collection	Yucatan	1	
X 11	Mexican Museum	Palenque	1	12 27
		Usumacinta	1	
		Jonuta	1	
	Peabody Museum	Camela	3	
		Nakum	1	
	University Museum	Coban	1	
		Highlands of Guatemala	1	
	Dieseldorff, 1926, figs. 38, 40, 41	Salinas	2	
		Chamá	1	
	<i>Grotesque and old:</i>			
	Dieseldorff, 1926, 42, 45, 46	Salinas	1	7
		Santa Cruz, unknown	2	
	Joyce, 1933, pl. 6 1-4	Lubaantun	4	
	<i>In long-sleeved robe:</i>			
	University Museum	Piedras Negras	7	8
	James Collection	Campeche?	1	
X 12a	Gann, 1926, p. 242	Hama Campeche, Chis.	1	4
	Schellhas, 1890, pl. 17: 3	Yucatan?	1	
	James Collection	Campeche?	1	
	Trocadero	Yucatan?	1	
b	Heye Museum	Palenque	1	8
		Izabal	1	
	Mexican Museum	Palenque	1	
		Jonuta	1	
		Riveras del Usumacinta	1	
		Yucatan	1	
	Private Collection	Quirigua	1	
	American Museum	Monte Cristo, Tab	1	
X 13a	University Museum	Coban	1	4
	American Museum	Jonuta	1	
	Mexican Museum		1	
	Vaillant, 1928, p. 567†	Usumacinta	1	

\* Also shown by Lehman, p. 43

† Also shown by Spinden, 1913, fig. 209

TYPE	REFERENCE	PROVENIENCIA	NUMBER	TOTAL
b	Peabody Museum	Camela	1	2
	Batres, 1908, pl. 49	Alvarado, Vera Cruz	1	
c	American Museum	Progreso, Yuc.	1	1
Sun God <sup>2</sup>	University Museum	Piedras Negras	1	6
	Dieseldorff, 1926, figs. 26, 174	Coban	1	
		Chajcar	1	
	Hewett, pl. 11	Quirigua	3	
Monkey?—Man	James Collection	Campeche	1	2
	Heye Museum	Palenque	1	
Jaguar-headed	Mexican Museum	Yucatan	1	4
	Peabody Museum	Tabasco	1	
	University Museum	Highlands of Guatemala	1	
	Dieseldorff, 1926, fig. 7	Chisec	1	
Dog-headed	Peabody Museum	Camela	1	4
	Dieseldorff, 1926, fig. 5	Chisec	1	
	Mexican Museum	Palenque; Yucatan <sup>2</sup>	2	
Deer-headed	Mexican Museum	Jonuta	1	1
Y 1a	Heye Museum	Palenque	1	8(9)
	Mexican Museum	Palenque	1	
		Jonuta	4(5)	
		Yucatan	1	
		San Andres Tuxtla	1	
b	Mexican Museum	Riveras del Usumacinta	2	6
		San Andres Tuxtla	3	
		El Meco, Yuc.	1	
c	Peabody Museum	Jaina	1	5
	Mexican Museum	Yucatan	1	
	James Collection	Jaina <sup>2</sup>	1	
	Schellhas, pl. 17: 4, 6	Yucatan	2	
Y 2a	Mexican Museum	Jonuta	2	2
b	Peabody Museum	Campeche	1	2
	Mexican Museum	Palenque	1	
H 1	University Museum	Chama	1	9
		Highlands of Guatemala	1	
	Gann, 1925, p. 88	Naranjo	1	
	Seler, 1908, pl. 4: 2*	Coban	2	
	Seler, 1915, pl. 49 †	San Andres Tuxtla	1	
	Dieseldorff, 1926, fig. 32; figs. 33, ‡ 34 §	Jalpemech	1	
		Coban	2	

\* *Alterthümer aus Guatemala* (Gesammelte Abhandlungen, 3, Berlin)

† Also shown by C. Seler-Sachs, pl. 10 (6), 32a.

‡ Also illustrated, Dieseldorff, 1926, fig. 65.

§ Also illustrated, Dieseldorff, 1926, fig. 44.

TYPE	REFERENCE	PROVENIENCE	NUMBER	TOTAL
C 1	Heye Museum	Tabasco	2	23
	American Museum	Jonuta	1	
		Campeche	1	
	Peabody Museum	Jaina, Campeche	5	
	Merida Museum	Jaina	1	
	Mexican Museum	Usumacinta	1	
		Yucatan	1	
	Rejil Collection	Campeche <sup>2</sup>	3	
	James Collection	Jaina <sup>2</sup>	2	
	Hamy, pl. 26: 81, 83	Jaina	2	
	Schellhas, pl. 17: 1, 2	Yucatan	2	
	Dieseldorff, 1926, fig. 50	Yucatan	1	
C 2	Peabody Museum	Jaina	1	5
	Merida Museum	Jaina	2	
	Fuhrmann, pl. 59		1	
	Batres, 1889b, pl. 7, 1	Teotihuacan	1	
T 1	Peabody Museum*	Jonuta	4	16
	Mexican Museum	Tabasco	1	
		Usumacinta	1	
		Yucatan	3	
	American Museum	Monte Cristo, Tab.	2	
		Jonuta	1	
	Heye Museum	Tabasco	1	
	Krickeberg, 1925, fig. 45	Yucatan	1	
	Fuhrmann, pl. 60		1	
	Batres, 1889a, pl. 1: 7		1	
T 2	Mexican Museum	Palenque	1	6
	Mexican Museum	Tecolpa, Tab.	2	
	Heye Museum	Tabasco	1	
	Fuhrmann, pl. 56		1	
	James Collection†	Jaina <sup>2</sup>	1	
T 3	Mexican Museum	Jonuta	2	3
		Riveras del Usumacinta	1	
VC 1	Strebel, 1889, pl. 32:	Cerro de las Mesas	7	14
	35, 39, 40, 41, 43,			
	44, 45			
	pl. 33: 16, 17	Cerro de las Mesas	2	
	Blom, 1926, fig. 13	Cocuile	1	
	Batres, 1908, pl. 50	Rio Papaloapam	1	

\* Also illustrated (one figurine), Spinden, 1913, pl. 17: 7

† Also illustrated, Spinden, 1913, pl. 17: 9.

TYPE	REFERENCE	PROVENIENCE	NUMBER	TOTAL
	Hamy, pl. 19: 60	La Estanzuela, Ver.	1	
	Mexican Museum	San Andres Tuxtla; un- known	2	
VC 2	Mexican Museum	Dicha Tuerta, Ver.	2	3
		Ocosingo, Chis.	1	
U 1	Peabody Museum*	Uloa Valley	19	29
	Heye Museum	Uloa Valley	10	
U 2	Heye Museum	Uloa Valley	3(5)	5(7)
	Peabody Museum	Uloa Valley	1	
	Gann, 1926, p. 198	Copan	1	
U 3	Heye Museum	Uloa Valley	7	8
	Peabody Museum†	Uloa Valley	1	
U 4a	Peabody Museum	Uloa Valley	1	2
	Heye Museum	Uloa Valley	1	
b	Peabody Museum	Uloa Valley	18	28
	Heye Museum	Uloa Valley	10	

\* One of these illustrated by Spinden, 1913, pl. 17. 1.

† Two of these illustrated by Spinden, 1913, pl. 17. 2. 3.

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## BOOK REVIEWS

### NORTH AND SOUTH AMERICA

*The Northern Algonquian Supreme Being.* JOHN M. COOPER. (Anthropological Series, Catholic University of America, No. 2. 78 pp. Washington: Catholic University of America, 1934.)

In this compact, meaty monograph Dr Cooper has presented evidence which establishes beyond doubt the aboriginality of the belief in a single Supreme Being by the native Cree and Montagnais of the James Bay region. Verbatim statements by fifteen natives are given, representing four bands west of the bay (Albany, Atawapiskat, Moose Factory, and Kesegami) and the Eastmain band on the east coast. The last is represented by the testimony of a single informant, obtained by Miss Regina Flannery. Data on the Davis Inlet and Barren Ground bands (Labrador Peninsula) supplied by Dr W. D. Strong, although somewhat divergent in detail from the James Bay information, are added to the field evidence.

The author's synthesis of the evidence includes the following points (1) There was but one Supreme Being recognized; (2) conceived as dwelling somewhere above, but with no trace of identification with, or relationship to, any heavenly body. (3) There was no mythological elaboration of the Supreme Being concept nor any linkage of it with the culture-hero or trickster cycle. (4) There was a somewhat varied synonymy for the deity, the commonest name being *manitu*. The term *kitei manitu* appears to have been a late introduction, the result of missionary influence about the middle of the last century or of the visit of two York Factory natives to Moose Factory in 1843 who, having absorbed some Christian ideas, passed themselves off as messengers from heaven. Cooper even thinks that such terms of address as "my father" or "our father" may be pre-Christian, which he supports by citing evidence from the usage of Algonkians elsewhere and also Athabascans. In view of the identification of the term *manitu* with some impersonal supernatural force by many writers on religion, it is worth emphasizing the testimony of Cooper that

nowhere among the Albany River Otchipwe, among the Eastern Cree, or among the Montagnais have I been able thus far to find the word *manitu* used to denote such force in connection with the Supreme Being belief, with conjuring, or with any other phase of magico-religious culture. *Manitu*, so far as I can discover, always denotes a supernatural personal being. . . . For "he has supernatural (conjuring) power," or "mind power" (as distinct from physical strength, they say *sōkadisi'u* (p. 38)

This parallels my own experience with a branch of the Ojibwa speaking peoples, who have achieved the undeserved notoriety of being an outstanding example of a group in which the term *manitu* is reputedly employed for an impersonal supernatural force or attribute.

All but one of Cooper's informants stated that (5) the Supreme Being was never seen and there was no clear information advanced as to "his" appearance, although

<sup>1</sup> The absence of sex gender in Algonkian languages may be connected with the absence of

one Indian thought that "he" had a body. (6) He was master or "boss" of "things in general including mankind" and other spiritual beings (e. g., powagans or guardian spirits). Informants differed as to whether "he" was maker or creator. (7) The Supreme Being, however, was active, rather than passive in relation to man. "He" gave people food and looked after life and health. "It was chiefly through dreams that he informed them what to do to have a successful hunt and to recover from sickness" (p. 40). (8) But "his" relationship to "the socio-moral law was either extremely indirect and tenuous or nil" (p. 40). On the other hand (9) prayers for food and health were offered in extreme need and (10) sacrifices of a bit of grease or meat, just before eating, were made. This was the "typical and exclusive one offered to the Supreme Being, usually with some simple non-formalized mental or vocal prayer" (p. 41). There was no (11) belief in a supreme evil spirit, several informants being emphatic in their denials.

In addition to presenting evidence from field inquiries on the points summarized above, Cooper offers an imposing array of data collected from documentary sources with reference to northern Algonkian and even Athabascan theism. Some of these data are from missionary literature, hitherto unexploited by most ethnologists. From this survey the author concludes (p. 66) that fragmentary as is the evidence, the available information

makes it reasonably clear that a Supreme Being belief is part of Algonquian culture as far back as our information goes. This would seem to add appreciably to the reliability of the data obtained for the James Bay Cree and Montagnais-Naskapi of the east coast and Labrador.

The reverse is also true. Dr Cooper's field work enhances the value of the brief and unelaborated statements with which we have been acquainted in the older literature. One cannot fail to recall in this connection the perspicacity of Father W. Schmidt whose pioneer efforts brought the cumulative force of such data to bear upon the problem of monotheism among Algonkian as well as other peoples at a time when it was more generally believed than now that missionary efforts, or the infiltration of monotheistic concepts from other classes of white men, were responsible for such beliefs. Dr Cooper has not side-stepped this problem. But he gives excellent reasons for rejecting this interpretation in the James Bay region. Finally, in view of the fact that the natives of this district are at present adherents of Christianity, the intrinsic difficulties confronting the investigator must not be overlooked. Dr Cooper's results indicate the degree to which he has surmounted these. Observing every caution in the collection of data and weighing with judicious care the reliability of his informants (pp. 45-48), it is no small achievement to have reached such interesting and convincing conclusions in respect to such a difficult and important topic.

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of anthropomorphization of the Supreme Being, although Cooper does not discuss this possibility.

*Old Chillicothe. Shawnee and Pioneer History. Conflicts and Romances in the Northwest Territory.* WILLIAM ALBERT GALLOWAY. (xiii, 336 pp., 23 pls. \$3.00. Xenia, Ohio: The Buckeye Press, 1934.)

Although the Shawnee have been the subject of enquiry for several anthropologists during the past half century, to date no systematic account of Shawnee ethnography has been published. In certain chapters of "Old Chillicothe" there is presented by Thomas Wildcat Alford, himself an Absentee Shawnee, a description of Shawnee culture which encompasses a greater variety of topics than any other account of the Shawnee so far published. These concern the Shawnee divisions (pp. 21-23, 308-309; referred to as "clans or septs"), boys' and girls' names and games (pp. 171-73), homes and family life (pp. 173-76), food and its preparation (pp. 183-89), religion and morals (pp. 177-79), tribal government (pp. 180-83), ceremonials and records of time (pp. 189-94), social life of the Shawnee (pp. 194-97), death and burial (pp. 198-99), the sacred bundles (pp. 303-305), the name groups (pp. 305-306; referred to as Umsoma), Shawnee origin legend (pp. 306-308), the guardian spirit quest (pp. 309-11), and a genealogy of Tecumseh (pp. 324-27). On the whole this ethnographic material is fairly reliable. Alford suffers from one serious disadvantage, however; when he was a young man the Absentee Shawnee sent him to Hampton Institute, Virginia, to be educated. While there he embraced Christianity and so lost his opportunity on his return from school to assume the chieftainship of one of the Shawnee divisions. He subsequently ceased to participate in the native culture of the conservative group of Absentee Shawnee and for this reason is not too well-informed concerning the esoteric phases of Shawnee life (religion, sex mores, etc.). The writer of this review, during recent field work among the Absentee Shawnee, employed Alford as an interpreter, but felt he was too sophisticated and out of touch with his own people to make an altogether satisfactory informant.

Alford has also attempted a statement regarding Shawnee movements during the historical period (pp. 24-42). For information on this subject he has leaned heavily upon the material contained in Charles A. Hanna's "The Wilderness Trail" without however acknowledging his source material. This oversight could have been remedied by Dr Galloway, the nominal author of "Old Chillicothe" and the sponsor of Alford's contributions.

For those who are interested in Tecumseh as an historical figure, a long and sentimental chapter (pp. 106-69) by Galloway giving details of Tecumseh's life and his courtship of Rebecca Galloway presents sufficiently convincing material to place Tecumseh definitely as a member of the Shawnee tribe.

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*The Five Civilized Tribes.* GRANT FOREMAN. (455 pp., 13 pls. \$4.00. Norman: University of Oklahoma Press, 1934.)

*The Rise and Fall of the Choctaw Republic.* ANGIE DEBO. (xvi, 314 pp., 26 pls., 6 maps. \$3.50. Norman. University of Oklahoma Press, 1934.)

History developed so much earlier than ethnology that it is usually assumed the student of any primitive tribe will find the historical side of his problem fairly well covered, but unfortunately modern historical methods have not been applied until recently to the long period of contact between our Southeastern Indians and the whites. The writings of Miss Abel were among the first in this field, to be followed by the standard volumes of Bolton and Crane, and now at length the great story of the Indian removal, and its after effects in the historical development of the near Southwest, and in particular of the state of Oklahoma, has become the self-imposed task of Dr Grant Foreman, whose interest was awakened through his connection with the work of the Dawes Commission.

Dr Foreman's earlier writings—"Indians and Pioneers," 1930; "Indian Removal," 1932; and "Advancing the Frontier," 1933—dealt respectively with the first voluntary removals of Southeastern Indians, the grand forced trek of the 30's, and the early struggles of the expatriated tribes in their new homes. In this latest volume he traces the history of the same people through their initial struggles to adapt themselves to the new conditions up to an apparently satisfactory adjustment in the 50's and until in the war between the states a new disrupting influence burst upon them. That this is a relatively virgin field is shown by the number of manuscript sources, and the list of local and special newspapers and periodicals cited. Dr Foreman has a keen instinct for human interest material, but he uses this, not solely on account of its emotional appeal, but to illustrate the political, social, and economic movements at work in the evolution of the five red republics of Oklahoma. The ethnologist will find many items to interest him, and the student of contemporary American history new light on many of his problems, but it will appeal most to the student of race relations and the executive and legislator who wish to benefit by prior experiences in interracial adjustments.

It might seem that Miss Debo's work would parallel in large measure that of Dr Foreman but it rather supplements it, for, instead of a horizontal study of several tribes during one period, it is a vertical monograph on a single tribe from earliest times to the end of autonomous government. It goes farther back than Foreman's since one chapter is devoted to the primitive Choctaw and one to Choctaw history before the removal. On the other hand the fate of the tribe subsequent to the Civil War is extended over nine chapters, the events of the period Dr Foreman considers being compressed into one. Miss Debo's work presents a similarly satisfactory use of original sources, published and unpublished. The treatment is somewhat less colorful but a trifle more factual and statistical.

It is to be hoped that further contributions to the history of Indian destruction and reconstruction may be looked for from both of these writers. Even as it is they have closed a large part of the gap in this particular phase of American history.

JOHN R. SWANTON

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1. *Prehistoric Rock Shelter in Val Verde County, Texas*. J. E. PEARCE AND A. T. JACKSON. (Bulletin, University of Texas, No. 3327; Bureau of Research in the

Social Sciences, Study No. 6; *Anthropological Papers*, Vol. 1, No. 3. 143 pp., 33 figs., 27 pls., 3 maps. Austin: University of Texas, 1933).

Excavation of the Fate Bell shelter by a party from the University of Texas constituted the first extensive investigations made in a previously little-known archaeological region in Val Verde County, Texas. The material and information obtained from that site form the basis for the present report. The monograph is by all odds the most complete and detailed paper on an archaeological project thus far emanating from Texas.

Comparisons between the material from Val Verde County and that from the Southwest, that from Brewster County, the area to the west of the Fate Bell district; and that from Central Texas point out interesting similarities and numerous differences. From this data the authors conclude that certain artifacts are similar to cognate forms from the Basket Maker phase in the Southwest; that the material as a whole bears a closer relation to that from Brewster County in the Big Bend region than to that from the Pueblo area; and that some traits in the Val Verde complex bear an unmistakable resemblance to those in the burnt rock mound region of Central Texas. The comparisons also demonstrate that the Fate Bell shelter produced three unusual types of artifacts: a distinctive flint projectile point, two unique sandal forms, and a kind of basketry restricted to that district. On the whole it is thought that the Fate Bell material indicates a modified Late Basket Maker pattern which probably persisted down to historical times.

There is a certain looseness and lack of continuity in the report which no doubt is due to the fact that it had three authors, although only two are listed on the title page. Doctor Pearce acted as editor and supervisor in the preparation of the monograph. Field notes of Mr Jackson, foreman in charge of excavations, served as a basis for the major part of the report. In fact it is stated that the bulk of these notes was taken over bodily. Sidney J. Thomas, a graduate student then at Texas, now at Harvard, had charge of the cleaning and analysis of specimens. His thesis, "The Archaeological Investigation of Fate Bell Shelter, Seminole Canyon, Val Verde County, Texas," submitted as part of the work for his Master's degree at Texas, has been drawn upon extensively and whole sections, as well as his maps and figures, were included in the report. So much of Thomas' material was used that he might well have been listed as one of the authors. The table of contents gives lists of maps, plates, and text figures but makes no reference to the headings for the various sections. It would facilitate the use of the report if the latter were given with their page numbers. There is no index. A minor item, but one which will cause some trouble for librarians, is that of denoting the paper No. 3 when 1 and 2 have not been published and apparently will not be for some time to come. Inasmuch as a series of separate monographs seems to be planned, the present might well have been No. 1.

Despite the factors mentioned in preceding paragraphs, the report is a marked improvement over previous publications and as stated before is the most complete and thorough paper produced by a Texas institution. It is to be hoped that Nos. 1 and 2 of the series will not be long delayed and that other reports on the work done

under Pearce's supervision will be made available. The Texas area is an important one and is as yet too little known by those outside the region.

FRANK H. H. ROBERTS, JR.

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*The Influence of Technique on the Decorative Style in the Domestic Pottery of Culhuacan.* ANITA BRENNER. (Columbia University Contributions to Anthropology, Vol. 13. 94 pp., 45 figs. New York: Columbia University Press, 1931.)

The author, already favorably known by previous publications, set herself the task of finding an explanation for the "careless but complex design . . . on the crudely shaped but well-baked ware" of Culhuacan, a small village at the foot of the Sierra de la Estrella (p. 11).

After having briefly reviewed the results of the classification of Prof Boas and of Miss Isabel Ramirez Castañeda, Miss Brenner discusses the characteristics of Culhuacan ware, and examines the conditions under which pottery is made and sold today in the region of Mexico under discussion: these conditions being of such striking similarity to those that obtained at the time of the Conquest, the author claims that a careful examination of them will solve problems pertaining to the pre-Conquest style (p. 15). From this analysis she concludes that the potters of Culhuacan worked mainly for wholesale consumption, producing a large amount of sturdy, usable pottery of little value: "thus the usefulness and durability of the object and its rapid manufacture, would seem to be the two chief factors considered most important by the Culhuacan potters" (pp. 17-18). This rapidity of execution accounts for the slovenly painted design, which was executed by what Miss Brenner aptly calls "calligraphic" methods, meaning by this that the designs are drawn by automatic movements of the hands: in this process automatic motor habits had a far greater part than conscious and controlled designing and drawing of a given decorative motif. These acquired motor habits, and the methods in which they were used, told even on the composition and on the elaboration of new designs (p. 59).

The author studies successively Composition (pp. 21-37), Design (pp. 38-82), and Rhythm (pp. 83-92), and sums up her Conclusions (pp. 93-94) in as lucid a way as she has presented and discussed her material.

Attention has lately been called to many cases where the art—the decorative as well as the representative art—of less-civilized groups had degenerated with appalling speed as soon as the natives had been introduced to the boons of Caucasian civilization; and it has been held as an axiom that the decorative art of primitive groups was always executed, if not with remarkable skill, at least with great care; slovenliness in design had been called upon, at best, to explain changes in design from realistic to conventional forms.

From this study it appears that, even within their own communities, slovenly work obtains; and what is more, that when they set their mind upon working slovenly, the result is far from being a rigid form of conventionalized design, but can become merely a nondescript, very ugly scribble.

Another interesting result of Miss Brenner's study is that she finds nowhere any evidence of "evolution" in the patterns (p. 94), as Hjalmar Stolpe, and so many others after him, were anxious to do.

The material from Culhuacan, which had already been called upon by Prof Boas ("Primitive Art" [Oslo, 1927], pp. 131-36) to illustrate his epoch-making discussion of technique versus symbolism in primitive art, has been made available to students in primitive art and decoration in a way for which Miss Brenner deserves the compliments and the gratitude of her co-workers in the field.

FRANS M. OLBRECHTS

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*Observaciones sobre la alfareria de los Médanos de Colón.* ANTONIO SERRANO. (Memorias del Museo de Paraná, No. 6, Paraná, 1933.) *Las culturas protohistóricas del Este Argentino y Uruguay.* ANTONIO SERRANO. (Memorias del Museo de Paraná, No. 7, Paraná, 1933.) *La estructura de los túmulos indígenas prehispánicos del Departamento de Gualeguaychú.* HÉCTOR GRESLEBIN. (Apartado de la Revista de la Sociedad "Amigos de la Arqueología," Vol. 5, pp. 1-51, Montevideo, 1931.) *Dos vasos indígenas—Hallados en Puerto Basilio.* HÉCTOR GRESLEBIN. (Solar, pp. 161-72, Buenos Aires, 1931.)

*Nueva hipótesis sobre el destino de las placas grabadas de la Patagonia prehistórica.* HÉCTOR GRESLEBIN. (Physis, Vol. 9, pp. 223-33, Buenos Aires, 1928.) *Descripción de dos nuevas placas rectangulares grabadas de Patagonia prehispánica.* HÉCTOR GRESLEBIN. (Physis, Vol. 10, pp. 8-16, Buenos Aires, 1930.) *Instrumental lítico de Patagonia prehispánica.* HÉCTOR GRESLEBIN. (Publicaciones del Museo Antropológico y Etnográfico de la Facultad de Filosofía y Letras, Series A, Vol. 2, pp. 99-119, Buenos Aires, 1932.)

*La antigüedad del hombre en la región de Sayape, Provincia de San Luis, República Argentina (Nota preliminar).* HÉCTOR GRESLEBIN. (Proceedings, Twenty-third International Congress of Americanists, pp. 305-12, New York, 1930.) *Las llamadas "botijas" o "tinajas" de la Provincia de San Luis (República Argentina).* HÉCTOR GRESLEBIN. (Physis, Vol. 9, pp. 46-71, Buenos Aires, 1928.) *Los "morteritos" de Cerro Varela.* HÉCTOR GRESLEBIN. (Solar, pp. 75-87, Buenos Aires, 1931.)

A group of ten pamphlets at hand, two by Antonio Serrano and the others by Héctor Greslebin, refer principally to three archaeological sections of the Argentine republic. The first four papers relate to new discoveries and new classifications of materials in the Province of Entre Rios, between the Paraná and Uruguay Rivers, commonly known as the Paraná Delta. The second group of three discuss the engraved stone tablets of Patagonia. Three more refer to finds in San Luis Province, Central Argentine.

The four papers dealing with the Entre Rios or Paraná Delta material are the most important. Greslebin has examined the oval mounds so characteristic of the region and pronounced them to be of two kinds: artificial, that is, completely man-

made, and semi-artificial, or additions to natural prominences. Even in the artificial mounds, however, the artifacts are all in the humus layer, leading him to the conclusion that the mounds were built primarily as raised dwelling sites, and secondarily used as burial hills. Finds of glass beads in some of the mounds are cited as evidence of continuous building into post-European times.

Serrano has classified the material from the Entre Rios section into five groups partly on typology and partly on geographic isolation, without however, any definite indications of chronology:

1. Guaraní: Mound cemeteries with groups of urns, decorated with finger scallops and painted bands of geometric figures. No bone work. Stone axes and tembetas.
2. Malabrigo: Modeled decoration, especially flat platters with tubular or concave handles which end in animal heads. Incised design. Secondary burial.
3. Thick ware: Cylindrical thick vessels with side holes, decorated with modeled and punched designs. Secondary burials and painted urns for child burial.
4. Inter-river: Non-mound dwelling and primary burial. No stone work. Good bone tools. Incised globular bowls. Also some painted ware and some simple modeled handles.
5. Ibicueña: Burial and dwelling mounds in low country. Ceramics with outline engraved designs, augmented with punches, but no modeling. Stone work poor, but bone common.

In Serrano's own analysis these five divisions correspond to two major types: the Guaraní and the Paraná (which includes Malabrigo, Thick ware, Inter-river, and Ibicueña), which is the generally accepted conclusion for the delta region reached by S. K. Lothrop. On the Uruguay side the same set-up is found without the subdivisions. A Tupi-Guaraní corresponds to the typical Guaraní, with the addition of perforated circular axes and effigy stone tablets characterized by hollows or depressions in their centers. A second group shows Paraná influence in general. A third group, however, shows Patagonian influence, with bolas, arrow points, and spear heads, as well as open bowls, some plain and some with simple point and line decoration. Greslebin, moreover, describes two globular bowls from Puerto Basilio with incised geometric designs which he believes are related to the incised stone tablets of Patagonia.

Outside of the classified material Serrano notes shell mounds in the Entre Rios section, a net-decorated ware suggestive of Santa Fé influence, and finally, a type of pottery tempered with a fresh water sponge (*Uruguaya coralliodes*) which has a northern distribution.

In three papers on the Patagonian incised stone tablets, Greslebin suggests (1928), confirms (1930), and asserts (1932) that they were intended as schematic models for poncho weaving. These stones are rectangular, 8-shape, or T-axis shape, and all have geometric incised designs, which compare favorably with textile designs and particularly with the Araucanian patterns. The argument is based on the fact that the rectangular stones have about the proportions of a poncho, that a seam effect is incised along the edge, and that the designs are similar.

Three more papers by Greslebin are unrelated notes on finds in San Luis Province of central Argentina. The first is a preliminary note on the association of human and fossil remains (notably *Megatherium*) in the sand erosion country of Sayape.



Obviously more work is necessary to confirm this find. The second paper deals with the numerous groups of fire pits called locally *botijas* or *tinajas*. These are circular structures something over fifty centimeters in diameter and fifty centimeters deep. The base is a ten centimeter layer of charcoal and ash, and the rest of the pit is solidly filled with ash, virtually without artifacts of any kind. Around the sides of the pit the clay edge is baked by the action of the fire, but not by special preparation. Greslebin refutes the three suggested uses for these fire pits. They could not have served for collecting rain water because they are filled with ash and the baked clay lining is not artificially prepared. They could not have been used for firing pottery as no sherds have been found. They could not have been used for depositing the ashes of chiefs as a chemical analysis of the contents shows too small a percentage of bone ash. One pit contained a skeleton which was probably a late intrusion. The possible use of these pits is left as an enigma. A final paper deals with a description of several grinding holes in the natural rocks around a pool at Cerro Varela. These holes are over thirty centimeters in diameter and one was fifty-three centimeters deep. Greslebin suggests that nomadic tribes met at the water holes and made these grinding holes, as there is no evidence of permanent habitation.

The papers on the Entre Rios section are the most important of this group of ten, and even in these, the contribution lies in an amplification of known material rather than in new discoveries or new interpretations.

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#### PREHISTORY AND PHYSICAL ANTHROPOLOGY

*The Stone Age Races of Kenya*. L. S. B. LEAKEY. (xii, 150 pp., 52 figs., 37 pls., map. \$12.75. London: Oxford University Press, Humphrey Milford, 1935.)

The excavations of the young British archaeologist, Dr L. S. B. Leakey, in Kenya, British East Africa, have resulted in finds of the very first order of importance and have secured for their discoverer a well deserved renown. "The Stone Age Races of Kenya" fulfills Dr Leakey's promise of a work upon the skeletal remains from Kenya, and also gives the first extended account of his portentous discoveries of 1932—remains of morphologically modern man and stone implements in deposits referred to Middle Pleistocene and Lower Pleistocene age.

For more than twenty years Sir Arthur Keith has forcefully advocated the early Pleistocene antiquity of *Homo sapiens*. This consummate master of the anatomy of fossil man succeeded in convincing some of us, at least, that he was right. It needed only one unassailable discovery, one cast-iron case of the remains of *Homo sapiens* undeniably *in situ* in an admittedly Lower Pleistocene deposit, accompanied by appropriate fauna, triumphantly to vindicate that great anthropologist. Preliminary reports seemed to indicate that the splendid researches of Dr Leakey had most copiously fulfilled the requirements of definitive proof; had provided not one but two irrefragable examples, not only geologically and palaeontologically fool-proof, but equipped, to boot, with perfectly suitable stone industries. The reviewer,

an early convert to Sir Arthur's views, was eager to greet these epochal discoveries with acclaim, and "e'en the ranks of Tuscany could scarce forbear to cheer." This enthusiasm became almost incandescent when the reports of a large committee of scientists, invited to sit in judgment upon Dr Leakey's discoveries, proved to be unanimously approbational. Now comes Dr Leakey's book.

The present reader, frankly, is appalled at Dr Leakey's temerity in redefining the boundary between the Pleistocene and the Pliocene, and the criteria for the Pleistocene itself. For the boundary, he lays down the following definition:

If members of one or more of the genera *Elephas*, *Bos*, and *Equus* (however primitive) occur in a deposit, that deposit is Pleistocene rather than Pliocene (p. 4).

Further, he divides the Pleistocene deposits into Lower, Middle, and Upper series on the basis of strangely mixed criteria. Lower Pleistocene deposits are those which antedate the evolution of the earliest Chellean stage of culture; Middle Pleistocene starts with the first stages of the Chellean culture and ends with the close of the Kamasian pluvial period; Upper Pleistocene is the Gamblian pluvial period and the succeeding Makalian wet phase. Thus he begins with palaeontological criteria, then switches to archaeological criteria, and ends up with climatological criteria. This *tour de force* in definition slightly undermined the confidence of the reviewer in Dr Leakey's geology.

First of all there must be raised anew the question of the validity of the geological and climatological correlations upon which Dr Leakey's case rests. Certainly some admirable and careful geological research has been carried on in Kenya by Nilsson, Wayland, Solomon, and others. All seem to agree upon the essential rightness of the conception of two great Pleistocene pluvial periods, separated by a long arid interval, and followed by a series of recent pluvial and interpluvial periods of shorter duration. Dr Leakey and his English colleagues who have done the geology, do not hesitate to equate the Kamasian, or first great pluvial period, with the Gunz-Mindel glaciations and interglacial period, although, of course, these correlations are tentative. Dr Erik Nilsson, the Swedish geologist, is much more conservative on the subject, and seems indisposed to commit himself beyond a correlation of the two pluvial periods with moraines of two glaciations which he distinguished and mapped with admirable precision upon the slopes of the nearby Mt. Elgon. One gathers from his work a rather dubious inference that the first pluvial period, or Kamasian, may correspond to the Riss glaciation in Europe, the great interpluvial period to the third interglacial period, and the second pluvial period to the fourth or Würm glaciation.<sup>1</sup> His geological data on the levels of the ancient lakes are presented with skill and completeness so as to inspire confidence. His correlation of the Kamasian with the Riss may or may not be correct. In any event, Dr Leakey has made two finds of *Homo sapiens* that he refers to the Kamasian or first pluvial period. These are thus at least as old as Middle Pleistocene, even with the later

<sup>1</sup> Erik Nilsson, Quaternary Glacial and Pluvial Lakes in British East Africa (Geografiska Annaler, Aug. Vol. 13, Haft 4: 249-349, 1931), cf. esp. p. 328.

dating of Nilsson. Consequently, even this correlation would not invalidate the claim that the Kanam mandible is the earliest find which can be referred to *Homo sapiens*, or a form of man virtually identical.

When we come to the account of the discovery of "Lower Pleistocene Man, *Homo kanamensis*," we naturally expect, first of all, a detailed account of a bomb-proof, impregnable, archaeological position, based upon all that is scientific in the way of methods and records of excavation. Yet the account is, to say the least, casual. A native worker dug out a mass of matrix, "from the side of this gully, *at about eight feet from the top of the gully*" (p. 15). This block of material yielded the Kanam mandible. There are no detailed measurements, no meticulous diagrams (although indeed sections of this gully and adjacent gullies, based upon levels, are given). There is not even a close-up photograph of the spot which contained the precious block; only a general view of the site with a sub-caption "The cross marks the gully but not the actual spot" (plate I). This regrettable omission is probably due to the fact that the bellows of Dr Leakey's camera developed an unsuspected leak (p. 28) so that many of his photographs were unsatisfactory. Somewhere near the spot which yielded the mandible there was discovered a *Deinotherium* tooth and somewhere else "in the same horizon" an artifact of the "pebble-tool" type, but the precise provenience of these associated finds relative to the mandible is not stated. Plate I also shows "some of the tools from Kanam," but does not indicate which of the three was found near the mandible. From the photograph these tools do not look convincing, but the reviewer is perfectly willing to accept Dr Leakey's identification of them as artifacts. Now, on the whole, in spite of certain gaps in the information relative to the locus of the mandible, Dr Leakey makes out an excellent case for its occurrence *in situ* in a Lower Pleistocene deposit characterized by *Deinotherium*, *Mastodon*, *Hipparion*, and a pre-Chellean culture. In the opinion of the present writer, Dr Leakey has established his point, but certain poignant regrets are aroused by an apparent vulnerability of his archaeological technique, as revealed by the account (which surely does an injustice to his method). Dr Leakey's hard luck with his camera gives this account an almost Galley Hillish smack.

The Kanjera finds, assigned to a Middle Pleistocene deposit, consist of fragments of four skulls, two of which were found *in situ*, and the other two, on the surface. The excavation is described, but there is only one very generalized diagrammatic section and no close-up photographs, exact measurements, or precise indications as to the relation of finds of Chellean tools and fossil animal remains to the human bones. Modern archaeological technique calls for a surveying method whereby each specimen could be returned with precision to the spot from which it was taken. Here again, one is forced to depend too much upon general statements concerning the age of the site, the type of associated remains, and the absence of evidence of intrusion. Nevertheless, the reviewer is again disposed to accept Dr Leakey's interpretation on general grounds, although with some misgivings.

The main part of the book concerns itself with the description and analysis of the skeletal remains of man discovered in Dr Leakey's diggings. Unfortunately

the author's knowledge of physical anthropology suggests the gifted amateur rather than the professional worker who has steeped himself in his subject. The description of the all-important Kanam mandible is neither precise nor detailed, and the plates which illustrate it are really wretched. Nor does Dr Leakey seem to be more at home in odontology. His discussion of the teeth does not command confidence. Certainly, this reviewer is unable to accept Dr Leakey's creation of a new species on the basis of the slender evidence offered. So far as one can make out from this book the *Homo kanamensis* is *Homo sapiens*.

The Kanjera skull fragments have not been made the subject of the meticulous anatomical description which is absolutely essential, but are discussed principally with reference to their size and their position in the crania. The plates illustrating them are so poor (since many of them are based upon photographs which are out of focus), that they do little to fill the lacunæ in the text. Under these circumstances one cannot appraise the accuracy of the author's cranial reconstructions nor give serious consideration to the measurements taken upon these reconstructions.

Throughout the volume are life-size, coarsely drawn views of skull reconstructions and of complete crania which are almost useless, since practically no attempt has been made to delineate anatomical detail and one can contemplate only vast areas of shading and cross-hatching.

Dr Leakey offers measurements on the later and more complete series of fossil crania and bones which his excavations have yielded. He very properly describes his measuring technique in detail. Trivial points are the spelling of acetabulum "ascetabulum," and of malleolus "maleolus" (pp. 42, 43). The author chooses to depart from standard procedure in the measurement of innominate bones. The full-sized plates illustrating the Upper Aurignacian skulls look very much like photographs of casts. The volume does not go beyond simple recording of raw measurements and indices on individual specimens. There is no attempt at statistical analysis nor at a really thorough investigation of racial affinities. The author goes so far as to say that from present knowledge "the Stone Age races of Kenya are not all of the type which would be termed typically 'negro'" (p. 129).

"The Stone Age Races of Kenya" is a cumbersome folio volume, printed in very large type, and sold at a price scarcely justified by format and contents. A more modest book, within the reach of the scientist's pocketbook, and without vast, sprawling plates, would have been a more judicious choice.

The reviewer does not wish to put himself in the detestable position of one who came to pray and remained to scoff. He was quite ready to join in the rousing British cheers which have greeted Dr Leakey's exploits, but this book has abated his enthusiasm. Dr Leakey is a brilliant young scientist, who by his epochal discoveries has made Anthropology his debtor. It seems improbable that any single anthropologist, however versatile and however experienced, would be competent to deal adequately with all of the different classes of evidence and with all of the varied masses of material which Dr Leakey's excavations have brought to light in a few years. The work should have been parcelled out among a group of experts, and executed less hastily and more thoroughly. An excellent model for scientists who are

confronted with a gargantuan task involving extensive research in geology, palaeontology, archaeology, and physical anthropology, is offered by the succession of admirable monographs issued from the Cenozoic Laboratory of China by the late Professor Davidson Black and his colleagues. Dr Leakey's researches are perhaps no less important, and one confidently expects in his future reports that he will attain a level of excellence in careful description, exhaustive analysis, and well-considered deduction which is commensurate with his scientific acumen and energy as a field archaeologist.

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*Das frühneolithische Skelett von Gross-Tinz in Schlesien.* OTTO RECHE AND JOHANNES NESTLER. (Veröffentlichungen des Staatlich-sächsischen Forschungs-Institutes für Völkerkunde zu Leipzig, 3rd Series, Vol. 1. 58 pp., 10 figs., 5 pls. Leipzig: R. Voigtlander's Verlag, 1933.)

The well preserved male skeleton described in this monograph was discovered near the village of Gross-Tinz in Silesia, buried immediately in or on the loess, and from the antler axe found with it is considered to have an antiquity of 6000 to 6500 years. It is, quite reasonably, the oldest human burial from Silesia.

Professor Reche gives a meticulous description of the teeth, the mandible, and the skull. Four pages of measurements and indices provide full information. Comparisons between this specimen and two crania from the Pritzerbersee are provided, but of comparable material from other regions there is a conspicuous lack. Why the male skeleton from Obercassel should have been ignored in this connection is a little mystifying. There are a number of interesting points of resemblance. The general conclusions with regard to the skull are summed up thus by Professor Reche:

Wir haben also ohne Zweifel . . . einen aussergewöhnlich typischer Vertreter der klassischen "Nordischen Rasse" vor uns, der in jedem Lehrbuch als "Typus" abgebildet werden konnte.

Dr Nestler's equally painstaking account of the remainder of the skeleton has, in addition to the numerous tables of measurements (following Martin), brief comparisons with a wide variety of other human types (modern European, Neolithic, Neanderthal, Cro-Magnon). His conclusions are that there are resemblances to Neolithic, to modern, and to Aurignacian man in varying degrees.

Briefly, the present find is a young, adult male with an excellently preserved dentition, a metopic suture, long and high headed, a narrow and strongly built straight face, a moderately broad nose, stout limb bones, well muscled, and with a stature of about five feet four inches. A brief comparison with the Obercassel male would seem to place no difficulty in the way of suggesting that Gross-Tinz belonged to the same racial stock: probably a widespread one in northwestern Europe at the end of the Ice Age. Obercassel having been long considered to be an early "Nordic"—of Magdalenian date—there is some justice in Professor Reche's

pronouncement on the Gross-Tinz specimen, but one wonders if there may not have been other factors which affected the specific form which that statement takes.

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*Notes on the Somatology and Pathology of Ancient Egypt.* R. WOOD LEIGH. (University of California Publications in American Archaeology and Ethnology, Vol. 34, No. 1. 34 pp., 7 pls. \$0.75. Berkeley: University of California Press, 1934.)

Dr Leigh has contributed another of his critical studies of prehistoric and early historic human pathology, with special reference to the dentition. On 230 fragmentary skulls, collected in 1899-1905 by the Hearst-University of California Expedition, certain maxillo-mandibular measurements and certain cranio-facio-dental observations have been made.

The majority of the skulls (male and female) are brachyuranic, microdont, and orthognathous, with index averages of 116.8, 41.2, and 95.4, respectively.

As the evidence accumulates it is obvious that dental disease is not as limited to modern times—"civilization"—as generally believed. In the sample studied by Dr Leigh, from one to four of the first permanent molars had pulps exposed by wear by the time the individual was fifty years of age. Following this fourth degree attrition occurred periodontoclasia almost regularly, with exfoliation resulting. Of the total number of skulls studied 12% had one or more carious teeth, 7% had radicular cysts, 10% had alveolar fistulae draining chronic maxillary sinusitis, and periapical osseous lesions were common. Possibly associated with dental pathology is the frequent occurrence of osteoarthritis.

We may well observe that if today we *break* down our teeth by incorrect diet, earlier peoples *wore* their teeth down by a rough diet. The end result is frequently identical—loss of the tooth!

The paper is concluded by a short discussion of food, principally corn, as a factor in dental disease. Dr Leigh is inclined to accept the general thesis that "luxury" and dental pathology go hand in hand, though he perhaps errs when he lists milk as one of the foods "certainly conducive to dental caries."

The illustrations of type skulls and mandibulae and of representative pathologies are excellent.

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## GENERAL

*An Introduction to Cultural Anthropology.* ROBERT H. LOWIE. (xiii, 365 pp., 37 pls., 2 maps. \$3.50. New York: Farrar and Rinehart, 1934.)

Dr Lowie's latest book fulfills the proportions of a deliberate and well conceived plan. In subjecting it to review the reader, teacher or critic, must measure his own

narrow purpose with that of the author and either approve of the method and say so or accept it in silence, for there is nothing legitimate to be said against the aim of the author to produce a sturdy introduction to anthropology presenting essential facts topically and deliberately avoiding theoretical discussion. And he has consummated his endeavor with an art, a lucidity, originality, and grasp of the bewildering difficulties which beset the novice, all his own. As individual modes of reception, even tastes, often vary widely, the method of approach in a new draft of the old stock material of anthropology will appeal to all who are teaching as well as learning the subject, and the volume will settle down to its deserved place on the shelf of text books, a place of the first rank. It should be no defection from the highest standard of critical judgment to say that the teaching anthropologist will find the book a positive delight and a help.

The orderly array of instances of custom, the aptness of illustrations from economic, social, and religious life, are a model combination of the comparative, the historical, and the analytical methods of dealing with ethnology; a combined source book and intellectual treatise. The author's wide experience in the realms of social and religious science and his understanding of student needs show forth in vigorous measures.

There are, however, some confidences to exchange between author and reviewer, and here is one place to present them

Some confusion in representing ideas of higher and lower racial status, mental traits, brain size and faculties, is apparent in the following statement (p. 7)

If races are to be ranked as higher and lower, it must be for their mental traits. Fair hair and long heads have no intrinsic value, though conceivably they may be *signs* of superiority. The only physical feature that directly suggests mental worth is the brain because in the animal kingdom a better brain does go with a higher status.

Here, however, there is again overlapping. Mongoloids are not inferior to Caucasians at all; Negroes somewhat; Australians more so, yet without falling before the more poorly equipped whites. There are really groups with larger and smaller brains, just as there are taller and shorter groups

Nevertheless, this result cannot be interpreted directly in terms of psychology. While very great differences in the brain are significant, lesser ones are not. In civilized countries autopsies sometimes prove criminals to have large brains while some great men were found to fall below the normal. A difference of merely a few hundred units is thus not decisive, and the smaller brains of Australians or other races are no positive proof of inferior faculties

Something akin to equivalent racial status is clear to Dr Lowie's mind, which never fails in clarity of judgment, yet here there is mist in conveying conclusions as to superiority-inferiority to minds untrained in the grading of human groups. Probably no one could do better in a more purely objective attempt at analysis. Yet one might expect an authority as potent in the anthropological court to avoid moulding opinion by "silently" advocating a graded rating of races until a solution can be predicted that will square with the full verdict of scientific judgement. It seems to one who professes no prejudicial convictions that to quote authorities on this point might have been preferable to stating it as is done. And it might also be

inserted here that the absence of reference in other instances to authorities where there are two sides to theoretical questions is noticeable. Sanction could be found for applying this comment also to paragraph 2 and the end of paragraph 4 on page 9.

Again a note of professional weariness, when encountering the conventional classification of culture periods (tabulation, p. 11) based solely upon an inventory of mechanical inventions and economic traits. The gradual sanitation of professional thought along lines of cultural classification from savagery to recency is finding its interests turning to consideration of other inventions than those concerned with minerals and machines. Strange that Dr Lowie should pass by the fields of social and religious science, which he has made so closely associated with his name, when tabulating culture periods sequentially as though he did not yet accept the social structure of the Australians to be a "sign" (p. 7, paragraph 2) of progressiveness more weighty than the simple status of their industry, or the religious complexity, even richness, of the Maori to be as indicative of cultural advancement as their ignorance of the metal age is, according to old methods of classification, an index of their primitiveness. Some of us will therefore find it difficult to understand Dr Lowie's mood in respect to the use of criteria in the classification of culture periods. We would ordinarily picture him as a spokesman for the adoption of a scale based upon mental rather than material achievements, in terms perhaps bolder than most of us who are convinced of its significance, and certainly more eloquent. Yet while we remind ourselves that the book is not prepared for critical expert theorists, the fact remains that student readers seeking light from a new source inevitably imbibe sense-judgment, often more positive than the author intends it to be, from the nature of his treatment of moot points. Since the audience for which the book is professedly intended is the non-expert audience, impressions made may always be safeguarded by an extra dose of caution. I am not at all sure that I could, for instance, avoid discussing the racial equation without bias myself.

Dr Lowie several times surprises us by stressing motives for human customs conventionally without regarding what we know, through recent findings in various areas, to be motives partaking largely of a spiritual nature, as art (p. 179-81), scalping, torture, cannibalism (p. 225), except for what is said in another section (p. 228).

The chapter on marriage and the family (XIII) leads from treatment of simple up to complicated forms of mating by gradual degrees and so clearly that nothing could be desired to prepare the way for the student whose first experiences with primitive social forms is so often found to be confusing. That the definition (p. 239) of perplexing marriage regulations among the extinct Tamanak of the Orinoco is self-explanatory to a layman upon whom the experiment was tried, after having read over just once the preceding four pages, is testimony to the clarity of style and arrangement of thought.

A final comment: I only wish that the author had also seen fit to treat as a selected area the tribes of eastern and northeastern North America, not so much for the benefit of students of general anthropology but to contribute his sagacity to a field where I for one am ready to admit the need of aid. In probably the most charming



piece of exposition in all his career Dr Lowie has set down the inevitable logic of elementary anthropology.

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*Patterns of Culture*. RUTH BENEDICT. (xiii, 291 pp. \$2.50. Boston and New York: Houghton Mifflin Company, 1934.)

Dr Benedict's book is done with the insight, dignity, and charged style that we have come to expect of her writings. It is an important contribution, in regard to which, however, two considerations must be kept in mind. First, it is a work for the intelligent non-anthropologist, not for the anthropologist. Second, as a book, it deals with culture patterns in the wider sense of the word, not primarily with the psychiatrically-delineated "configurations" which she discussed some years ago in the *AMERICAN ANTHROPOLOGIST*.

The basic concept is that of culture occurring in certain patterns which determine its fabric and are of influence on the lives led by all individuals under a culture. These patterns make up the character or distinctive quality of each culture; its "genius," to use an old phrase aptly reused by Dr Boas in the preface. This quality of course inheres largely in forms, or interrelations of forms; it can never be adequately formulated in terms of culture content alone. Nor can it be measured or demonstrated. Essentially, it is seizable and definable by subjective empirical approach. The estimation of the relative weight of the patterns in a culture, for instance, must be done primarily by feeling; and their documentation is of the sort which substantiates an a priori, synthetic apperception, whose validity depends on the fit of the pattern parts and their leaving no significant remainder of the culture undealt with. No approach is farther than this from the customary analytic one of Boas; and it speaks for the catholicity of the group of which he is leader that the present work is from the pen of a many years' associate. Those who will, may quarrel with the approach as "unscientific." They would have to quarrel also with Burckhardt's "Renaissance," Bryce's "Commonwealth," Redfield's "Tepoztlan."

Very considerably, the book is propaganda for the anthropological attitude, as entertained fervently by Dr Benedict, to the audience of cultivated intelligence generally. Hence there are passages that deal with concepts which to professional anthropologists have become somewhat thread-bare: the insufficiency of racial heredity to explain differences of culture, for instance (pp. 233-36). This is not a stricture. With the objective of the book what it is, such passages are appropriate and necessary.

In her seventh chapter, on the nature of society, the author argues explicitly that "we do not need a plank of configuration written into the platform of an ethnological school" (p. 229). However, more than half the text (166 pages of 278) is given over to a picturing of Zuñi, Dobu, Kwakiutl from the angle of the semi-psychiatric configurations which Dr Benedict was the first to develop. Despite her warnings, therefore, there is likely to be a residuum of misimpression that her gen-

eral concept of patterns means nearly the same thing as her special type of configurations. Her depiction of the two American cultures is worked over, not a reprint from the article in this journal. The third, on Dobu, is of course wholly new. Incidentally, with its compression from a book into a chapter, the effect of Dobu comes out even harsher than in Fortune's hands: the culture is pathological to the point of repellant.

Throughout, the book has the quality of distinctive, almost passionately felt, balanced thinking precisely expressed.

One paragraph may be pardoned the anthropologist. The reviewer does not feel that Dr Benedict has carried her configuration approach too far: rather it remains to be developed farther, now that this book is done. She points out that the Kwakiutl are specialized in their megalomania: the southern Salish have an individualistic culture, the interior Salish an uncoordinated one. She shows illuminating contrasts to the Pueblos among Pima, Navaho, Plains tribes at special points. Are not these cultures worth treating systematically from the same point of view? We have full enough data on most of them. What matter if their configurations come out less dominated by a single attitude? They are just as important. The psychologist does not study only accentuated psychoses or the most extreme personalities. Such may serve best for an entry into a new field; but the goal of understanding is normality in its diversity. Every culture must have its "genius;" to know any one is a contribution to the understanding of how human culture acts. If her studies of Zuñi and Kwakiutl continue to remain isolated, their meaning will be uncertain, their validity will be questioned. The reviewer hopes she will push farther, into more resistant material, and give us new stimuli and insights.

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*Thoughts, Talks and Tramps.* EVERARD IM THURN. Edited with a Memoir by R. R. Marett. (xxiii, 285 pp., frontispiece, map. \$4.00. London: Humphrey Milford, Oxford University Press, 1934).

This collection of thirteen intimate and entertaining articles, most of which are reprinted from obscure publications, contains a wealth of first-hand ethnological observations regarding the natives of British Guiana and the islands of the South Pacific.

The twenty years spent in British Guiana as curator of the museum at Georgetown and later as magistrate and government agent of the Northwest Province gave to Im Thurn an intimate knowledge of native customs which accounted for his unparalleled success as an administrator of native affairs. In subsequent years when he held the important positions of Governor of Fiji and High Commissioner of the Western Pacific, Im Thurn's regime stands as the classic example of applied anthropology.

The present articles reflect his great interest in the problem of the effects of white contact upon native cultures but are of equal interest to those less philosoph-

ically inclined because of the wealth of ethnological detail which they contain, much of which, Im Thurn explains, is recounted as written in the field. The book contains much more solid information of interest to the anthropological student than its rather general title would imply.

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*Sex and Culture*. J. D. UNWIN. (xxiii, 676 pp. \$12.00. London: Oxford University Press, Humphrey Milford, 1934.)

The thesis of this voluminous work is that cultural energy in all human societies is the result of restrictions upon sexual behavior. The more stringent these have been, the higher the culture has risen in the scale. The author has studied eighty primitive societies, being guided, he states, only by the character of the descriptions available, and he concludes that there is an invariable correlation between the degree of sexual restriction and cultural achievement.

In order to attain this absolute correlation, he has had to manipulate his definitions both of sexual restrictions and of cultural achievement. His correlations, in fact, only concern the limitation of pre-nuptial freedom in women and the nature of religious rites. It is never quite clear why he regards pre-nuptial restrictions as being so much more dynamic than post-nuptial, but restrictions upon the sexual opportunities of women are more desirable than upon those of men, he says, because women are more important in child rearing.

In defining cultural achievement the standard is surprising. The lowest level recognized is that characterized by religion without post-funeral honor of the individual dead or without worship in temples, these two being the criteria of the middle and highest level of primitive cultural achievement. For a culture to rise from the lowest plane to the next higher level it is only necessary to restrict pre-nuptial freedom of women; to rise to the highest level, where they will be capable of building temples, it is only necessary to demand tokens of virginity. It is not necessary that the restrictions shall be enforced for all females in a society. Thus, Samoa has the necessary cultural energy because restrictions are imposed on one girl in the village, the taupou, and rates as a culture with the most stringent restrictions, whereas Zuñi, for instance, ranks as one with complete absence of restrictions, having, in the author's words, not even "irregular or occasional continence."

It is impossible within the limits of a brief review to criticize the long list of absurdities that are involved in the correlations in this volume. They can be indicated from the author's handling of American Indian material. No tribes of North America north of Mexico have, according to his definitions, either temples or ancestor cult, and must therefore have no restrictions upon sexual freedom. He has described twenty-five tribes from North America, but he has omitted without comment or excuse the entire area of the chastity belt. If, as he says, he was guided in his selection entirely by the excellence of the ethnographic material available, it

would have been natural to include at least the Menomini and the Cheyenne. The latter's prohibition of pre-nuptial sex life would of course have played havoc with his one-to-one correlation between high cultural status characterized by temples, and the existence of pre-nuptial restrictions.

The final chapter of the volume is entitled "Necessity in Human Affairs" and contains the laws based upon his correlations. Thus:

Any society in which complete pre-nuptial freedom (outside the exogamic regulations and prohibited degrees) has been permitted for at least three generations will be in the zoistic cultural condition (p. 347).

The moral in regard to our immediate cultural past is strongly stated:

As soon as . . . marriage and divorce by mutual consent became part of the inherited tradition of a complete new generation, the energy, either of the whole society or of a group within that society, decreased, and then disappeared (p. 412).

For the future he seems to consider it necessary that women shall be given legal status equal to men's in order that their situation may be bearable enough so that cultural energy is produced.

The volume is an extreme example of the manipulation of anthropological material to support private programs of social reform, in this case, a program of return to the immediate Victorian past. It makes clear, as has already been abundantly demonstrated in anthropological literature, that any thesis, no matter how unlikely, can be upheld by a suitable rearrangement of cultural facts from primitive peoples. Only insistence upon a greater scrupulousness and a greater intelligence can prevent the recurrence of such volumes of special pleading

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*A Manual of Excavation in the Near East. Methods of Digging and Recording of the Tell en-Nasbeh Expedition in Palestine.* WILLIAM FREDERIC BADÈ. (vii, 81 pp., 14 illus. \$1.50 Berkeley: University of California Press, 1934.)

Considerable popular interest was aroused recently by articles in the press and semi-scientific publications which announced that ancient finger prints impressed in the soft clay by Palestinian potters as they shaped their vessels now served the archaeologist as "date-marks." This evidence, it was indicated, aided in determining the contemporaneity of occupation levels at various parts of a mound and also assisted in correlating assemblages of pottery found in tombs with the material from different layers. The expedition which contributed this item to the complex of archaeological procedure likewise developed and elaborated upon the technique of excavation and the methods of recording described by Director Badè in his little manual. In the introduction it is suggested that the methods described, although set forth in a Palestinian context, are applicable to archaeological enterprises anywhere.

Considered from the viewpoint of the Southwest, a number of the features

discussed are applicable to excavations in that region, while others are not. The main survey map, which is the basis for the system of recording, is the contour control type with superimposed grid, with which most present day field men are familiar. Conditions in the Southwest call for much smaller squares than those indicated in the Manual but the general principles described by Badè hold good. It would be advisable, however, to give even more specific location with respect to the identifying square of the peg than that indicated in the text. The section on the organization and management of excavation gangs is interesting to read, but most of the methods mentioned are not applicable to the Southwest because the laborers and the manner of working are so totally different from those in Palestine. In a broad way some of the suggestions concerning the staff, division of duties, etc., would be of help where a large undertaking such as that of Hodge at Hawikuh, Judd at Bonito, or Kidder at Pecos, was involved, but for the average expedition they are not germane.

Descriptions of the various methods of recording, of the handling of specimens, what to save and what to discard are complete and detailed. The techniques discussed are adaptable to the Southwest. As a matter of fact similar plans of procedure have been in use for a number of years. Suggestions of value will be found in the sections on the use of the camera and the subject of surveying and mapping. This is also true of the discussion of the problems of stratigraphy. It is in the latter connection that the finger-print dating is described, but from the text the reader gathers that the method is still in its experimental stages.

The Manual would be more of a contribution if it contained further details about additional items. The author refers to "knife and brush work" yet does not describe the process. The general reader would be interested and the beginner helped by an explanation of the technique involved. This is true also with respect to the question of skeletal remains. There is a discussion of the subject of finding and clearing of tombs but no mention of the way in which the bones were handled either for preservation or removal. Were tomb (grave) cards or form sheets, such as used by many American archaeologists, a part of the record or was the necessary information recorded in the director's journal? If special cards are provided a facsimile of one would add to the information in the Manual. It might serve as a guide for other excavators. Further, what is the technique of excavating a tomb, or a room, or a silo for that matter? Is the material removed layer by layer according to the nature of the strata, by levels based on arbitrary measurement, or as a homogeneous unit? Indications in the text are that the layer by layer or so-called onion-skin method was used on occasion. A brief description of the manner of procedure would be in order.

A paragraph on the director's journal, its form, style, and the system of recording features not otherwise taken care of by maps, specimen cards, and the registry book would enhance the Manual. The same may be said for the subjects of the treatment of perishable objects, the kinds of preservatives employed and how they are applied, the packing and boxing of objects for shipment. This additional information could have been included in the book without materially increasing its size and

would have made it far more helpful to beginners and students in archaeology. The items which are presented demonstrate that the Tell en-Nasbeh expedition was especially attentive to the problems of technique and suggest that features such as mentioned above probably received similar treatment. Consequently it is to be regretted that they were not incorporated in the text so that the publication could truly be considered a "Manual of Excavation."

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BUREAU OF AMERICAN ETHNOLOGY

*A Decade of Progress in Eugenics: Scientific Papers of the Third International Congress of Eugenics.* HARRY H. LAUGHLIN (ed.). (xi, 531 pp., 28 pls. \$6.00. Baltimore: Williams and Wilkins Co., 1934.)

This is the publication of the Eugenics Congress held in August 1932, at the American Museum of Natural History, and includes a total of sixty-five papers and a summary of the exhibits arranged at the museum for the members and guests of the Congress. It is a considerably smaller output than that of the 1922 Congress from which the two volumes: "Eugenics, Genetics and the Family," and "Eugenics in Race and State" resulted, and on the whole less provocative. Only a relatively few papers are devoted specifically to reports on human genetics; there is a small undistinguished section on method, and another on racial questions. The bulk of the discussion is devoted to a consideration of the various factors and agencies making for eugenic and dysgenic states in the population: the falling birth and marriage rates of the fit, the preservation in various ways of the maladjusted and inadequate, the effects of war, the biology of fertility, and the possible means at our disposal for effecting reform.

While there is almost nowhere in the volume an adequate analysis or appraisal of the evidence which is available on the nature-nurture problem, and which should be basic to any eugenics thinking (H. J. Muller's article on "The Dominance of Economics" is a notable exception to this statement and a penetrating criticism of the whole eugenics program), there is in a few papers a sober acknowledgement of our lack of any definitive knowledge on the nature and inheritance of mental disorder. The discussion by Pollock, Malzberg, and Fuller on the manic-depressive psychoses, and Dr Florence Powdermaker's article on the social factors in mental development are examples in point, stressing the need respectively for more careful methods of investigation and for a recognition of the environmental factors which may be operative in producing some of the vast hordes of mental deviates. There are also some interesting compilations of statistics on birth rates, marriage rates, a straightforward presentation of population statistics by J. H. Landman, and a few other items which are sound enough if not particularly stimulating.

It is the papers which deal with ways and means of reversing the proportion of eu- and dysgenic elements in the population, however, which most vigorously belie the title of the volume. Here one finds ready and unchallenging acceptance of the simple notion that mental disorders and defects are one and inherited, a per-

verse disregard for any evidence to the contrary, and the dangerously confused reasoning that if feeble-mindedness is not entirely inherited, then at least a feeble-minded environment is inherited and it makes very little difference anyway whether accurate distinctions are possible. Sterilization is variously brandished as a means for solving the unemployment problem, the crime problem, as a measure which must be "a valuable asset to humanity" since it possesses "a sufficient appeal to induce 50% of the States of the Union to adopt statutes permitting its use," and in one inspired article by L. K. Sadler as a method which if "enforced throughout the United States would result in less than one hundred years in eliminating at least 90% of crime, insanity, feeble-mindedness, moronism, and abnormal sexuality, not to mention many other forms of defectiveness and degeneracy." Such arrogantly sweeping figures need no comment from the more humbly ignorant among us.

On the positive side, that of increasing the eugenic elements of the population, there are still the tacit assumptions that the "better" classes are the carriers of the most desirable traits for the race, and that if somehow these better classes could be made to realize their responsibilities to the future they would marry and procreate eugenically. Dr Muller's comment is perhaps the most apt: "Is it to be wondered at that a census of eugenists has disclosed an appalling failure to reproduce themselves, despite the fact that they are maximally steeped in their own doctrines?"

Or can it be that that fact is a first indication of effective action by eugenists? Certainly there is not much else to indicate that they have made measurable strides in any direction.

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## DISCUSSION AND CORRESPONDENCE

### A NOTE ON THE CULTURAL AFFILIATIONS OF NORTHERN MEXICAN NOMADS

Dr Ralph Beals has rendered students of the ethnology of Mexico and the Southwest an important service in his interesting and stimulating paper, "The Comparative Ethnology of Northern Mexico Before 1750"<sup>1</sup> As those who have seen the study will remember, Dr Beals goes to Spanish sources for the recovery of important material, and marshalls his data to divide northern Mexico into tentative culture provinces. By a statistical treatment of the traits found within their boundaries he compares these provinces with each other, with southern Mexico, and with the Southwest and Southeast of the United States, analyzing his tables to offer valuable suggestions concerning the probable paths of diffusion of the region and the possible routes by which southern Mexican influence might have reached the Southwest.

Among Dr Beals' comments and conclusions there are one or two which seem to run counter to the writer's field experience, and since the questions raised have bearing on central problems of Dr Beals' paper, it might be well to make a brief statement concerning them.

One of the provinces which Dr Beals has described he calls that of the non-agricultural nomads, who occupy the region of the northern plateau. Included among the aborigines of this province are the Mescalero Apache and Chiricahua Apache, both of whom this writer has studied in the field.

Unfortunately, Dr Beals has identified the Chiricahua Apache with the Western Apache. The latter term should be reserved strictly for the Apache groups now stationed upon the San Carlos and White Mountain reservations in Arizona (White Mountain or Coyotero Apache, Tonto Apache, San Carlos Apache, and Cibecue Apache). None of these Western Apache groups lived in Mexico, and they are to be sharply differentiated from the Chiricahua Apache whose cultural and linguistic affiliations are rather with the Mescalero. Dr Harry Hoijer, who has done linguistic work among these peoples, classifies Chiricahua and Mescalero as dialects of the same language, much closer to each other than either is to Western Apache. In many major particulars of culture there is a decided cleavage between the Mescalero-Chiricahua usage on one hand and Western Apache custom on the other. Thus the Western Apache have a strongly developed clan system, while the Mescalero and Chiricahua have no sibs at all. The Western Apache have an important agricultural complex; the Chiricahua and Mescalero pay slight attention to the raising of crops. Wherever a major cultural difference is noted between the Western Apache and the Mescalero, we can be almost certain that the Chiricahua will conform to the Mescalero rather than to the Western Apache practice. For example, the Western Apache tell an origin myth of emergence from an underworld, whereas the Mescalero do not. True to our rule, the Chiricahua agree with the Mescalero and not with the Western Apache in this, as well as in so many other particulars.

<sup>1</sup> *Ibero-Americana*, 2, Berkeley: University of California Press, 1932.

This confusion of Apache tribes is not of special import, however, except that if we attempt to characterize the cultures of northern Mexico, we should know of which tribes we are speaking.

More pertinent to our discussion is Dr Beals' evaluation of the culture content of this province of the nomads. Dr Beals takes 116 basic traits found from southern Mexico to the Southwest and Southeast and indicates their occurrence in the various culture provinces. His table shows that 89 of these 116 traits have been recorded for southern Mexico, 62 for the Southwest, 50 for the Southeast, and only 31 for the northern nomads.

From the paucity of traits recorded for the nomads Dr Beals draws the inevitable conclusions. No sooner has he arranged some of the material in tabular form than he remarks, "Table 1 is an effort to make this situation more graphic. It shows clearly the relative richness of the culture of the agricultural peoples compared with that of the nomads."<sup>2</sup> Again he says, "Even with the slight collection of data given in Table 1, it is evident that the solution to the problem of connections and transmissions between southern Mexico and the United States must be sought among the agricultural peoples. It is also among the agricultural peoples that the greatest differentiation occurs."<sup>3</sup> Later he states, "It seems a fairly certain assumption that what transmissions occurred between the two regions rarely if ever went directly across the north Mexican desert plateau, but went around it."<sup>4</sup>

Few will deny the persuasiveness of the suggestions Dr Beals makes from the data in his possession. The whole question is whether or not those data are complete and trustworthy enough to permit of even a "fairly certain" assumption which rules out the northern plateau as a possible route of culture diffusion. Upon looking over the list of 116 traits in Dr Beals' Table 2, the writer was amazed to find that to the 31 traits recorded for all the tribes of nomads by all the authorities utilized by Dr Beals, he could add 30 more traits from his study of two of these tribes. The list of these additions is as follows:

Turquoise	Thatched Roofs	Rain Ceremonies
Sling	Gourd Utensils	Ceremonial Fire
Shield	Caps	Ceremonial Trees
Mask	Kicking Race	Incense
Captive Eagles	Painted Clothing	Palisades
Tattooing	Tambourine Type Drum	Spear-Thrower
Musical Bow	Infanticide	Pottery Drum
Patole (Dice or Stave Game)	Pinole (made of mesquite)	Maize Cultivation
Cairn or Wayside Shrine	Food Offerings	Squash Cultivation
Poisoned Arrows	Vision and Dream <sup>5</sup>	Irrigation

<sup>2</sup> Page 136

<sup>3</sup> *Ibid*

<sup>4</sup> Page 147

<sup>5</sup> Dr Beals indicated that the trait (though unrecorded), was probably present in the area. In that he is correct

The first 20 of these traits should be added to Dr Beals' table without reservation. The validity of the last 10 is more doubtful, and it may be that some of them should be eliminated. In respect to the first 6 of the 10 doubtful traits it is not always clear that what the writer found among the Chiricahua or Mescalero corresponds exactly to the trait Dr Beals had in mind when he compiled his list.

There is the case of rain ceremonies. The Apaches certainly performed ceremonies to bring rain, but they were not often connected with crops. A storm was more likely to be desired for purification after widespread sickness, to retard an enemy, or to replenish the drinking water supply. Is this to be equated with the rain ceremonies of the far south and Southwest?

What of the ceremonial fire? At the Apache four-day girl's puberty rite a fire is supposed to be kindled with a fire-drill by either the girl or the shaman who sings for her, and it must be kept burning throughout the ceremony. Does this suggest connection with the sacred fire of a temple?

Again, trees were a source of supernatural power to these Apaches. The particular tree from which or under which the individual gained his supernatural experience might be singled out and named in songs. Beliefs clustered about certain trees and bushes. Some were known to be "witched," those who ate from their fruits would be poisoned. How close are these concepts to what Dr Beals has in mind?

Sages, other pungent smelling plants, and animal parts were thrown on hot coals to create smoke and odor. Any person who had had anything to do with the dead or with the death rites was obliged to purify himself and his possessions by bathing in that smoke. The practice was an integral part of many rites, and a specific for headache, nosebleed, and other ailments. Shall this be called use of incense?

The Apaches often surrounded their individual camps with a rude fence of cactus and utilized the sharp-pointed yucca plant for the purpose of enclosing and protecting the camp, too. To some this may not seem elaborate enough to be termed a palisade.

Many may be surprised to see the spear-thrower included in the list of additional traits. The writer hastens to add that the Apaches do not have the conventional spear-thrower. What the Mescalero do utilize is the principle of the spear-thrower. Men used to wear a rope tied to the right wrist and carried a short spear notched about midway along the shaft. To use the spear, the rope was slipped around this notch in such a manner that it became loosened as the spear was propelled forward. This device allowed the thrower to clasp the spear near the point, while the rope stretched backward and was attached to the spear at the notch to the rear. Thus when the spear was hurled the guidance of the rope gave the effect of artificially lengthening the arm. The writer has seen improvised spears thrown long distances with force and accuracy by this means. Now it is evident that while the Mescalero did not receive the spear-thrower in form, unless we are to accept this as an independent invention, we must agree that the idea and principle of the atlatl did penetrate to them.



The difficulty in placing these traits and deciding whether or not they belong on our list involves a critique of a statistical approach to anthropological data which assumes that a trait in one culture is the counterpart or equivalent of a formally similar or comparable trait in another culture. These difficulties are multiplied when the trait in question does not fall under the heading of material culture and is consequently not always easy to define in formal terms. But such a critique is too involved a subject with which to burden a brief statement and will not be attempted here.

In regard to the last four items listed there is no doubt of their existence before reservation days. The question is one of their age: did these agricultural traits and the pottery drum which is generally associated with them exist for these Apaches prior to 1750 as Dr Beals' paper would require if they are to be considered here?

It may seem strange to speak of the agricultural traits of nomads. Nevertheless, the manner in which the Apaches carried on the small amount of crop-raising in which they engaged offered little hindrance to their roving life. They planted seed in some sheltered spot, came back once or twice to pull the worst of the weeds, and returned again for the harvesting. If water was nearby and easy to divert into the fields, irrigation was practiced. Not all families raised crops, in fact very few did; but as far back as the memories of the old men now living can carry us, a little agriculture was carried on in this fashion. It may well be that agriculture was begun among these people after 1750. But there seems to be no way to decide the question with finality now, so the traits are listed with the above reservations. Incidentally, if proof should be forthcoming that the traits do antedate 1750, it would somewhat modify Dr Beals' picture of a non-agricultural northern province.

When the 30 traits listed above are added to the 31 to which Dr Beals found reference, the alleged meagerness of the nomad culture largely vanishes. The combined 61 traits of the nomads compare very favorably with the 55 positively recorded for the Jalisco-Tepic province, the 56 of Old Sinaloa, the 63 of Old Sonora, the 62 of southwestern United States, and the various totals mentioned for other provinces at the beginning of this article. One may eliminate a number of the doubtful cases and still have an impressive total left for the nomads.

There is no reason to think that the Apaches possessed a richer culture than the other nomads of northern Mexico. When the writer was compiling a library thesis concerning the Apaches some years ago he read through volume after volume of old source material which supposedly dealt with these nomads or included observations concerning them. He found few references to any of the traits which are given above. He did find that it was an almost invariable rule for travellers, clergymen, and officials to dismiss nomadic peoples with a few contemptuous and harsh epithets, and that governments were formerly more concerned with exterminating these wandering tribes than with preserving their lore. It is not too daring to guess that the lack of references to traits of the northern nomads which Dr Beals noted is not so much due to the absence of the traits as it is to this discrimination and selective process. To this must be added the inherent difficulty which those who are not professional ethnographers will have in discovering any but the most obvious traits among

nomadic peoples. If special regard is constantly paid by a settled people to a tree near a temple, it is not so difficult to ascertain and note this. It is another matter to obtain reliable data on the attitude of nomadic peoples in regard to trees.

Nor must it be assumed because these Apache tribes are believed to have come originally from the north, that the additional traits are some which show mostly northern affiliations and could not be duplicated for other northern Mexican nomads. Of the 30 additional traits which have been enumerated, 24 are also listed for southern Mexico, and Dr Beals indicates his belief that a twenty-fifth, the poisoned arrow, existed in southern Mexico too. Of the 24 additional traits which the nomads and southern Mexico have in common, 18 are also to be found in southwestern United States.

It may make things a bit more graphic if the writer uses Dr Beals' own method. Dr Beals arranged a table (Table 3) which he named "Cultural Connections." He reduced the number of basic traits under consideration from 116 to 104. He sought to show the number of these 104 traits recorded for each culture province and the number and percentage of its traits which each province shares with every other province.

Dr Beals found that 78 of his 104 traits had been recorded for southern Mexico. He found according to his sources that only 17 of these 78 traits of southern Mexico were also common to the northern nomads, or a scant 22 percent. But in examining these 78 southern Mexican traits we can add 22 more to the 17 which Dr Beals credited to the nomads, and the total is brought up to 39 traits which southern Mexico shares with the nomads, 50 percent of southern Mexico's 78 traits. Compare this to the 56 traits (71 percent) which southern Mexico shares with the Jalisco-Tepic area, the 43 (55 percent) with Tepic-Culiacan; the 38 (48 percent) with Old Sinaloa; the 40 (51 percent) with Old Sonora; the 42 (54 percent) with southwestern United States, the 55 (70 percent) with Southern Sierra; the 17 (22 percent) with Central-Agriculturists, the 25 (32 percent) with Northern Sierra, the 22 (28 percent) with Tamaulipas; the 33 traits (42 percent) which southern Mexico shares with southeastern United States; and it is apparent, so far as present evidence indicates, that the northern nomads or the northern plateau cannot be ruled out as a possible cultural avenue between southern Mexico and southwestern United States. The writer does not mean to insist now that the nomads must have been important in the transmission of cultural elements to the Southwest, but he does mean to imply that the statistical proof that the nomads could not have acted in such capacity is most inconclusive. A study of the Lipan Apache which the writer hopes to initiate soon should give us much more information on the point.

In conclusion it might be said that even a stronger case for the influence of southern factors upon the northern nomads could be built up with little effort. There are a number of traits suggestive of the south (such as shooting fish with barbed arrows and use of the enema tube) which the writer has recorded for the Apache tribes, but which are not included in the list of traits used by Dr Beals for his calculations.

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## PETROGLYPHS SHOW THAT THE ANCIENTS OF THE SOUTHWEST WORE MASKS

An article by White in a recent number of the *AMERICAN ANTHROPOLOGIST* gives a brief discussion of "Masks in the Southwest,"<sup>1</sup> to which the following should be added.

Many pictographic groups in the Uintah Basin area, in northeastern Utah, show actors wearing animal-head masks, exhibiting the horns. A group on Ashley Creek, near Vernal, which we photographed as P87, is described by the writer as follows:<sup>2</sup>

The central upper figure of the lower group is that of a man wearing a bird for a headdress. The figure at the left of the picture is not masked and is probably the man of ceremonies. This man attracts notice in that he is wearing his hair in side bobs. One man of the group is wearing a buffalo skin with horns for a headdress. Another has a horned headdress which extends from his head on each side in comb fashion. There are three human beings that are masked to represent some beast or insect. The whole group probably is an animal dance scene much like the animal dance scenes that can be observed in the Southwest at the present time. And again it is quite possible that it is a death-dance scene involving images of the dead (the upper scene of this picture was of hand effigies), similar to those described by Kroeber<sup>3</sup> as held by the Maidu (pp. 431-432), the Kawaiisu (p. 604), the Tubatulabal (p. 609), and the Kitanimuk (p. 613), the circles (with eyes, of the upper group) being crude images of the dead people for whom the ceremony is being conducted.

Masked actors are shown in several pictographic groups in Nine Mile Canyon, east of Price, Utah. The men are usually shown as wearing horned headdresses, as described by the writer under pictographs N-P7, N-P43, and N-P47 and corresponding cuts in Figure 4.<sup>4</sup> These masked men in the Nine Mile Canyon pictographs are dubbed "Cornute Men" by Morss,<sup>5</sup> the writer's N-P7 masked man being figured by him in Plate 13, b, and described under his "Nine Mile Canyon Pictographs,"

<sup>1</sup> Vol. 36:626-28, 1934.

<sup>2</sup> Albert B. Reagan, *The Pictographs of Ashley and Dry Fork Valleys in Northeastern Utah* (Transactions, Kansas Academy of Sciences, Vol. 34), p. 199.

<sup>3</sup> Handbook of the Indians of California (Bulletin, Bureau of American Ethnology, 78, 1925).

<sup>4</sup> Albert B. Reagan, *Anciently Inhabited Caves of the Vernal (Utah) District, with some additional Notes on Nine Mile Canyon, Northeast Utah* (*Ibid.*, Vol. 36), pp. 62-63. The N-P7 group is also described in other reports of the writer on this region, as follows: *Some Archaeological Notes on Nine Mile Canyon, Utah* (*El Palacio*, Vol. 31, No. 4:45-71, Santa Fé, July 29, 1931) and in *Some Notes on the Picture Writing North of Mexico* (Bulletin, Wagner Free Institute of Science of Philadelphia, Vol. 7:38-54, November, 1932) where a photograph of group N-P7 is the upper figure on page 42.

<sup>5</sup> Noel Morss, *The Ancient Culture of the Fremont River in Utah* (Papers, Peabody Museum of American Archaeology and Ethnology, Vol. 12, No. 3, 1931). Also see Julian H. Steward, *Petroglyphs of California and Adjoining States* (University of California Publications in American Archaeology and Ethnology, Vol. 24, No. 2, 1929), p. 217, for a discussion of katchina-like pictographs; Jesse Walter Fewkes, *Hopi Katchinas* (Twenty-first Annual Report, Bureau of American Ethnology, pp. 3-120, 1903) for types of Hopi katchinas.

pp. 40-41. He also found cornute pictographs in the Fremont River area in Utah and northward at many other places along Green River and its tributaries to the Nine Mile Canyon country (pp. 34-42, pls. 13-18)

It might be argued that these animal headdresses were not masks in the sense of this discussion, though there are glyphs which seem to show that the skin covered the face and that holes were cut through it for peep holes. But to look further, there are pictographic scenes which show unmistakable masks. Several groups which Morss examined at Fruita, Utah ("Dancer Pictographs," p. 34, pls. 15-16) showed unmistakable masks. Morss states (p. 35), "The face may be left blank or the feature roughly indicated or a mask may be shown," and in his conclusion on the pictographs of the region he further states (p. 42), "The Fremont anthropomorphs seem to have been developed from Basket-maker prototypes and indicate the personification of supernatural beings in forms similar to those now familiar in the Southwest." Steward also mentions the glyphs of the Fruita-Thompson area in Utah (p. 152, pls. 81-82): "One type of the figures (found in the Fruita region . . .) is closely similar to the katchina dance costumes of the Pueblo Indians;" and the figures he exhibits on the two plates are all of the Fremont type of culture.

Concerning the age of these pictographs we note that the glyphs of P87 in Ashley Valley are probably of Basket Maker age, while those of Nine Mile Canyon which were examined by Morss and the writer are of the Fremont culture stage, as are also those from Fruita. These all have the characteristics of the Fremont culture. Morss holds this culture flourished in Pueblo II times, stating in his introduction "The presence of small amounts of black-on-white and corrugated pottery, with other evidence, showed that this complex [the Fremont culture stage] was contemporary with Pueblo II in other regions" (therefore not later than 900 A.D.). This conclusion was arrived at simultaneously by the writer on the basis of finds in the Uintah Basin (as given in the articles mentioned above). So the "rock writings" show beyond doubt that masks were used in Indian katchina ceremonies before the coming of the Spaniards.

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### THE BULBED ENEMA SYRINGE IN NORTH AMERICA

In Appendix 1 of Comparative Ethnological Studies, Volume 8 ("Modifications in Indian Culture through Inventions and Loans," 1930) the late E. Nordenskiöld brought together the relevant data and discussed the use of enema tubes and syringes among the American aborigines. He expressed the opinion that "the peculiar idea of using enematic injections has probably been independently invented both in the Old World and the New." Two types of devices for this purpose are distinguishable. One has a bulb and the other has not. "The bulbless type is known from N. W. North America to the Chocó in Colombia, and *the bulbed one from large tracts of South America*" (italics ours).

It is the purpose of this brief note to call attention to the use of the *bulbed* type of enema syringe by certain North American natives, a fact overlooked by Norden-skjöld, but one which nicely fits into the series of culture traits compiled by him which occur north of Mexico in North America and in South America but, in the intervening regions, sporadically, if at all.<sup>1</sup> The positive information which has come to my notice is confined to Algonkian and Siouan speaking people and, for these groups, only from a few localities and tribal units. Systematic inquiry in the field and information recorded in documentary sources may reveal a much wider distribution of the bulbed syringe.

A. Skinner reported the bulbed enema syringe for the Eastern Cree.<sup>2</sup> In the course of field work among the Norway House and Cross Lake Cree (Manitoba) in 1930, I found the same device in use. The bulb was made of a jackfish (*Esox lucius*) bladder and the tube of the leg bone of a bird (?).

Dr J. M. Cooper made inquiries regarding the enema syringe among the James Bay Cree, but failed to secure affirmative information. At Atawapiskat (100 miles north of Fort Albany), however, a type in which a small cotton bag is used was known. The native name for it was sisobátcigan, "thing or instrument for squirting water." Dr Cooper says that his informant was doubtful whether the device was of aboriginal or European origin.<sup>3</sup>

Among Saulteaux-Ojibwa groups the enema syringe seems to be a well-known device. A. Skinner,<sup>4</sup> F. Densmore,<sup>5</sup> and H. H. Smith<sup>6</sup> refer to it and Baraga records the term pindabawadjigan, "clyster-pipe," in his Dictionary.<sup>7</sup> This is the term by which the bulbed enema syringe is known among all the Saulteaux groups east of Lake Winnipeg which I have visited, and its cognate is employed by the Norway House and Cross Lake Cree. The Berens River Saulteaux, like the latter, use a jackfish bladder. The tube is made from the wing bone of the Canada goose and by means of a sinew wrapping is attached to the bladder. The same device is known to the Hollow Water River band to the south and to the Island Lake band to the north-east. H. H. Smith likewise refers to the use of the bulbed syringe among the Meskwaki (Fox).<sup>8</sup>

<sup>1</sup> Table I, Comparative Ethnological Studies, Vol. 9 (1931) and Appendix 1. The latter had previously appeared in Ymer (1926) under the title "En jämförelse mellan indiankulturen i södra Sydamerika och i Nordamerika."

<sup>2</sup> Notes on the Eastern Cree and Northern Saulteaux (Anthropological Papers, American Museum of Natural History, Vol. 9, 1911), p. 77. Skinner cites p. 190 in the 1795 edition of Hearne's "Journey" in connection with his own observation, but Hearne makes no reference to an *enema syringe*. What he does describe is the practice of *blowing* into the anus of a patient.

<sup>3</sup> Personal letter.

<sup>4</sup> *Op cit.*, p. 161.

<sup>5</sup> Uses of Plants by the Chippewa Indians (Bulletin, Bureau of American Ethnology, 44, 1928), pp. 331-32.

<sup>6</sup> Ethnobotany of the Ojibwa Indians (Bulletin, Public Museum of Milwaukee, Vol. 4, 1932), p. 342.

<sup>7</sup> R. P. Bishop Baraga, A Dictionary of the Ojchipwe Language (Montreal, 1878), Part 1.

<sup>8</sup> Ethnobotany of the Meskwaki (Bulletin, Public Museum of Milwaukee, Vol. 4, 1928), p. 219.

For Siouan speaking groups, M. R. Gilmore<sup>9</sup> has recorded the use of an infusion of the bark and root of the Kentucky coffee-tree ((*Gymnocladus dioica* [L.] Koch) as a rectal injection for constipation among the Dakota, Omaha, Ponca, Winnebago, and Oto. "This remedy was used from time immemorial," he says. "Prior to contact with Europeans the Indians made their own syringes, an animal bladder being used for the bulb and a hollow cylindrical bone, as the leg bone of a prairie chicken, turkey, goose, or other bird, was used for the tube. The bulb was attached to the tube by sinew wrapping."

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<sup>9</sup> Uses of Plants by the Indians of the Missouri River Region (Thirty-third Annual Report, Bureau of American Ethnology, 1919, pp. 43-154), p. 89.

## NOTES AND NEWS

### ANNUAL MEETING OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

The annual meeting of the association will be held at Phillips Academy, Andover, Massachusetts, on December 27-29, 1935 (Friday to Sunday, inclusive).

### MEXICAN LANGUAGES

The "Instituto Mexicano de Investigaciones Linguisticas" (Universidad Nacional de Mexico) "plans to publish successively, as its financial means may permit the most complete and recent dictionaries and grammars of the numerous indigenous languages still spoken in this country, as well as dialectical studies of the Spanish language, either reediting in critical editions our most representative works thereon, or reproducing the already existing monographs on regional language."

### GRANTS AND FELLOWSHIPS OF THE SOCIAL SCIENCE RESEARCH COUNCIL

It is announced that applications for grants-in-aid in support of research during the period between April 1, 1936 and April 1, 1937 must be filed before January 15, 1936, on forms to be secured from the Secretary for Grants-in-Aid, Social Science Research Council, 230 Park Avenue, New York City.

Among the awards for 1935-1936 are: Otto Klineberg for a study of emotional expression among the Chinese; Ronald L. Olson for a study of cultural changes involved in the shift from paternal to maternal descent among the Haisla and Heiltsuk of British Columbia; Julian H. Steward for a study of the ecological aspects of Shoshoni society.

Applications for Post-Doctoral Research Training Fellowships and Pre-Doctoral Field Fellowships are to be submitted to the Fellowship Secretary by December 1, 1935, and for Pre-Doctoral Fellowships for Graduate Study by March 15, 1936.

A NEW SERIES, "GENERAL SERIES IN ANTHROPOLOGY," has been issued, offering an inexpensive vehicle for the publication of monographic material. There have appeared No. 1—The Prophet Dance of the Northwest and its Derivatives: the Source of the Ghost Dance, by Leslie Spier, and No. 2—Taos Pueblo, by Elsie Clews Parsons. (Leslie Spier, editor: George Banta Publishing Company, Menasha, Wis., agent.)

EDWARD H. THOMPSON, famous for his long residence in Yucatan and his pioneer archaeological explorations there, died in Plainfield, New Jersey, on May 18, 1935 at the age of seventy-five. He was the friend of all the early investigators in Yucatan and his ownership of Chichen Itza made him a well known figure to all Maya students. He was American Consul in Yucatan for almost twenty-five years, combining

these services with archaeological investigations. His principal work was at Chichen Itza where he discovered the "High Priest's Grave," the famous tablet of the Initial Series, and uncovered the "Temple of the Painted Columns." He discovered several new sites in the interior of Yucatan and knew much of the folklore of the present natives.

Collections gathered by him are in the Field Museum in Chicago and in the Peabody Museum, Cambridge. The remarkable collection of objects dredged from the Cenote of Sacrifice at Chichen, now in the Peabody Museum, will always remain a monument to his indefatigable zeal and to his desire to preserve for all time something of the life of the Mayas whom he loved.

A M T.



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narrow purpose with that of the author and either approve of the method and say so or accept it in silence, for there is nothing legitimate to be said against the aim of the author to produce a sturdy introduction to anthropology presenting essential facts topically and deliberately avoiding theoretical discussion. And he has consummated his endeavor with an art, a lucidity, originality, and grasp of the bewildering difficulties which beset the novice, all his own. As individual modes of reception, even tastes, often vary widely, the method of approach in a new draft of the old stock material of anthropology will appeal to all who are teaching as well as learning the subject, and the volume will settle down to its deserved place on the shelf of text books, a place of the first rank. It should be no defection from the highest standard of critical judgment to say that the teaching anthropologist will find the book a positive delight and a help.

The orderly array of instances of custom, the aptness of illustrations from economic, social, and religious life, are a model combination of the comparative, the historical, and the analytical methods of dealing with ethnology; a combined source book and intellectual treatise. The author's wide experience in the realms of social and religious science and his understanding of student needs show forth in vigorous measures

There are, however, some confidences to exchange between author and reviewer, and here is one place to present them.

Some confusion in representing ideas of higher and lower racial status, mental traits, brain size and faculties, is apparent in the following statement (p. 7).

If races are to be ranked as higher and lower, it must be for their mental traits. Fair hair and long heads have no intrinsic value, though conceivably they may be *signs* of superiority. The only physical feature that directly suggests mental worth is the brain because in the animal kingdom a better brain does go with a higher status.

Here, however, there is again overlapping. Mongoloids are not inferior to Caucasians at all; Negroes somewhat; Australians more so, yet without falling before the more poorly equipped whites. There are really groups with larger and smaller brains, just as there are taller and shorter groups.

Nevertheless, this result cannot be interpreted directly in terms of psychology. While very great differences in the brain are significant, lesser ones are not. In civilized countries autopsies sometimes prove criminals to have large brains while some great men were found to fall below the normal. A difference of merely a few hundred units is thus not decisive, and the smaller brains of Australians or other races are no positive proof of inferior faculties.

Something akin to equivalent racial status is clear to Dr Lowie's mind, which never fails in clarity of judgment, yet here there is mist in conveying conclusions as to superiority-inferiority to minds untrained in the grading of human groups. Probably no one could do better in a more purely objective attempt at analysis. Yet one might expect an authority as potent in the anthropological court to avoid moulding opinion by "silently" advocating a graded rating of races until a solution can be predicted that will square with the full verdict of scientific judgement. It seems to one who professes no prejudicial convictions that to quote authorities on this point might have been preferable to stating it as is done. And it might also be

inserted here that the absence of reference in other instances to authorities where there are two sides to theoretical questions is noticeable. Sanction could be found for applying this comment also to paragraph 2 and the end of paragraph 4 on page 9.

Again a note of professional weariness, when encountering the conventional classification of culture periods (tabulation, p. 11) based solely upon an inventory of mechanical inventions and economic traits. The gradual sanitation of professional thought along lines of cultural classification from savagery to recency is finding its interests turning to consideration of other inventions than those concerned with minerals and machines. Strange that Dr Lowie should pass by the fields of social and religious science, which he has made so closely associated with his name, when tabulating culture periods sequentially as though he did not yet accept the social structure of the Australians to be a "sign" (p. 7, paragraph 2) of progressiveness more weighty than the simple status of their industry, or the religious complexity, even richness, of the Maori to be as indicative of cultural advancement as their ignorance of the metal age is, according to old methods of classification, an index of their primitiveness. Some of us will therefore find it difficult to understand Dr Lowie's mood in respect to the use of criteria in the classification of culture periods. We would ordinarily picture him as a spokesman for the adoption of a scale based upon mental rather than material achievements, in terms perhaps bolder than most of us who are convinced of its significance, and certainly more eloquent. Yet while we remind ourselves that the book is not prepared for critical expert theorists, the fact remains that student readers seeking light from a new source inevitably imbibe sense-judgment, often more positive than the author intends it to be, from the nature of his treatment of moot points. Since the audience for which the book is professedly intended is the non-expert audience, impressions made may always be safeguarded by an extra dose of caution. I am not at all sure that I could, for instance, avoid discussing the racial equation without bias myself.

Dr Lowie several times surprises us by stressing motives for human customs conventionally without regarding what we know, through recent findings in various areas, to be motives partaking largely of a spiritual nature, as art (p. 179-81), scalping, torture, cannibalism (p. 225), except for what is said in another section (p. 228).

The chapter on marriage and the family (XIII) leads from treatment of simple up to complicated forms of mating by gradual degrees and so clearly that nothing could be desired to prepare the way for the student whose first experiences with primitive social forms is so often found to be confusing. That the definition (p. 239) of perplexing marriage regulations among the extinct Tamanak of the Orinoco is self-explanatory to a layman upon whom the experiment was tried, after having read over just once the preceding four pages, is testimony to the clarity of style and arrangement of thought.

A final comment: I only wish that the author had also seen fit to treat as a selected area the tribes of eastern and northeastern North America, not so much for the benefit of students of general anthropology but to contribute his sagacity to a field where I for one am ready to admit the need of aid. In probably the most charming



piece of exposition in all his career Dr Lowie has set down the inevitable logic of elementary anthropology.

FRANK G. SPECK

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*Patterns of Culture.* RUTH BENEDICT. (xiii, 291 pp. \$2.50. Boston and New York: Houghton Mifflin Company, 1934.)

Dr Benedict's book is done with the insight, dignity, and charged style that we have come to expect of her writings. It is an important contribution, in regard to which, however, two considerations must be kept in mind. First, it is a work for the intelligent non-anthropologist, not for the anthropologist. Second, as a book, it deals with culture patterns in the wider sense of the word, not primarily with the psychiatrically-delineated "configurations" which she discussed some years ago in the *AMERICAN ANTHROPOLOGIST*.

The basic concept is that of culture occurring in certain patterns which determine its fabric and are of influence on the lives led by all individuals under a culture. These patterns make up the character or distinctive quality of each culture; its "genius," to use an old phrase aptly reused by Dr Boas in the preface. This quality of course inheres largely in forms, or interrelations of forms; it can never be adequately formulated in terms of culture content alone. Nor can it be measured or demonstrated. Essentially, it is seizable and definable by subjective empirical approach. The estimation of the relative weight of the patterns in a culture, for instance, must be done primarily by feeling; and their documentation is of the sort which substantiates an a priori, synthetic apperception, whose validity depends on the fit of the pattern parts and their leaving no significant remainder of the culture undealt with. No approach is farther than this from the customary analytic one of Boas; and it speaks for the catholicity of the group of which he is leader that the present work is from the pen of a many years' associate. Those who will, may quarrel with the approach as "unscientific." They would have to quarrel also with Burckhardt's "Renaissance," Bryce's "Commonwealth," Redfield's "Tepoztlan."

Very considerably, the book is propaganda for the anthropological attitude, as entertained fervently by Dr Benedict, to the audience of cultivated intelligence generally. Hence there are passages that deal with concepts which to professional anthropologists have become somewhat thread-bare: the insufficiency of racial heredity to explain differences of culture, for instance (pp. 233-36). This is not a stricture. With the objective of the book what it is, such passages are appropriate and necessary.

In her seventh chapter, on the nature of society, the author argues explicitly that "we do not need a plank of configuration written into the platform of an ethnological school" (p. 229). However, more than half the text (166 pages of 278) is given over to a picturing of Zuñi, Dobu, Kwakiutl from the angle of the semi-psychiatric configurations which Dr Benedict was the first to develop. Despite her warnings, therefore, there is likely to be a residuum of misimpression that her gen-

eral concept of patterns means nearly the same thing as her special type of configurations. Her depiction of the two American cultures is worked over, not a reprint from the article in this journal. The third, on Dobu, is of course wholly new. Incidentally, with its compression from a book into a chapter, the effect of Dobu comes out even harsher than in Fortune's hands: the culture is pathological to the point of repellence.

Throughout, the book has the quality of distinctive, almost passionately felt, balanced thinking precisely expressed.

One paragraph may be pardoned the anthropologist. The reviewer does not feel that Dr Benedict has carried her configuration approach too far: rather it remains to be developed farther, now that this book is done. She points out that the Kwakiutl are specialized in their megalomania: the southern Salish have an individualistic culture, the interior Salish an uncoordinated one. She shows illuminating contrasts to the Pueblos among Pima, Navaho, Plains tribes at special points. Are not these cultures worth treating systematically from the same point of view? We have full enough data on most of them. What matter if their configurations come out less dominated by a single attitude? They are just as important. The psychologist does not study only accentuated psychoses or the most extreme personalities. Such may serve best for an entry into a new field; but the goal of understanding is normality in its diversity. Every culture must have its "genius;" to know any one is a contribution to the understanding of how human culture acts. If her studies of Zuni and Kwakiutl continue to remain isolated, their meaning will be uncertain, their validity will be questioned. The reviewer hopes she will push farther, into more resistant material, and give us new stimuli and insights.

A. L. KROEBER

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*Thoughts, Talks and Tramps.* EVERARD IM THURN. Edited with a Memoir by R. R. Marett. (xxiii, 285 pp., frontispiece, map. \$4.00. London: Humphrey Milford, Oxford University Press, 1934).

This collection of thirteen intimate and entertaining articles, most of which are reprinted from obscure publications, contains a wealth of first-hand ethnological observations regarding the natives of British Guiana and the islands of the South Pacific.

The twenty years spent in British Guiana as curator of the museum at Georgetown and later as magistrate and government agent of the Northwest Province gave to Im Thurn an intimate knowledge of native customs which accounted for his unparalleled success as an administrator of native affairs. In subsequent years when he held the important positions of Governor of Fiji and High Commissioner of the Western Pacific, Im Thurn's regime stands as the classic example of applied anthropology.

The present articles reflect his great interest in the problem of the effects of white contact upon native cultures but are of equal interest to those less philosoph-

ically inclined because of the wealth of ethnological detail which they contain, much of which, Im Thurn explains, is recounted as written in the field. The book contains much more solid information of interest to the anthropological student than its rather general title would imply.

M. W. STIRLING

BUREAU OF AMERICAN ETHNOLOGY

*Sex and Culture.* J. D. UNWIN. (xxiii, 676 pp. \$12.00. London: Oxford University Press, Humphrey Milford, 1934.)

The thesis of this voluminous work is that cultural energy in all human societies is the result of restrictions upon sexual behavior. The more stringent these have been, the higher the culture has risen in the scale. The author has studied eighty primitive societies, being guided, he states, only by the character of the descriptions available, and he concludes that there is an invariable correlation between the degree of sexual restriction and cultural achievement.

In order to attain this absolute correlation, he has had to manipulate his definitions both of sexual restrictions and of cultural achievement. His correlations, in fact, only concern the limitation of pre-nuptial freedom in women and the nature of religious rites. It is never quite clear why he regards pre-nuptial restrictions as being so much more dynamic than post-nuptial, but restrictions upon the sexual opportunities of women are more desirable than upon those of men, he says, because women are more important in child rearing.

In defining cultural achievement the standard is surprising. The lowest level recognized is that characterized by religion without post-funeral honor of the individual dead or without worship in temples, these two being the criteria of the middle and highest level of primitive cultural achievement. For a culture to rise from the lowest plane to the next higher level it is only necessary to restrict pre-nuptial freedom of women; to rise to the highest level, where they will be capable of building temples, it is only necessary to demand tokens of virginity. It is not necessary that the restrictions shall be enforced for all females in a society. Thus, Samoa has the necessary cultural energy because restrictions are imposed on one girl in the village, the taupou, and rates as a culture with the most stringent restrictions, whereas Zuñi, for instance, ranks as one with complete absence of restrictions, having, in the author's words, not even "irregular or occasional continence."

It is impossible within the limits of a brief review to criticize the long list of absurdities that are involved in the correlations in this volume. They can be indicated from the author's handling of American Indian material. No tribes of North America north of Mexico have, according to his definitions, either temples or ancestor cult, and must therefore have no restrictions upon sexual freedom. He has described twenty-five tribes from North America, but he has omitted without comment or excuse the entire area of the chastity belt. If, as he says, he was guided in his selection entirely by the excellence of the ethnographic material available, it

would have been natural to include at least the Menomini and the Cheyenne. The latter's prohibition of pre-nuptial sex life would of course have played havoc with his one-to-one correlation between high cultural status characterized by temples, and the existence of pre-nuptial restrictions.

The final chapter of the volume is entitled "Necessity in Human Affairs" and contains the laws based upon his correlations. Thus:

Any society in which complete pre-nuptial freedom (outside the exogamic regulations and prohibited degrees) has been permitted for at least three generations will be in the zoistic cultural condition (p. 347).

The moral in regard to our immediate cultural past is strongly stated:

As soon as . . . marriage and divorce by mutual consent became part of the inherited tradition of a complete new generation, the energy, either of the whole society or of a group within that society, decreased, and then disappeared (p. 412).

For the future he seems to consider it necessary that women shall be given legal status equal to men's in order that their situation may be bearable enough so that cultural energy is produced.

The volume is an extreme example of the manipulation of anthropological material to support private programs of social reform, in this case, a program of return to the immediate Victorian past. It makes clear, as has already been abundantly demonstrated in anthropological literature, that any thesis, no matter how unlikely, can be upheld by a suitable rearrangement of cultural facts from primitive peoples. Only insistence upon a greater scrupulousness and a greater intelligence can prevent the recurrence of such volumes of special pleading.

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*A Manual of Excavation in the Near East. Methods of Digging and Recording of the Tell en-Nasbeh Expedition in Palestine.* WILLIAM FREDERIC BADÈ. (vii, 81 pp., 14 illus. \$1 50. Berkeley: University of California Press, 1934.)

Considerable popular interest was aroused recently by articles in the press and semi-scientific publications which announced that ancient finger prints impressed in the soft clay by Palestinian potters as they shaped their vessels now served the archaeologist as "date-marks." This evidence, it was indicated, aided in determining the contemporaneity of occupation levels at various parts of a mound and also assisted in correlating assemblages of pottery found in tombs with the material from different layers. The expedition which contributed this item to the complex of archaeological procedure likewise developed and elaborated upon the technique of excavation and the methods of recording described by Director Badè in his little manual. In the introduction it is suggested that the methods described, although set forth in a Palestinian context, are applicable to archaeological enterprises anywhere.

Considered from the viewpoint of the Southwest, a number of the features

discussed are applicable to excavations in that region, while others are not. The main survey map, which is the basis for the system of recording, is the contour control type with superimposed grid, with which most present day field men are familiar. Conditions in the Southwest call for much smaller squares than those indicated in the Manual but the general principles described by Badè hold good. It would be advisable, however, to give even more specific location with respect to the identifying square of the peg than that indicated in the text. The section on the organization and management of excavation gangs is interesting to read, but most of the methods mentioned are not applicable to the Southwest because the laborers and the manner of working are so totally different from those in Palestine. In a broad way some of the suggestions concerning the staff, division of duties, etc., would be of help where a large undertaking such as that of Hodge at Hawikuh, Judd at Bonito, or Kidder at Pecos, was involved, but for the average expedition they are not germane.

Descriptions of the various methods of recording, of the handling of specimens, what to save and what to discard are complete and detailed. The techniques discussed are adaptable to the Southwest. As a matter of fact similar plans of procedure have been in use for a number of years. Suggestions of value will be found in the sections on the use of the camera and the subject of surveying and mapping. This is also true of the discussion of the problems of stratigraphy. It is in the latter connection that the finger-print dating is described, but from the text the reader gathers that the method is still in its experimental stages.

The Manual would be more of a contribution if it contained further details about additional items. The author refers to "knife and brush work" yet does not describe the process. The general reader would be interested and the beginner helped by an explanation of the technique involved. This is true also with respect to the question of skeletal remains. There is a discussion of the subject of finding and clearing of tombs but no mention of the way in which the bones were handled either for preservation or removal. Were tomb (grave) cards or form sheets, such as used by many American archaeologists, a part of the record or was the necessary information recorded in the director's journal? If special cards are provided a facsimile of one would add to the information in the Manual. It might serve as a guide for other excavators. Further, what is the technique of excavating a tomb, or a room, or a silo for that matter? Is the material removed layer by layer according to the nature of the strata, by levels based on arbitrary measurement, or as a homogeneous unit? Indications in the text are that the layer by layer or so-called onion-skin method was used on occasion. A brief description of the manner of procedure would be in order.

A paragraph on the director's journal, its form, style, and the system of recording features not otherwise taken care of by maps, specimen cards, and the registry book would enhance the Manual. The same may be said for the subjects of the treatment of perishable objects, the kinds of preservatives employed and how they are applied, the packing and boxing of objects for shipment. This additional information could have been included in the book without materially increasing its size and

would have made it far more helpful to beginners and students in archaeology. The items which are presented demonstrate that the Tell en-Nasbeh expedition was especially attentive to the problems of technique and suggest that features such as mentioned above probably received similar treatment. Consequently it is to be regretted that they were not incorporated in the text so that the publication could truly be considered a "Manual of Excavation."

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BUREAU OF AMERICAN ETHNOLOGY

*A Decade of Progress in Eugenics: Scientific Papers of the Third International Congress of Eugenics.* HARRY H. LAUGHLIN (ed.). (xi, 531 pp., 28 pls. \$6.00. Baltimore: Williams and Wilkins Co., 1934.)

This is the publication of the Eugenics Congress held in August 1932, at the American Museum of Natural History, and includes a total of sixty-five papers and a summary of the exhibits arranged at the museum for the members and guests of the Congress. It is a considerably smaller output than that of the 1922 Congress from which the two volumes: "Eugenics, Genetics and the Family," and "Eugenics in Race and State" resulted, and on the whole less provocative. Only a relatively few papers are devoted specifically to reports on human genetics; there is a small undistinguished section on method, and another on racial questions. The bulk of the discussion is devoted to a consideration of the various factors and agencies making for eugenic and dysgenic states in the population: the falling birth and marriage rates of the fit, the preservation in various ways of the maladjusted and inadequate, the effects of war, the biology of fertility, and the possible means at our disposal for effecting reform.

While there is almost nowhere in the volume an adequate analysis or appraisal of the evidence which is available on the nature-nurture problem, and which should be basic to any eugenics thinking (H. J. Muller's article on "The Dominance of Economics" is a notable exception to this statement and a penetrating criticism of the whole eugenics program), there is in a few papers a sober acknowledgement of our lack of any definitive knowledge on the nature and inheritance of mental disorder. The discussion by Pollock, Malzberg, and Fuller on the manic-depressive psychoses, and Dr Florence Powdermaker's article on the social factors in mental development are examples in point, stressing the need respectively for more careful methods of investigation and for a recognition of the environmental factors which may be operative in producing some of the vast hordes of mental deviates. There are also some interesting compilations of statistics on birth rates, marriage rates, a straightforward presentation of population statistics by J. H. Landman, and a few other items which are sound enough if not particularly stimulating.

It is the papers which deal with ways and means of reversing the proportion of eu- and dysgenic elements in the population, however, which most vigorously belie the title of the volume. Here one finds ready and unchallenging acceptance of the simple notion that mental disorders and defects are one and inherited, a per-

verse disregard for any evidence to the contrary, and the dangerously confused reasoning that if feeble-mindedness is not entirely inherited, then at least a feeble-minded environment is inherited and it makes very little difference anyway whether accurate distinctions are possible. Sterilization is variously brandished as a means for solving the unemployment problem, the crime problem, as a measure which must be "a valuable asset to humanity" since it possesses "a sufficient appeal to induce 50% of the States of the Union to adopt statutes permitting its use," and in one inspired article by L. K. Sadler as a method which if "enforced throughout the United States would result in less than one hundred years in eliminating at least 90% of crime, insanity, feeble-mindedness, moronism, and abnormal sexuality, not to mention many other forms of defectiveness and degeneracy." Such arrogantly sweeping figures need no comment from the more humbly ignorant among us.

On the positive side, that of increasing the eugenic elements of the population, there are still the tacit assumptions that the "better" classes are the carriers of the most desirable traits for the race, and that if somehow these better classes could be made to realize their responsibilities to the future they would marry and procreate eugenically. Dr Muller's comment is perhaps the most apt: "Is it to be wondered at that a census of eugenisists has disclosed an appalling failure to reproduce themselves, despite the fact that they are maximally steeped in their own doctrines?"

Or can it be that that fact is a first indication of effective action by eugenists? Certainly there is not much else to indicate that they have made measurable strides in any direction.

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## DISCUSSION AND CORRESPONDENCE

### A NOTE ON THE CULTURAL AFFILIATIONS OF NORTHERN MEXICAN NOMADS

Dr Ralph Beals has rendered students of the ethnology of Mexico and the Southwest an important service in his interesting and stimulating paper, "The Comparative Ethnology of Northern Mexico Before 1750"<sup>1</sup> As those who have seen the study will remember, Dr Beals goes to Spanish sources for the recovery of important material, and marshalls his data to divide northern Mexico into tentative culture provinces. By a statistical treatment of the traits found within their boundaries he compares these provinces with each other, with southern Mexico, and with the Southwest and Southeast of the United States, analyzing his tables to offer valuable suggestions concerning the probable paths of diffusion of the region and the possible routes by which southern Mexican influence might have reached the Southwest.

Among Dr Beals' comments and conclusions there are one or two which seem to run counter to the writer's field experience, and since the questions raised have bearing on central problems of Dr Beals' paper, it might be well to make a brief statement concerning them.

One of the provinces which Dr Beals has described he calls that of the non-agricultural nomads, who occupy the region of the northern plateau. Included among the aborigines of this province are the Mescalero Apache and Chiricahua Apache, both of whom this writer has studied in the field.

Unfortunately, Dr Beals has identified the Chiricahua Apache with the Western Apache. The latter term should be reserved strictly for the Apache groups now stationed upon the San Carlos and White Mountain reservations in Arizona (White Mountain or Coyotero Apache, Tonto Apache, San Carlos Apache, and Cibecue Apache). None of these Western Apache groups lived in Mexico, and they are to be sharply differentiated from the Chiricahua Apache whose cultural and linguistic affiliations are rather with the Mescalero. Dr Harry Hoijer, who has done linguistic work among these peoples, classifies Chiricahua and Mescalero as dialects of the same language, much closer to each other than either is to Western Apache. In many major particulars of culture there is a decided cleavage between the Mescalero-Chiricahua usage on one hand and Western Apache custom on the other. Thus the Western Apache have a strongly developed clan system, while the Mescalero and Chiricahua have no sibs at all. The Western Apache have an important agricultural complex; the Chiricahua and Mescalero pay slight attention to the raising of crops. Wherever a major cultural difference is noted between the Western Apache and the Mescalero, we can be almost certain that the Chiricahua will conform to the Mescalero rather than to the Western Apache practice. For example, the Western Apache tell an origin myth of emergence from an underworld, whereas the Mescalero do not. True to our rule, the Chiricahua agree with the Mescalero and not with the Western Apache in this, as well as in so many other particulars.

<sup>1</sup> *Ibero-Americana* 2, Berkeley: University of California Press, 1932.

This confusion of Apache tribes is not of special import, however, except that if we attempt to characterize the cultures of northern Mexico, we should know of which tribes we are speaking.

More pertinent to our discussion is Dr Beals' evaluation of the culture content of this province of the nomads. Dr Beals takes 116 basic traits found from southern Mexico to the Southwest and Southeast and indicates their occurrence in the various culture provinces. His table shows that 89 of these 116 traits have been recorded for southern Mexico, 62 for the Southwest, 50 for the Southeast, and only 31 for the northern nomads.

From the paucity of traits recorded for the nomads Dr Beals draws the inevitable conclusions. No sooner has he arranged some of the material in tabular form than he remarks, "Table 1 is an effort to make this situation more graphic. It shows clearly the relative richness of the culture of the agricultural peoples compared with that of the nomads."<sup>2</sup> Again he says, "Even with the slight collection of data given in Table 1, it is evident that the solution to the problem of connections and transmissions between southern Mexico and the United States must be sought among the agricultural peoples. It is also among the agricultural peoples that the greatest differentiation occurs."<sup>3</sup> Later he states, "It seems a fairly certain assumption that what transmissions occurred between the two regions rarely if ever went directly across the north Mexican desert plateau, but went around it."<sup>4</sup>

Few will deny the persuasiveness of the suggestions Dr Beals makes from the data in his possession. The whole question is whether or not those data are complete and trustworthy enough to permit of even a "fairly certain" assumption which rules out the northern plateau as a possible route of culture diffusion. Upon looking over the list of 116 traits in Dr Beals' Table 2, the writer was amazed to find that to the 31 traits recorded for all the tribes of nomads by all the authorities utilized by Dr Beals, he could add 30 more traits from his study of two of these tribes. The list of these additions is as follows:

Turquoise	Thatched Roofs	Rain Ceremonies
Sling	Gourd Utensils	Ceremonial Fire
Shield	Caps	Ceremonial Trees
Mask	Kicking Race	Incense
Captive Eagles	Painted Clothing	Palisades
Tattooing	Tambourine Type Drum	Spear-Thrower
Musical Bow	Infanticide	Pottery Drum
Patole (Dice or Stave Game)	Pinole (made of mesquite)	Maize Cultivation
Cairn or Wayside Shrine	Food Offerings	Squash Cultivation
Poisoned Arrows	Vision and Dream <sup>5</sup>	Irrigation

<sup>2</sup> Page 136

<sup>3</sup> *Ibid*

<sup>4</sup> Page 147

<sup>5</sup> Dr Beals indicated that the trait, though unrecorded, was probably present in the area. In that he is correct.

The first 20 of these traits should be added to Dr Beals' table without reservation. The validity of the last 10 is more doubtful, and it may be that some of them should be eliminated. In respect to the first 6 of the 10 doubtful traits it is not always clear that what the writer found among the Chiricahua or Mescalero corresponds exactly to the trait Dr Beals had in mind when he compiled his list.

There is the case of rain ceremonies. The Apaches certainly performed ceremonies to bring rain, but they were not often connected with crops. A storm was more likely to be desired for purification after widespread sickness, to retard an enemy, or to replenish the drinking water supply. Is this to be equated with the rain ceremonies of the far south and southwest?

What of the ceremonial fire? At the Apache four-day girl's puberty rite a fire is supposed to be kindled with a fire-drill by either the girl or the shaman who sings for her, and it must be kept burning throughout the ceremony. Does this suggest connection with the sacred fire of a temple?

Again, trees were a source of supernatural power to these Apaches. The particular tree from which or under which the individual gained his supernatural experience might be singled out and named in songs. Beliefs clustered about certain trees and bushes. Some were known to be "witched;" those who ate from their fruits would be poisoned. How close are these concepts to what Dr Beals has in mind?

Sages, other pungent smelling plants, and animal parts were thrown on hot coals to create smoke and odor. Any person who had had anything to do with the dead or with the death rites was obliged to purify himself and his possessions by bathing in that smoke. The practice was an integral part of many rites, and a specific for headache, nosebleed, and other ailments. Shall this be called use of incense?

The Apaches often surrounded their individual camps with a rude fence of cactus and utilized the sharp-pointed yucca plant for the purpose of enclosing and protecting the camp, too. To some this may not seem elaborate enough to be termed a palisade.

Many may be surprised to see the spear-thrower included in the list of additional traits. The writer hastens to add that the Apaches do not have the conventional spear-thrower. What the Mescalero do utilize is the principle of the spear-thrower. Men used to wear a rope tied to the right wrist and carried a short spear notched about midway along the shaft. To use the spear, the rope was slipped around this notch in such a manner that it became loosened as the spear was propelled forward. This device allowed the thrower to clasp the spear near the point, while the rope stretched backward and was attached to the spear at the notch to the rear. Thus when the spear was hurled the guidance of the rope gave the effect of artificially lengthening the arm. The writer has seen improvised spears thrown long distances with force and accuracy by this means. Now it is evident that while the Mescalero did not receive the spear-thrower in form, unless we are to accept this as an independent invention, we must agree that the idea and principle of the atlatl did penetrate to them.



The difficulty in placing these traits and deciding whether or not they belong on our list involves a critique of a statistical approach to anthropological data which assumes that a trait in one culture is the counterpart or equivalent of a formally similar or comparable trait in another culture. These difficulties are multiplied when the trait in question does not fall under the heading of material culture and is consequently not always easy to define in formal terms. But such a critique is too involved a subject with which to burden a brief statement and will not be attempted here.

In regard to the last four items listed there is no doubt of their existence before reservation days. The question is one of their age: did these agricultural traits and the pottery drum which is generally associated with them exist for these Apaches prior to 1750 as Dr Beals' paper would require if they are to be considered here?

It may seem strange to speak of the agricultural traits of nomads. Nevertheless, the manner in which the Apaches carried on the small amount of crop-raising in which they engaged offered little hindrance to their roving life. They planted seed in some sheltered spot, came back once or twice to pull the worst of the weeds, and returned again for the harvesting. If water was nearby and easy to divert into the fields, irrigation was practiced. Not all families raised crops, in fact very few did; but as far back as the memories of the old men now living can carry us, a little agriculture was carried on in this fashion. It may well be that agriculture was begun among these people after 1750. But there seems to be no way to decide the question with finality now, so the traits are listed with the above reservations. Incidentally, if proof should be forthcoming that the traits do antedate 1750, it would somewhat modify Dr Beals' picture of a non-agricultural northern province.

When the 30 traits listed above are added to the 31 to which Dr Beals found reference, the alleged meagerness of the nomad culture largely vanishes. The combined 61 traits of the nomads compare very favorably with the 55 positively recorded for the Jalisco-Tepic province, the 56 of Old Sinaloa, the 63 of Old Sonora, the 62 of southwestern United States, and the various totals mentioned for other provinces at the beginning of this article. One may eliminate a number of the doubtful cases and still have an impressive total left for the nomads.

There is no reason to think that the Apaches possessed a richer culture than the other nomads of northern Mexico. When the writer was compiling a library thesis concerning the Apaches some years ago he read through volume after volume of old source material which supposedly dealt with these nomads or included observations concerning them. He found few references to any of the traits which are given above. He did find that it was an almost invariable rule for travellers, clergymen, and officials to dismiss nomadic peoples with a few contemptuous and harsh epithets, and that governments were formerly more concerned with exterminating these wandering tribes than with preserving their lore. It is not too daring to guess that the lack of references to traits of the northern nomads which Dr Beals noted is not so much due to the absence of the traits as it is to this discrimination and selective process. To this must be added the inherent difficulty which those who are not professional ethnographers will have in discovering any but the most obvious traits among

nomadic peoples. If special regard is constantly paid by a settled people to a tree near a temple, it is not so difficult to ascertain and note this. It is another matter to obtain reliable data on the attitude of nomadic peoples in regard to trees.

Nor must it be assumed because these Apache tribes are believed to have come originally from the north, that the additional traits are some which show mostly northern affiliations and could not be duplicated for other northern Mexican nomads. Of the 30 additional traits which have been enumerated, 24 are also listed for southern Mexico, and Dr Beals indicates his belief that a twenty-fifth, the poisoned arrow, existed in southern Mexico too. Of the 24 additional traits which the nomads and southern Mexico have in common, 18 are also to be found in southwestern United States.

It may make things a bit more graphic if the writer uses Dr Beals' own method. Dr Beals arranged a table (Table 3) which he named "Cultural Connections." He reduced the number of basic traits under consideration from 116 to 104. He sought to show the number of these 104 traits recorded for each culture province and the number and percentage of its traits which each province shares with every other province.

Dr Beals found that 78 of his 104 traits had been recorded for southern Mexico. He found according to his sources that only 17 of these 78 traits of southern Mexico were also common to the northern nomads, or a scant 22 percent. But in examining these 78 southern Mexican traits we can add 22 more to the 17 which Dr Beals credited to the nomads, and the total is brought up to 39 traits which southern Mexico shares with the nomads, 50 percent of southern Mexico's 78 traits. Compare this to the 56 traits (71 percent) which southern Mexico shares with the Jalisco-Tepic area; the 43 (55 percent) with Tepic-Culiacan; the 38 (48 percent) with Old Sinaloa; the 40 (51 percent) with Old Sonora; the 42 (54 percent) with southwestern United States; the 55 (70 percent) with Southern Sierra; the 17 (22 percent) with Central-Agriculturists; the 25 (32 percent) with Northern Sierra; the 22 (28 percent) with Tamaulipas; the 33 traits (42 percent) which southern Mexico shares with southeastern United States; and it is apparent, so far as present evidence indicates, that the northern nomads or the northern plateau cannot be ruled out as a possible cultural avenue between southern Mexico and southwestern United States. The writer does not mean to insist now that the nomads must have been important in the transmission of cultural elements to the Southwest, but he does mean to imply that the statistical proof that the nomads could not have acted in such capacity is most inconclusive. A study of the Lipan Apache which the writer hopes to initiate soon should give us much more information on the point.

In conclusion it might be said that even a stronger case for the influence of southern factors upon the northern nomads could be built up with little effort. There are a number of traits suggestive of the south (such as shooting fish with barbed arrows and use of the enema tube) which the writer has recorded for the Apache tribes but which are not included in the list of traits used by Dr Beals for his calculations.

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# PETROGLYPHS SHOW THAT THE ANCIENTS OF THE SOUTHWEST WORE MASKS

An article by White in a recent number of the *AMERICAN ANTHROPOLOGIST* gives a brief discussion of "Masks in the Southwest,"<sup>1</sup> to which the following should be added.

Many pictographic groups in the Uintah Basin area, in northeastern Utah, show actors wearing animal-head masks, exhibiting the horns. A group on Ashley Creek, near Vernal, which we photographed as P87, is described by the writer as follows:<sup>2</sup>

The central upper figure of the lower group is that of a man wearing a bird for a headdress. The figure at the left of the picture is not masked and is probably the man of ceremonies. This man attracts notice in that he is wearing his hair in side bobs. One man of the group is wearing a buffalo skin with horns for a headdress. Another has a horned headdress which extends from his head on each side in comb fashion. There are three human beings that are masked to represent some beast or insect. The whole group probably is an animal dance scene much like the animal dance scenes that can be observed in the Southwest at the present time. And again it is quite possible that it is a death-dance scene involving images of the dead (the upper scene of this picture was of hand effigies), similar to those described by Kroeber<sup>3</sup> as held by the Maidu (pp. 431-432), the Kawaiisu (p. 604), the Tubatulabal (p. 609), and the Kitanemuk (p. 613), the circles (with eyes, of the upper group) being crude images of the dead people for whom the ceremony is being conducted.

Masked actors are shown in several pictographic groups in Nine Mile Canyon, east of Price, Utah. The men are usually shown as wearing horned headdresses, as described by the writer under pictographs N-P7, N-P43, and N-P47 and corresponding cuts in Figure 4.<sup>4</sup> These masked men in the Nine Mile Canyon pictographs are dubbed "Cornute Men" by Morss,<sup>5</sup> the writer's N-P7 masked man being figured by him in Plate 13, b, and described under his "Nine Mile Canyon Pictographs,"

<sup>1</sup> Vol. 36:626-28, 1934.

<sup>2</sup> Albert B. Reagan, *The Pictographs of Ashley and Dry Fork Valleys in Northeastern Utah* (Transactions, Kansas Academy of Sciences, Vol. 34), p. 199.

<sup>3</sup> *Handbook of the Indians of California* (Bulletin, Bureau of American Ethnology, 78, 1925).

<sup>4</sup> Albert B. Reagan, *Anciently Inhabited Caves of the Vernal (Utah) District, with some additional Notes on Nine Mile Canyon, Northeast Utah* (*Ibid.*, Vol. 36), pp. 62-63. The N-P7 group is also described in other reports of the writer on this region, as follows. *Some Archaeological Notes on Nine Mile Canyon, Utah* (*El Palacio*, Vol. 31, No. 4:45-71, Santa Fé, July 29, 1931) and in *Some Notes on the Picture Writing North of Mexico* (Bulletin, Wagner Free Institute of Science of Philadelphia, Vol. 7, 38-54, November, 1932) where a photograph of group N-P7 is the upper figure on page 42.

<sup>5</sup> Noel Morss, *The Ancient Culture of the Fremont River in Utah* (Papers, Peabody Museum of American Archaeology and Ethnology, Vol. 12, No. 3, 1931). Also see Julian H. Steward, *Petroglyphs of California and Adjoining States* (University of California Publications in American Archaeology and Ethnology, Vol. 24, No. 2, 1929), p. 217, for a discussion of katchina-like pictographs; Jesse Walter Fewkes, *Hopi Katchinas* (Twenty-first Annual Report, Bureau of American Ethnology, pp. 3-126, 1903) for types of Hopi katchinas.

pp. 40-41. He also found cornute pictographs in the Fremont River area in Utah and northward at many other places along Green River and its tributaries to the Nine Mile Canyon country (pp. 34-42, pls. 13-18).

It might be argued that these animal headdresses were not masks in the sense of this discussion, though there are glyphs which seem to show that the skin covered the face and that holes were cut through it for peep holes. But to look further, there are pictographic scenes which show unmistakable masks. Several groups which Morss examined at Fruita, Utah ("Dancer Pictographs," p. 34, pls. 15-16) showed unmistakable masks. Morss states (p. 35), "The face may be left blank or the feature roughly indicated or a mask may be shown:" and in his conclusion on the pictographs of the region he further states (p. 42), "The Fremont anthropomorphs seem to have been developed from Basket-maker prototypes and indicate the personification of supernatural beings in forms similar to those now familiar in the Southwest." Steward also mentions the glyphs of the Fruita-Thompson area in Utah (p. 152, pls. 81-82): "One type of the figures (found in the Fruita region . . .) is closely similar to the katchina dance costumes of the Pueblo Indians;" and the figures he exhibits on the two plates are all of the Fremont type of culture.

Concerning the age of these pictographs we note that the glyphs of P87 in Ashley Valley are probably of Basket Maker age, while those of Nine Mile Canyon which were examined by Morss and the writer are of the Fremont culture stage, as are also those from Fruita. These all have the characteristics of the Fremont culture. Morss holds this culture flourished in Pueblo II times, stating in his introduction "The presence of small amounts of black-on-white and corrugated pottery, with other evidence, showed that this complex [the Fremont culture stage] was contemporary with Pueblo II in other regions" (therefore not later than 900 A.D.). This conclusion was arrived at simultaneously by the writer on the basis of finds in the Uintah Basin (as given in the articles mentioned above). So the "rock writings" show beyond doubt that masks were used in Indian katchina ceremonies before the coming of the Spaniards.

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### THE BULBED ENEMA SYRINGE IN NORTH AMERICA

In Appendix 1 of *Comparative Ethnological Studies*, Volume 8 ("Modifications in Indian Culture through Inventions and Loans," 1930) the late E. Nordenskiöld brought together the relevant data and discussed the use of enema tubes and syringes among the American aborigines. He expressed the opinion that "the peculiar idea of using enematic injections has probably been independently invented both in the Old World and the New." Two types of devices for this purpose are distinguishable. One has a bulb and the other has not. "The bulbless type is known from N. W. North America to the Chocó in Colombia, and *the bulbed one from large tracts of South America*" (*italics ours*).

It is the purpose of this brief note to call attention to the use of the *bulbed* type of enema syringe by certain North American natives, a fact overlooked by Norden-skiöld, but one which nicely fits into the series of culture traits compiled by him which occur north of Mexico in North America and in South America but, in the intervening regions, sporadically, if at all.<sup>1</sup> The positive information which has come to my notice is confined to Algonkian and Siouan speaking people and, for these groups, only from a few localities and tribal units. Systematic inquiry in the field and information recorded in documentary sources may reveal a much wider distribution of the bulbed syringe.

A. Skinner reported the bulbed enema syringe for the Eastern Cree.<sup>2</sup> In the course of field work among the Norway House and Cross Lake Cree (Manitoba) in 1930, I found the same device in use. The bulb was made of a jackfish (*Esox lucius*) bladder and the tube of the leg bone of a bird (?).

Dr J. M. Cooper made inquiries regarding the enema syringe among the James Bay Cree, but failed to secure affirmative information. At Atawapiskat (100 miles north of Fort Albany), however, a type in which a small cotton bag is used was known. The native name for it was sisobátcigan, "thing or instrument for squirting water." Dr Cooper says that his informant was doubtful whether the device was of aboriginal or European origin.<sup>3</sup>

Among Saulteaux-Ojibwa groups the enema syringe seems to be a well-known device. A. Skinner,<sup>4</sup> F. Densmore,<sup>5</sup> and H. H. Smith<sup>6</sup> refer to it and Baraga records the term pindabawadjigan, "clyster-pipe," in his Dictionary.<sup>7</sup> This is the term by which the bulbed enema syringe is known among all the Saulteaux groups east of Lake Winnipeg which I have visited, and its cognate is employed by the Norway House and Cross Lake Cree. The Berens River Saulteaux, like the latter, use a jackfish bladder. The tube is made from the wing bone of the Canada goose and by means of a sinew wrapping is attached to the bladder. The same device is known to the Hollow Water River band to the south and to the Island Lake band to the north-east. H. H. Smith likewise refers to the use of the bulbed syringe among the Meskwaki (Fox).<sup>8</sup>

<sup>1</sup> Table I, Comparative Ethnological Studies, Vol. 9 (1931) and Appendix 1. The latter had previously appeared in Ymer (1926) under the title "En jámförelse mellan indiankulturen i södra Sydamerika och i Nordamerika."

<sup>2</sup> Notes on the Eastern Cree and Northern Saulteaux (Anthropological Papers, American Museum of Natural History, Vol. 9, 1911), p. 77. Skinner cites p. 190 in the 1795 edition of Hearne's "Journey" in connection with his own observation, but Hearne makes no reference to an *enema syringe*. What he does describe is the practice of *blowing* into the anus of a patient.

<sup>3</sup> Personal letter.

<sup>4</sup> *Op. cit.* p. 161

<sup>5</sup> Uses of Plants by the Chippewa Indians (Bulletin, Bureau of American Ethnology, 44, 1928), pp. 331-32.

<sup>6</sup> Ethnobotany of the Ojibwa Indians (Bulletin, Public Museum of Milwaukee, Vol. 4, 1932), p. 342

<sup>7</sup> R. P. Bishop Baraga, A Dictionary of the Ojibwe Language (Montreal, 1878), Part 1

<sup>8</sup> Ethnobotany of the Meskwaki (Bulletin, Public Museum of Milwaukee, Vol. 4, 1928), p. 219.

For Siouan speaking groups, M. R. Gilmore<sup>9</sup> has recorded the use of an infusion of the bark and root of the Kentucky coffee-tree ((*Gymnocladus dioica* [L] Koch) as a rectal injection for constipation among the Dakota, Omaha, Ponca, Winnebago, and Oto. "This remedy was used from time immemorial," he says. "Prior to contact with Europeans the Indians made their own syringes, an animal bladder being used for the bulb and a hollow cylindrical bone, as the leg bone of a prairie chicken, turkey, goose, or other bird, was used for the tube. The bulb was attached to the tube by sinew wrapping."

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<sup>9</sup> Uses of Plants by the Indians of the Missouri River Region (Thirty-third Annual Report, Bureau of American Ethnology, 1919, pp 43-154), p. 89.

## NOTES AND NEWS

### ANNUAL MEETING OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

The annual meeting of the association will be held at Phillips Academy, Andover, Massachusetts, on December 27-29, 1935 (Friday to Sunday, inclusive).

### MEXICAN LANGUAGES

The "Instituto Mexicano de Investigaciones Linguisticas" (Universidad Nacional de Mexico) "plans to publish successively, as its financial means may permit the most complete and recent dictionaries and grammars of the numerous indigenous languages still spoken in this country, as well as dialectical studies of the Spanish language, either reediting in critical editions our most representative works thereon, or reproducing the already existing monographs on regional language."

### GRANTS AND FELLOWSHIPS OF THE SOCIAL SCIENCE RESEARCH COUNCIL

It is announced that applications for grants-in-aid in support of research during the period between April 1, 1936 and April 1, 1937 must be filed before January 15, 1936, on forms to be secured from the Secretary for Grants-in-Aid, Social Science Research Council, 230 Park Avenue, New York City.

Among the awards for 1935-1936 are: Otto Klineberg for a study of emotional expression among the Chinese; Ronald L. Olson for a study of cultural changes involved in the shift from paternal to maternal descent among the Haisla and Heiltsuk of British Columbia; Julian H. Steward for a study of the ecological aspects of Shoshoni society.

Applications for Post-Doctoral Research Training Fellowships and Pre-Doctoral Field Fellowships are to be submitted to the Fellowship Secretary by December 1, 1935, and for Pre-Doctoral Fellowships for Graduate Study by March 15, 1936.

A NEW SERIES, "GENERAL SERIES IN ANTHROPOLOGY," has been issued, offering an inexpensive vehicle for the publication of monographic material. There have appeared No. 1—The Prophet Dance of the Northwest and its Derivatives: the Source of the Ghost Dance, by Leslie Spier, and No. 2—Taos Pueblo, by Elsie Clews Parsons. (Leslie Spier, editor: George Banta Publishing Company, Menasha, Wis., agent.)

EDWARD H. THOMPSON, famous for his long residence in Yucatan and his pioneer archaeological explorations there, died in Plainfield, New Jersey, on May 18, 1935 at the age of seventy-five. He was the friend of all the early investigators in Yucatan and his ownership of Chichen Itza made him a well known figure to all Maya students. He was American Consul in Yucatan for almost twenty-five years, combining

these services with archaeological investigations. His principal work was at Chichen Itza where he discovered the "High Priest's Grave," the famous tablet of the Initial Series, and uncovered the "Temple of the Painted Columns." He discovered several new sites in the interior of Yucatan and knew much of the folklore of the present natives.

Collections gathered by him are in the Field Museum in Chicago and in the Peabody Museum, Cambridge. The remarkable collection of objects dredged from the Cenote of Sacrifice at Chichen, now in the Peabody Museum, will always remain a monument to his indefatigable zeal and to his desire to preserve for all time something of the life of the Mayas whom he loved.

A. M. T.



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